The effect of school finance centralization on school revenue and spending: Evidence from a reform in Michigan

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Abstract

In 1995, the Michigan state government centralized a school finance system by removing local discretion on school revenue. The theoretical framework says that centralization has the price and income effects on the pivotal voter's demand for school spending, probably reducing overall spending levels. Using the district-level panel data on school finance in Michigan and neighboring states for the period of fiscal year 1990-2004, this paper provides evidence for the effect of the Michigan school finance reform on the level of school revenue and spending. The study finds that the reform decreases the level of school revenue and spending with a time lag at the state level. The results also suggest that the reform equalizes revenue and spending among school districts at the expense of high-revenue district's resources without a large increase in low-revenue district's resources.

Keywords: Centralization of school finance, Michigan school finance reform, School revenue and spending
JEL classification: H77, I22, I28

I. Introduction

In the United States, public education has traditionally been financed through local property taxes, which makes school spending strongly correlated with local property wealth. In economics, this local financing system is justified by the efficiency argument for close tax-benefit linkage in the context of the Tiebout model. The wide spending disparity between high- and low-income school districts, however, has been a great concern in this finance system. Since the 1970s, many lawsuits and legislative actions have occurred across the country to reduce spending inequalities, bringing significant changes to the existing school finance system in many states. The Michigan school finance reform was one of these changes in the school finance system. It substantially centralized school revenue sources so that the state government had more money to equally distribute across districts.

The Michigan school finance reform is distinguished from most other state-level school finance reforms in an important way. Most reforms simply changed grant formulas in favor of low-income districts, leaving districts' discretion on revenue relatively intact. Thus, districts have largely funded revenue from their own sources even after reforms. On the other hand, the Michigan school finance reform sharply reduced local property taxes and introduced foundation grants that account for a large share of districts' revenue, so that districts have become highly dependent on state funding with limited discretion. Thus, the Michigan school finance reform can be categorized as the centralization of school finance with limited local supplementation (Loeb, 2001). In my paper, I use the term "centralization" to mean the transfer of local discretion on school revenue to the state government with some equalization plans.

The Michigan school finance reform may achieve spending equality through centralization, but there may be consequences for school spending levels. Fischel (1986 and 1996) argues that centralization is unpopular in high-income districts, so that they may not be financially supportive

of such a system which redistributes resources from high-income districts to low-income districts. It can be re-expressed that centralization increases high-income districts' tax price (defined as the amount of local tax for one additional dollar of school spending).[1] Silva and Sonstelie (1995) incorporate this argument through their analysis of the price and income effects of centralization on spending levels. In their model, there exists the voter who is pivotal in determining the spending level, and the price and income effects depend on the pivotal voter's tax price and income. With the assumption about who the pivotal voter is and a tax price schedule, they suggest that the income effect is negative and the price effect is positive, but the overall effect of centralization on the spending level is empirically negative in California.

The potential effects of centralization on school spending levels have drawn attention from several researchers. Manwaring and Sheffrin (1997) estimate the effect of state-level school finance reforms on the desired level of school spending across the country by entering interaction terms between a reform dummy and several control variables in their regression. By these interaction terms, the estimated effects can differ over time and across states. The results suggest that desired school spending levels fell in California, where the reform centralized school finance, but rose in other states where the reforms did not centralize school finance as much. Downes and Shah (2006) also enter interaction terms between a reform dummy and other control variables to estimate the effect of the reform on the level of school spending across the country. They find that court-ordered reforms reduce the spending level, but legislative reforms increase the spending level. The results may imply that constraints on local discretion on school spending have a negative effect on the mean spending level, as court-ordered reforms usually impose tighter constraints on local discretion. Hoxby (2001) focuses on tax price and its effect on mean school spending. She

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[1] The increase in tax price means that households should pay higher additional taxes for a marginal increase in per-pupil spending.

finds that, at the district level across the country, a higher tax price leads to lower school spending per pupil, implying that less local discretion would result in lower spending.

In addition, there exists a voluminous empirical literature on the effect of state-level school finance reforms on school spending across the country. Papers generally find that school finance reforms reduce school spending inequalities among districts (e.g., Card and Payne, 2002; Murray, Evans, and Schwab, 1998) and increase mean per-pupil spending (e.g., Jackson, Johnson, and Persico, 2016; Lafortune, Rothstein, and Schanzenbach, 2016; Sims, 2011a; Sims, 2011b). Many of the school finance reforms did not entail the significant centralization of school finance, however, so that the estimated effects do not provide clear implication with respect to centralization; most school finance reforms modified grant formulas to equalize school spending among districts without substantially transferring discretion on school spending from local governments to the state government. Based on calculated tax prices in Hoxby (2001), it seems that only two school finance reforms (California and New Mexico) could be classified as centralization out of 34 reforms that occurred during the period of 1970-1990.

In this present paper, I estimate the effect of the Michigan school finance reform on the level of school revenue and spending, motivated by the fact that the reform can be categorized as the centralization of school finance. The paper employs the difference-in-differences event study method that dynamically compares districts between Michigan and neighboring states (Illinois and Indiana), considering the neighboring states as the control group. School districts are grouped together by the pre-reform level of school revenue, and the heterogeneous effect of the reform is examined across groups. The study finds that the reform increases the level of school revenue and spending at the state level only in the first two year of the reform; the reform eventually decreases it two years after and onwards. The results also suggest that the reform equalizes revenue and

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spending among districts at the expense of high-revenue district's resources without a large increase in low-revenue district's resources.

The contribution of this paper is as follows. First, the consequences of school finance centralization for the level of revenue and spending have not been fully studied in the literature. Although there are multiple national-level studies on the effect of school finance reforms on spending levels across the country (e.g., Lafortune, Rothstein, and Schanzenbach, 2016; Downes and Shah, 2006; Card and Payne, 2002; Manwaring and Sheffrin, 1997), reforms differ considerably from each other across states and often bring little centralization to the existing school finance systems. Therefore, except for Hoxby (2001), there is difficulty in directly linking their results to the consequences of centralization, even though some of the studies attempt to draw out implication with respect to centralization. The results of the national-level studies might not have straightforward interpretations as dissimilar state-level reforms are grouped together into the same category. Second, the existing literature has not fully explored the consequences of the Michigan school finance reform for the level of revenue and spending. Although multiple papers have studied the Michigan school finance reform, they focus on revenue/spending equalization among districts, resource allocation among spending types, or educational attainment (Chakrabarti and Roy, 2017 and 2015; Hyman, 2017; Roy, 2011; Papke, 2005; Zimmer and Jones, 2005). Therefore, questions about the effects of the reform on the level of school revenue and spending are little answered in these papers. Chaudhary (2009) estimates the effect of the Michigan school finance reform on log per-pupil spending, but the effect on the spending level is not the main focus of her study. Furthermore, my paper improves her empirical model by accounting for the dynamic

response of the outcome variable.[2] I believe that the failure to account for this issue may lead to biased estimates, which will be discussed further in Section V.

The remainder of the paper is organized as follows. In Section II, the Michigan school finance system for the pre- and post-reform periods is introduced. Section III briefly presents the theoretical framework for the effect of centralization on the spending level. In Sections IV and V, the data set and empirical strategy are explained. Section VI presents the estimation results, and Section VII concludes.

II. School Finance in Michigan

Before the reform in fiscal year (FY) 1995, Michigan had the power equalization system. Under the system, the local property tax base below the state minimum tax base was subsidized by the state government.[3] This system was intended to equalize school revenue by guaranteeing that lower-income districts had the same power to raise revenue as higher-income districts. Before the reform in Michigan, districts below the 20th percentile of school revenue had funded approximately 60% of their revenue alone, and districts above the 80th percentile had funded 80% alone (as shown in figure 1). The Michigan program's minimum tax base was fairly low, so that few districts benefited from the power equalization program. In FY 1994, 39% of districts received the positive power equalization grant (excluding a flat grant) from the state government (Courant and Loeb, 1997).[4]

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[2] The dynamic response may have occurred because 1) constraints on local discretion became increasingly tighter over time after the reform, 2) reform's equalization plan was phased in over time, and 3) the reform may have lagged effects.
[3] The local property tax base is called the State Equalized Value, which was approximately one-half of market value in Michigan.
[4] For districts that had tax bases above the minimum tax base, their flat grants were reduced by the amount in excess of the minimum tax base times property tax rates; however, no district could receive a negative power equalization grant (including a flat grant).

Substantial inequalities in school spending and widespread calls for a larger role of the state government in public education were important factors leading to Michigan's reform in 1994. The reform was also largely motivated by the heavy property tax burden. Before the reform, Michigan's residents had a property tax burden approximately 33% higher than the national average, but a sales tax burden approximately 32% lower than the national average (Office of Revenue and Tax Analysis, 2002). There had been a strong demand to adjust this imbalance between property and sales taxes and provide relief for property taxes in Michigan. This resulted in a referendum on school finance reform, which was passed 69% to 31% in FY 1994 (the new program became effective in FY 1995).

The reform immediately eliminated a school operating tax on owner-occupied housing, and the school operating tax rate on nonhomestead properties (such as rental homes) was required to drop to 18 mills if the tax rate was above 18 mills. Furthermore, a cap on the increase in property taxable values was also introduced so that the rate of growth in taxable property values could not exceed both 5% and the inflation rate each year. As a result, each district government had increasingly less discretion on how much additional local revenue it could raise over time so that governments became more reliant on state grants for school revenue. To make up for the resulting revenue loss, the reform increased a state general sales tax from 4% to 6% and newly introduced statewide property taxes of 6 mills.[5]

Under the new school finance system, some high-income school districts were qualified for levying a hold-harmless tax on properties to ensure that post-reform school revenue was not smaller than pre-reform school revenue. In practice, however, the hold-harmless tax was not often imposed. Thus, we can say that local-source revenue for school operation was effectively capped

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[5] The reform also increased an excise tax on cigarette by 50 cents per pack and newly introduced a tobacco tax of 16%. The reform reduced the state income tax by 0.2%.

by the 18 mill school operating tax on nonhomestead properties. Even after the reform, however, districts retained full discretion on property taxes for school capital spending.

The reform introduced the foundation aid program that granted the minimum per-pupil school revenue (called a foundation allowance) to each district. The level of the foundation allowance initially depended on district's per-pupil revenue in FY 1994 so that higher-revenue districts initially received larger foundation allowances. The foundation aid program gradually reduced the initial gap in foundation allowances by increasing smaller foundation allowances faster than larger foundation allowances over time. The gradual equalization of foundation allowances is reported in the existing literature (e.g., Chakrabarti and Roy, 2015; Roy, 2011).

In short, the Michigan school finance reform imposed constraints on local-source revenue, increased state-level taxes, and equally distributed funding to each district. Following Loeb (2001), I categorize the reform as school finance centralization with limited local supplementation. Centralization may be characterized by two factors: 1) transfer of discretion on school revenue from the district governments to the state government and 2) revenue equalization among districts. The transfer of discretion gives the state government more money for grants to districts, which should naturally entail the state's revenue equalization program (such as a foundation aid program). Revenue equalization may not be the same as centralization, as equalization programs could be implemented with little transfer of local discretion[6]; however, it is difficult to imagine the transfer of discretion without some equalization programs.

Table 1 presents school revenue from federal, state, and local sources in the pre- and post-reform years. Column (3) shows that per-pupil revenue from state sources increased by approximately $3,200 in the first year of the reform. The increase in state-source revenue seems

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[6] For example, the state government can change the existing grant formula for school districts without the transfer of discretion on school revenue.

to be mostly due to an increase in formula grants (e.g., foundation grants and flat grants). We can also see that the increase in state-source revenue was offset by a drop in local-source revenue by approximately $3,100 per pupil. The drop in local-source revenue was mainly attributed to a drop in property tax revenue.

Figure 1 shows trends in mean percentages of revenue from local sources. The bottom (top) revenue group is defined as districts with per-pupil revenue below the 20th (above the 80th) percentile of the per-pupil revenue within each state in FY 1994. We can see that the percentage of local-source revenue dropped dramatically by approximately 40% in both bottom- and top-revenue groups in Michigan as a result of the reform. This indicates that districts became heavily dependent on state's grant in Michigan as switching from the power equalization program to the foundation aid program. On the other hand, there were no discontinuous changes in neighboring states' trends for our study period. This is because there were no major changes made to the school finance systems in neighboring states for the study period, supporting the use of neighboring states as the control group for the empirical analysis.

Figure 2 presents trends in mean per-pupil revenue and current spending of the bottom- and top-revenue groups. When compared to neighboring states' trends, it seems that per-pupil revenue and spending grew faster in Michigan for a per-reform period. This paper seeks to analyze trends in Michigan's school revenue and spending, compared to the counterfactual trend in which the reform would have not happened. Thus, an important issue with the empirical identification will be how to capture the different preexisting among states, which will be addressed in Section V.

III. Theoretical Framework

This section briefly describes the theoretical framework for the effect of centralization on the spending level by analyzing the cases of the decentralized and centralized systems.

The effect of centralization on the level of spending can be explained by price and income effects. It is assumed that the voter with *median* income is pivotal in determining the spending level under the purely centralized school finance system, whereas the voter with *mean* income is pivotal in determining the (mean) spending level under the purely decentralized school finance system (Silva and Sonstelie, 1995; Fernandez and Rogerson, 1999).[7] Because median income is generally lower than mean income, we can say that the income effect of centralization is negative. The price effect depends on how the pivotal voter's tax price changes as school finance is centralized. Tax price may be close to one for every voter under the purely decentralized system, whereas it may increase with voter's income under the purely centralized system due to equalization plans. Silva and Sonstelie (1995) suggest that the pivotal voter's tax price would drop as a state switches from the decentralized system to the centralized system if the tax structure of the centralized system is sufficiently progressive; in this case, the price effect is positive.

Figure 3 shows the relationship between a voter's demand for school spending per pupil and voter's income. A voter has the demand for school spending per pupil given by $D(P,I)$, where P is voter's tax price and I is voter's income $\left(\frac{\partial D(P,I)}{\partial P} < 0 \; and \; \frac{\partial D(P,I)}{\partial I} > 0\right)$. I assume that $P = 1$ for every voter under the purely decentralized system and that tax price is an increasing function of income $\left(\frac{dP(I)}{dI} > 0\right)$ under the purely centralized system. Then, as seen in the figure, $D_{decentl}$ is increasing faster with respect to income than D_{centl} is, where $D_{decentl}$ and D_{centl} are a demand function for school spending per pupil under the decentralized and centralized systems,

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[7] Assuming that the demand for school spending is a linear function of income and that families sort into communities according to income under the decentralized school finance system, mean school spending should be equal to the demand of the mean income voter.

respectively. With the assumption of the pivotal voter under the decentralized and centralized systems as mentioned above, mean per-pupil spending is $D^*_{decentl}$ under the decentralized system and is D^*_{centl} under the centralized system, where I_m is mean income and I_{md} is median income. A movement along demand curve $D_{decentl}$ from point A to point C represents the income effect of centralization, and a movement from point C to point B represents the price effect; the income effect is negative, and the price effect is positive. We can see that $D^*_{centl} < D^*_{decentl}$, meaning that centralization decreases mean per-pupil spending.

Let us consider the case of the Michigan reformed system, which is a centralized system with capped local supplementation. Voters first choose the desired level of local supplementation with given state-source revenue and then choose the desired level of state-source revenue in order to maximize their utility (Loeb, 2001). In this framework, low-income voters whose tax price for local supplementation is higher than tax price for state-source revenue choose no local supplementation, given the optimal level of state-source revenue; high-income voters whose tax price for local supplementation is lower than tax price for state-source revenue choose positive local supplementation, given the optimal level of state-source revenue. As a result, school spending would be supplemented by own revenue for high-income voters with income above I_0, where I_0 indicates the level of income above which tax price becomes greater than one.

The reform increased the sales tax instead of the reduction in the property tax, which made the tax structure less progressive. As a result, the tax price can be increasing in income more slowly and also can be more concave. In this case, I_0 might be smaller, and the demand function might be steeper under the centralized system, which is represented by I'_0 and D'_{centl} in the figure; I assume that I'_0 is located on the left-hand side of I_{md}. We can see that mean per-pupil spending is smaller than D^*_{centl}; both income and price effects are negative in this case.

III. Data

My empirical analysis is based on panel data for the period from FY 1990-2004 in Michigan and neighboring states (Illinois and Indiana). This data set has multiple sources. Data on district-level revenues, expenditures, and the number of pupils are obtained from the Public Elementary-Secondary Education Finance Data of the U.S. Census Bureau for the period from FY 1992-2004. Because the Government Census does not provide data before 1992, I add school finance data for the period from FY 1990-1991 from the Common Core of Data (CCD).[8] Both the Government Census and the CCD data set are based on common school finance data that are submitted by each state education agency, so that there is little threat to the consistency of the panel data between the two different sources.

I complete the construction of my panel data set by including data on the state-level unemployment rate and log personal income that are obtained from the Bureau of Labor Statistics and the Bureau of Economic Analysis, respectively. These economic variables are reported in terms of calendar years and are 6-month forward in terms of fiscal years.

The description of variables is presented in Table 2. Instructional spending includes instructor salaries as well as costs of class supplies and materials. Supportive service spending includes health and psychological services costs, administrative costs, cost of school fiscal services, costs of operation and plant maintenance, and student transportation costs. Instructional and supportive service spending are exclusive each other.

IV. Empirical Strategy

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[8] School finance data in 1990 come from the Local Education Agency Finance Survey of the CCD, and the data in 1991 come from the Longitudinal Fiscal-Nonfiscal Detail File of the CCD.

Considering districts in Michigan as the treatment group and districts in Illinois and Indiana as the control group, I estimate the effect of the reform by employing the difference-in-differences (DD) event study framework. I choose these neighboring states as the control group for two reasons. First, there was no major change in the school finance system in these states for the study period. Although Illinois and Indiana had minor school finance reforms or a change in formulas for state foundation grants to districts for the study period, these reforms were on a much smaller scale compared to the Michigan school finance reform[9]. These reforms are grouped together and controlled in my regression, and the effect of the Michigan school finance reform can be identified as my treatment reform did not coincide with these reforms. Second, Illinois and Indiana are geographically close to Michigan, so that socioeconomic factors might not evolve very differently among them. Ohio borders Michigan, but I do not choose Ohio as the control group since it successively had court-ordered school finance reforms on a large scale in 1997, 2000, and 2002 (Jackson et al., 2016; Lafortune et al., 2016).

To control for potentially different pre-existing trends between Michigan and neighboring states, I include state-group specific time trends in the DD event study regression as follows:

$$Y_{sgdt} = \alpha + \sum_{k=0}^{6+} \beta_k Reform_{st}^k + \sum_{g=1}^{5} (X_{st}' * D_g)\gamma_g + \sum_{g=1}^{5} \delta_{sg}(t_s * D_g)$$

$$+ \text{District-effects}_{sgd} + \text{Year-effects}_t + \varepsilon_{sgdt}$$

(1)

Y_{sgdt} is an outcome variable (e.g., per-pupil revenue) in state s, group g, district d, and fiscal year t. I divide districts into five revenue groups based on per-pupil revenue in FY 1994 (one year

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[9] By PA 90-548, Illinois increased the foundation level from $3,108 to $4,225 in FY1999. Indiana rewrote its foundation grant formula to adjust school revenue more equal among districts in 1993. These legislative changes might increase foundation grants to low-revenue districts, but noticeable changes are not observed in Figure 1.

before the reform); group 1 is districts with per-pupil revenue below the 20$^{\text{th}}$ percentile of per-pupil revenue within each state, group 2 is districts with per-pupil revenue between the 20$^{\text{th}}$ and 40$^{\text{th}}$ percentile of per-pupil revenue within each state, and so on. Revenue groups are time-invariant. D_g is a dummy for revenue groups with $g \in \{1, 2, 3, 4, 5\}$. For example, $D_{g=5}$ is one if a district belongs to group 5 and zero otherwise.

$Reform_{st}^k$ is an independent variable of interest, which is a dummy for indicating the effective year of the Michigan school finance reform. Superscript k indicates k years since the effective year of the reform. Thus, $Reform_{st}^0$ is one for districts in Michigan in FY 1995 and zero otherwise; $Reform_{st}^k$ is a k-year lagged variable for $Reform_{st}^0$. By including the set of $Reform_{st}^k$ for $k = 1, \ldots, 6+$, I will estimate the dynamic effects of the reform. $District\text{-}effects_{sgd}$ is district-fixed effects, and $Year\text{-}effects_t$ is year effects.

To control for different economic trends between Michigan and neighboring states, I include the state-level unemployment rate and state-level log personal income in X_{st}. By interacting these economic controls with D_g, I account for the possibility that state-level economic conditions affect each revenue group differently. X_{st} also includes a dummy for the effective year of minor school finance reforms in neighboring states as well as its lagged variables. A key strategy to control for different preexisting trends between Michigan and neighboring states is including state-group specific linear time trends $\delta_{sg}(t_s * D_g)$. It might capture unobserved confounders evolving differently among states as well as revenue groups over time, which still remains uncaptured by state-level economic controls interacted with D_g.

Considering that the reform was intended to equalize school spending among districts by increasing lower-revenue district's foundation allowance faster, the interesting question is whether the reform achieved this goal. Thus, this paper investigates how the effect of the reform on the

level of revenue and spending differs among revenue groups by estimating the following regression:

$$
Y_{sgdt} = \alpha + \sum_{k=0}^{6+} \sum_{g=1}^{5} \beta_{k_g} \left(Reform_{st}^k * D_g \right) + \sum_{g=1}^{5} \left(X_{st}' * D_g \right) \gamma_g
$$

$$
+ \sum_{g=1}^{5} \delta_{sg} \left(t_s * D_g \right) + District\text{-}effects_{sgd} + Year\text{-}effects_t \qquad (2)
$$

$$
+ \varepsilon_{sgdt}
$$

In the above regression, I interact $Reform_{st}^k$ with D_g with $g \in \{1, 2, 3, 4, 5\}$, which is analogous to the triple differences estimation. By doing so, the coefficients on $\left(Reform_{st}^k * D_g \right)$ produce the effect of the reform by revenue group. Since the empirical model still employs DD event study framework, the above regression produces the dynamic effect of the reform by revenue group. In this paper, I report the estimated effect for each revenue group as the average of β_{k_g} over k: $\beta_g = \frac{1}{7} \sum_{k=0}^{6+} \beta_{k_g}$.

My DD event study method outlined here is differentiated from the standard DD method (using a single treatment dummy) in that the DD event study method estimates the dynamic response of outcome variables. My method is more suited than the standard DD method for this research because 1) the reform increasingly tightened constraints on local discretion over time by limiting the rate of growth in taxable property values, 2) the reform was intended to gradually equalize school spending over time rather than suddenly cutting grants to high-income districts and increasing grants to low-income districts all at once, and 3) the reform could have lagged effects over time. When the magnitude of the treatment effect changes over time, state-specific time trends are likely to capture the dynamic effects of treatment that are not captured by single treatment

dummy in the standard DD model, so that DD estimators can be biased (Wolfers, 2006). A simple remedy for this problem is using the DD event study method and then estimating the dynamic effect of treatment. Since my paper uses district-level panel data, standard errors are clustered at the district level.

V. Results

Figure 4 presents the estimated effect of the reform on revenue sources. The specification in this figure is equation (1). Figure 4 (a) shows that the reform decreases local-source revenue per pupil by about $2,600 in the effective year of the reform and that the effect has a downward trend over time. This dynamic effect may occur partly because constraints on local discretion were increasingly tightened over time due to a limitation on the growth in taxable property values.

In figure 4 (b), we can also see that the reform increases state-source revenue per pupil by about $3,000 in the effective year, and the effect is relatively stable in magnitude over time. The dynamic effect on state-source revenue may be affected by at least two factors. First, increasingly tightened constraints on local discretion may continuously increase the demand for state funding over time. Second, the gradual equalization of school revenue might make high-income (low-income) districts' tax price for state funding gradually increase (decrease) over time; as a result, high-income (low-income) districts' demand for state funding might gradually decrease (increase) over time. The overall dynamic effect of the reform on state-source revenue is ambiguous in theory, and the results show the stable response of revenue from state sources over time.

Table 3 reports estimated effect of the reform on per-pupil revenue. employing the model in equation (1). The specifications are with and without economic controls and state-group time trends. Regardless of the specifications in column (1) through (3), the reform increases per-pupil

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revenue only in the first two year of the reform and then significantly decreases it after that. The estimated effect has a downward trend and tends to be a bit suppressed four years after and onwards. In column (3) with economic controls and time trends, the average of estimated effects over a post-reform period (0 - 6 years+) is about -\$190 but not significant (reported in the second row to last). As a robustness check, I estimate the effect of the reform for a pre-reform period in column (4) through (6) to test pre-existing trends. Due to a short pre-reform period in my data, however, I only include an one-year leading dummy for the reform; as more leading dummies are included, fewer pre-reform years are left for economic controls and time trends to identify pre-existing trends. In column (4), the estimate for the one-year leading dummy appears to be positive and significant. After including economic controls as well as time trends in column (5) and (6), however, I am able to significantly suppress the estimated effect one year before the reform.
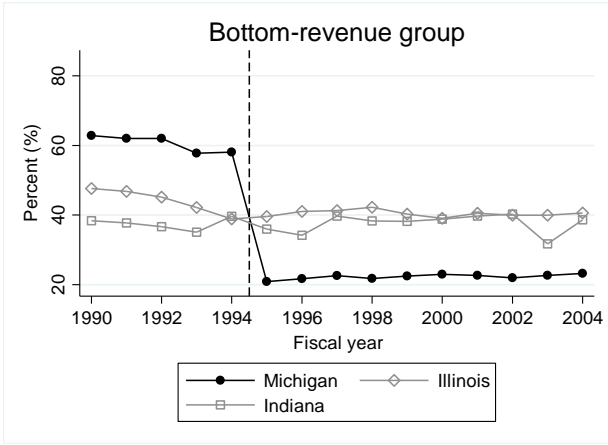
Using the standard DD method (using a single treatment dummy) instead of the DD event study method, state time trends can incorrectly capture post-reform downward trends in revenue in our case. Therefore, the standard DD estimator would only capture the remaining trend after treatment group's trend is incorrectly adjusted upward by confounded state time trends, biasing the DD estimator toward a positive value. Using districts in Illinois as the control group, Chaudhary (2009) employs the standard DD method with state-specific time trends and concludes that Michigan's reform increases log per-pupil expenditure, but her results may not be free from the confounded state time trends. To check whether the use of the standard DD estimator significantly changes my results, I re-estimate Table 3 by including a single reform dummy instead of reform dummies 0 – 6 years+ and then find very different results (reported in the last row); the estimated effects are now positive and significant. The results support the importance of using the DD event study method when the outcome variable has a dynamic response.

Table 4 presents the estimated effect of the reform on per-pupil instructional spending in column (1) through (3) and per-pupil supportive service spending in column (4) through (6) respectively. In column (1), the reform appears to increase per-pupil instructional spending for a post-reform period. After including economic controls and time trends in column (2), however, we can see that the reform increases per-pupil instructional spending only in the first two year of the reform; the estimated effect becomes negative two years after and onwards. In column (4), the reform appears to increase per-pupil supportive service spending in the first two year of the reform, but these positive effects are suppressed after including economic trends and time trends in column (5). In column (3) and (6), I find that the reform has little effect on both instructional and supportive service spending per pupil one year before the reform. Using a single reform dummy instead of reform dummies 0 – 6 years+, the estimated effects are positive in the last row. Again, it supports the importance of accounting for a dynamic response of the outcome variable in our case.
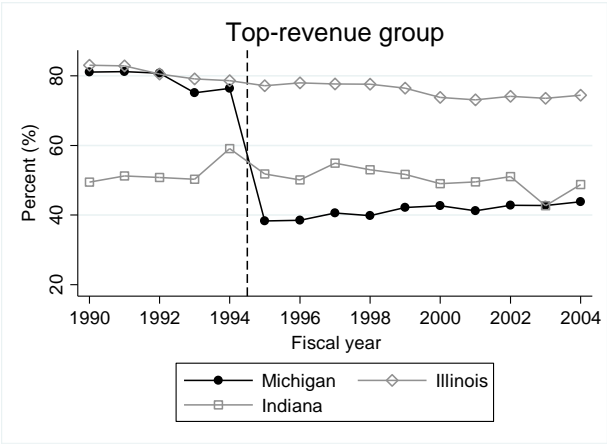
Table 5 shows the estimated effect of the reform on per-pupil revenue and spending by revenue group, using the model in equation (2). In this table, the average of the estimated effects for a post-reform period (0 – 6 years+) is reported for each revenue group. Group 1 is the bottom revenue group, and group 5 is the top revenue group. The results show that the reform equalizes revenue and spending among revenue groups. In column (2), the reform increases per-pupil revenue in group 1 by about $500 but decreases it in group 5 by about $1,400. In column (4) and (6), I find that the reform equalizes per-pupil spending among groups without increases in the bottom group. The reform even decreases per-pupil instructional spending in low-revenue groups as seen in column (4). It seems that the reform equalizes revenue and spending among districts at the expense of high-revenue district's resources without a large increase in low-revenue district's resources.

VI. Concluding remarks

This paper estimates the effect of Michigan school finance reform on the level of school revenue and spending, considering that the reform can be categorized as the centralization of school finance. I find that the reform decreases the level of revenue and spending with a time lag at the state level. The reform equalizes revenue and spending among districts, but the evidence for whether low-revenue districts have more resources than before is limited. On the other hand, high-revenue districts lose the substantial amount of revenue and spending due to the reform. The results imply potential tradeoff between the level of revenue/spending and revenue/spending equality among districts.

Fig. 1: Trends in the percent of school revenue from local sources by revenue group

Neighboring states include Illinois and Indiana. Each point is the state-group mean of the district-level percentage of local-source revenue.
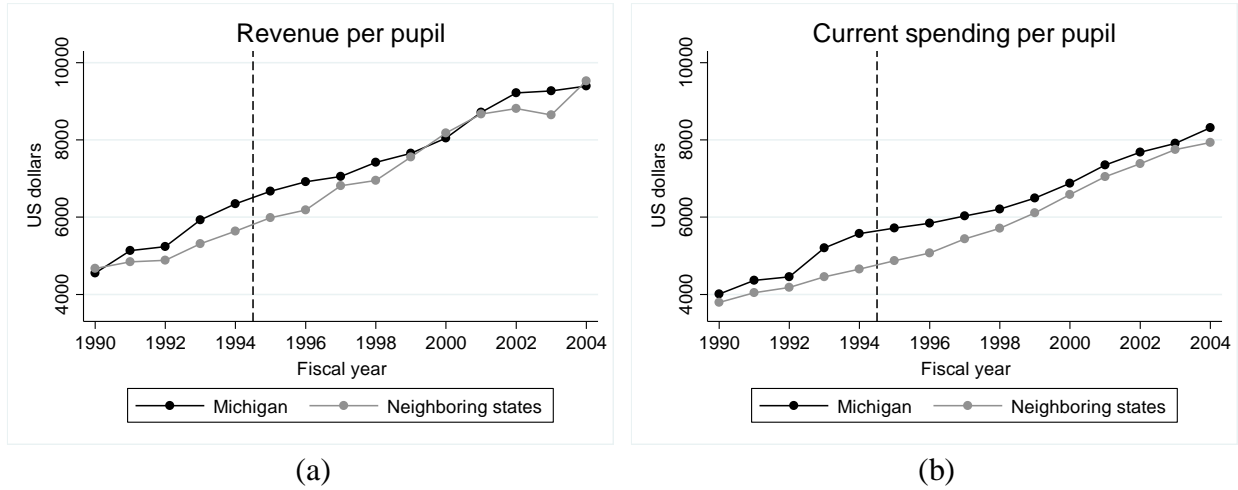
Fig. 2: Trends in per-pupil school revenue and current spending

Neighboring states include Illinois and Indiana. Each point is the state mean of district-level per-pupil revenue.
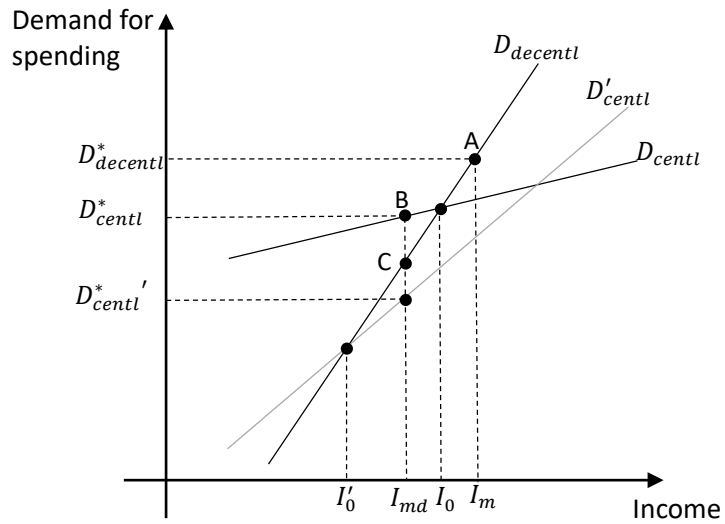

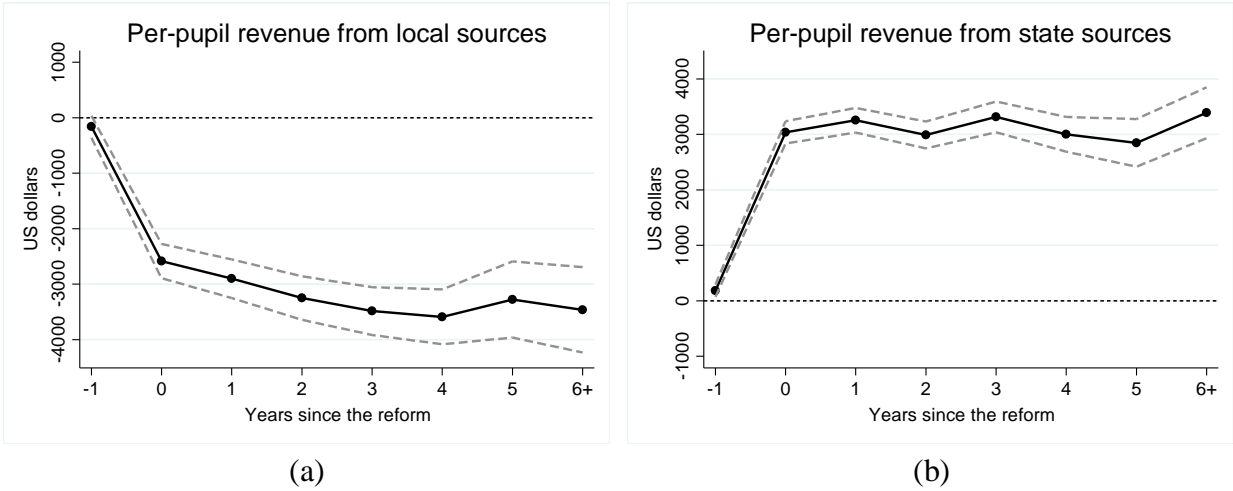Fig. 3: Demand for spending under the decentralized and centralized systems

Fig. 4: Estimated effect of the reform on revenue sources

The DD event study method with state-group time trends is employed to estimate. Black solid lines show the estimated effect of the reform, and gray dash lines indicate its 95% confidence intervals.

Table 1: Sources of school revenue in Michigan

	Mean of variables (Per-pupil US dollar)		
	Pre-reform (FY 1994) (1)	Post-reform (FY 1995) (2)	Differences ((2)-(1)) (3)
Federal sources	332.123 [11.835]	357.431 [12.252]	25.307 (17.035)
State sources	2,303.895 [59.220]	5,535.799 [50.959]	3,231.903 (78.127)
Formula grants	1,478.929 [53.552]	5,165.475 [45.700]	3,686.547 (70.401)
Other state sources	824.966 [21.480]	370.323 [19.702]	-454.643 (29.146)
Local sources	5,429.296 [125.917]	2,294.871 [70.280]	-3,134.425 (144.202)
Property taxes	4,787.414 [115.803]	1,589.379 [63.483]	-3,198.035 (81.671)
Other local sources	641.882 [32.480]	705.492 [21.684]	63.610 (39.053)
Sum	8,065.31 [105.689]	8,188.10 [74.627]	122.79 (129.381)

Values are adjusted to 2004 prices by using the CPI. Standard deviations are in brackets, and standard errors are in parentheses.

Table 2: Description of variables (Obs.= 25,505)

Variable	Mean [S.D.]
Per-pupil school revenue from local sources ($)	3,425.647 [2,432.825]
Per-pupil school revenue from state sources ($)	3,202.249 [1,725.423]
Per-pupil school revenue ($)	6,928.563 [2,503.860]
Per-pupil instructional spending ($)	3,326.118 [1,158.751]
Per-pupil spending for supportive services ($)	2,098.060 [868.071]
Indicator for the effective year of the Michigan school finance reform	0.021 [0.142]
Indicator for the effective year of minor reforms in neighboring states	0.045 [0.208]
State-level unemployment rate	5.698 [1.436]
State-level log per-capita personal income	10.143 [0.198]

The unit of observation is school districts in Michigan, Illinois, and Indiana. The study period is 1990-2004.

Table 3: Effect of the reform on per-pupil school revenue

Year since the reform	Per-pupil school revenue ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
-1 year				410.969***	54.229	-16.759
				(63.204)	(72.931)	(120.145)
Effective year	364.471***	450.379***	421.110***	452.180***	475.002***	398.213**
	(41.414)	(53.726)	(95.661)	(45.140)	(64.549)	(191.687)
1 year	301.242***	339.050***	273.029***	388.994***	367.953***	246.239
	(51.460)	(67.040)	(103.982)	(57.471)	(82.943)	(215.587)
2 years	-28.773	-204.213***	-323.323***	58.985	-167.078**	-352.162
	(49.599)	(59.667)	(121.918)	(51.237)	(69.651)	(237.677)
3 years	129.673**	-104.827	-262.818*	217.436***	-63.670	-295.187
	(54.834)	(68.516)	(137.061)	(57.503)	(80.355)	(266.935)
4 years	-286.041***	-506.005***	-649.699***	-201.359***	-466.723***	-686.583**
	(56.486)	(69.722)	(161.475)	(58.791)	(79.189)	(305.938)
5 years	-789.190***	-826.390***	-558.769**	-610.591***	-773.519***	-611.769
	(77.489)	(99.871)	(217.923)	(84.454)	(118.760)	(431.700)
6 years+	-274.522***	-372.674***	-225.205	-142.981	-337.262***	-279.695
	(96.319)	(105.512)	(268.890)	(96.799)	(108.502)	(478.279)
Economic controls	No	Yes	Yes	No	Yes	Yes
Time trends	No	No	Yes	No	No	Yes
Obs.	25,505	25,505	25,505	25,505	25,505	25,505
R-squared	0.695	0.698	0.699	0.695	0.698	0.699
Reform 0 - 6 years+	-83.306*	-174.955***	-189.382	23.238	-137.899*	-225.849
	(45.070)	(60.860)	(151.082)	49.361	(74.336)	(300.247)
Reform single dummy	38.924	84.471	375.358***	156.018***	417.994***	817.837***
	(41.010)	(53.756)	(65.986)	(44.704)	(60.925)	(81.082)

Economic controls include the state-level unemployment rate (interacted with revenue group dummies) and state-level log personal income (interacted with revenue group dummies). Time trends are state-group specific linear time trends. Regressions include year dummies for minor reforms (interacted with revenue group dummies), district-fixed effects, and year effects. The last two rows report the average of the dynamic effect of the reform $0 - 6$ years+ and the estimated effect of the reform using a single treatment dummy respectively. Standard errors are clustered at the district level and are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect of the reform on per-pupil instructional and supportive service spending

Year since the reform	Per-pupil instructional spending ($)			Per-pupil supportive service spending ($)		
	(1)	(2)	(3)	(4)	(5)	(6)
-1 year			50.367			-10.005
			(44.590)			(32.815)
Effective year	437.660***	105.492***	174.306**	54.034***	2.349	-11.320
	(15.033)	(31.292)	(78.652)	(12.887)	(25.329)	(56.749)
1 year	483.403***	74.659**	155.172*	38.394***	-42.842	-58.835
	(19.271)	(37.848)	(93.206)	(14.470)	(30.701)	(65.774)
2 years	449.426***	-19.143	67.532	-24.141	-164.016***	-181.233**
	(23.897)	(38.486)	(97.713)	(18.046)	(34.782)	(71.721)
3 years	342.653***	-197.858***	-100.578	-63.649***	-233.432***	-252.755***
	(28.470)	(45.204)	(111.221)	(19.480)	(38.800)	(80.256)
4 years	243.620***	-382.800***	-271.950**	-51.389**	-212.700***	-234.719**
	(28.396)	(51.583)	(126.069)	(20.272)	(44.744)	(92.109)
5 years	187.867***	-593.155***	-433.872**	-178.015***	-213.501***	-245.140*
	(40.360)	(76.974)	(184.482)	(28.764)	(61.157)	(128.871)
6 years+	289.265***	-597.465***	-433.703**	-204.882***	-244.190***	-276.719**
	(44.253)	(86.346)	(191.180)	(35.987)	(73.222)	(139.435)
Economic controls	No	Yes	Yes	No	Yes	Yes
Time trends	No	Yes	Yes	No	Yes	Yes
Obs.	25,505	25,505	25,505	25,505	25,505	25,505
R-squared	0.746	0.750	0.750	0.690	0.696	0.696
Reform 0 - 6 years+	347.699***	-230.038***	-120.442	-61.378	-158.333***	-180.103**
	(22.539)	(49.703)	(124.894)	(17.153)	(41.998)	(89.667)
Reform single dummy	377.638***	372.235***	633.971***	-24.352	21.624	124.222***
	(21.022)	(26.017)	(27.624)	(14.922)	(20.099)	(24.008)

Economic controls include the state-level unemployment rate (interacted with revenue group dummies) and state-level log personal income (interacted with revenue group dummies). Time trends are state-group specific linear time trends. Regressions include year dummies for minor reforms (interacted with revenue group dummies), district-fixed effects, and year effects. The last two rows report the average of the dynamic effect of the reform $0 - 6$ years+ and the estimated effect of the reform using a single treatment dummy respectively. Standard errors are clustered at the district level and are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of the reform on per-pupil school revenue and current spending by revenue group

Revenue group	Per-pupil school revenue ($)		Per-pupil instructional spending ($)		Per-pupil supportive service spending ($)	
	(1)	(2)	(3)	(4)	(5)	(6)
Group 1	54.279	513.694***	363.200***	-126.398**	-61.475***	-16.370
	(54.850)	(137.664)	(27.685)	(60.633)	(17.985)	(44.456)
Group 2	-92.119*	105.964	347.933***	-170.882***	-95.264***	-66.924
	(48.654)	(146.714)	(25.316)	(60.052)	(21.015)	(40.740)
Group 3	-111.924**	-29.366	296.537***	-236.055***	-117.657***	-131.247***
	(49.543)	(143.412)	(26.089)	(64.233)	(18.579)	(47.671)
Group 4	-78.419	-165.778	341.778***	-212.195***	-35.857	-155.851***
	(64.995)	(166.764)	(32.075)	(72.799)	(22.815)	(54.092)
Group 5	-190.955	-1377.440***	389.727***	-405.528**	4.599	-422.745***
	(131.543)	(557.844)	(74.428)	(160.961)	(53.208)	(134.273)
Economic controls	No	Yes	No	Yes	No	Yes
Time trends	No	Yes	No	Yes	No	Yes
Obs.	25,505	25,505	25,505	25,505	25,505	25,505
R-squared	0.696	0.700	0.749	0.751	0.693	0.697

The average of the dynamic effect of the reform 0 – 6 years+ are reported for each revenue group. Group 1 is school districts with per-pupil revenue below the 20th percentile of per-pupil revenue within each state in FY 1994, Group 2 is school districts with per-pupil revenue between the 20th and the 40th percentile of per-pupil revenue within each state in FY 1994, and so on. Economic controls include the state-level unemployment rate (interacted with revenue group dummies) and state-level log personal income (interacted with revenue group dummies). Time trends are state-group specific linear time trends. Regressions include year dummies for minor reforms (interacted with revenue group dummies), district-fixed effects, and year effects. Standard errors are clustered at the district level and are in parentheses.
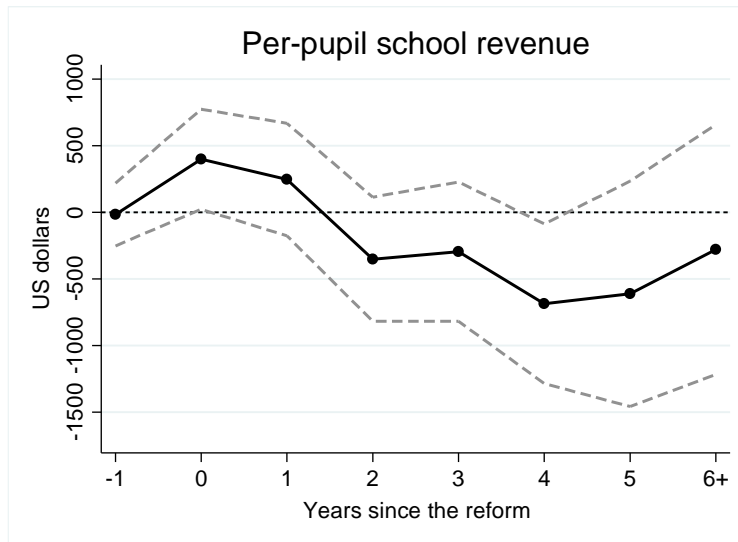* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix A

Fig. A1: Estimated effect of the reform on school revenue

The DD event study method with state-group time trends is employed to estimate. Black solid lines show the estimated effect of the reform, and gray dash lines indicate its 95% confidence intervals

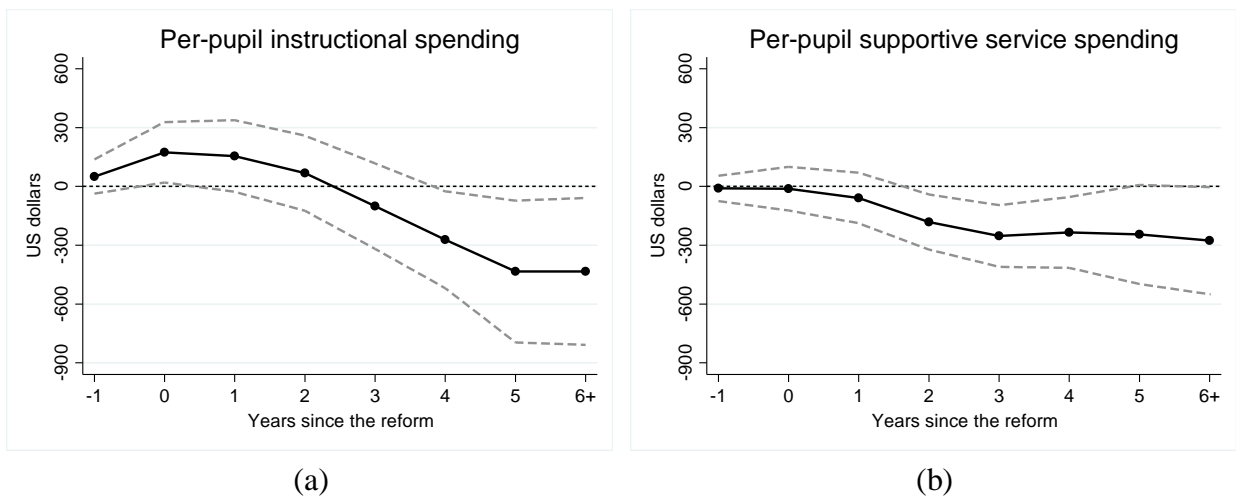(a)   (b)

Fig. A2: Estimated effect of the reform on current spending

The DD event study method with state-group time trends is employed to estimate. Black solid lines show the estimated effect of the reform, and gray dash lines indicate its 95% confidence intervals

Table A1: Data sources

Variable	Data sources
Revenue from state sources Revenue from local sources School revenue Instructional spending Supportive service spending # pupils	• Local Education Agency Finance Survey Data, Common Core of Data, National Center for Education Statistics; FY 1990 • Longitudinal School District Fiscal-Nonfiscal Detail File, Common Core of Data, National Center for Education Statistics; FY 1991 • Public Elementary-Secondary Education Finance data, U.S. Census Bureau; FY 1992-2004
Unemployment rate	• U.S. Bureau of Labor Statistics
Log per-capita personal income	• U.S. Bureau of Economic Analysis

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