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Essays on the Economic Effect of School Finance Policies

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ABSTRACT

ESSAYS ON THE ECONOMIC EFFECT OF SCHOOL FINANCE POLICIES

BY

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This dissertation consists of three chapters empirically analyzing how households and state-local governments respond to economic incentives created by school finance policies.

The first chapter analyzes what effect school capital investments have on housing values and household location choice. If the benefit of school capital investments outweighs the potential increase in local taxes, it would create an incentive for households to move into communities with school capital investments so that school capital investments may increase housing values in the context of the Tiebout model. My research identifies an exogenous variation in school capital investments by exploiting the lottery allocation of entitlement to an interest-free construction bond among districts in California. Although the lottery is exogenous, additional non-lottery allocation complicates identification. I develop an empirical model based on a sample selection method to create a counterfactual state in which additional non-lottery allocation would not have existed. I find that receiving the interest-free construction bond increases school capital expenditure and housing values at the district level. I find little evidence for the effect of the bond on household sorting and student's academic outcomes.

The second chapter studies the centralization of school finance in Michigan and its consequence for school revenue and spending. In an attempt to reduce spending disparities between rich and poor school districts, the Michigan state government centralized a school finance system by restricting local discretion on raising school revenue and increasing grants to district governments. Previous theoretical studies suggest that the centralization could reduce the level of school spending, but the empirical evidence is limited in the literature. Using the district-level panel data on school finance in Michigan and 4 neighboring states for the period of fiscal year 1990-2004, I estimate the effect of the centralization on the level of school revenue and spending and find that the centralization significantly levels down school revenue and spending.

The third chapter investigates how households value the school finance reform's fiscal package in the case of the Michigan reform by estimating the effect on housing values, based on the Tiebout model in which fiscal attractiveness is capitalized into housing values. Although the previous studies have examined how U.S. states school finance reforms affect school resources and educational outcomes, there exists little literature on whether they are fiscally attractive to households beyond the effect on them. My research fills this gap in the literature. I find that the reform increases median housing values in Michigan, having a greater positive effect on housing values in wealthier communities. It implies that the reform benefits Michigan households on average but benefits wealthier households more.

ESSAYS ON THE ECONOMIC EFFECT OF SCHOOL FINANCE POLICIES

BY

JINSUB CHOI

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ACCEPTANCE

This dissertation was prepared under the direction of Jinsub Choi's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Introduction

In the United States, education is the largest expenditure category for state and local governments, followed by health care and public safety programs. In fiscal year 2012, 31.9 % of state and local direct general expenditures went toward education, and over two-third of it were devoted to elementary and secondary education[1]. However, the level of school spending is not very equal across school districts. For example, in fiscal year 2012, school spending in New York City's school district, the largest school district in the United States in terms of the number of students, is $20,226 per pupil, whereas school spending in the largest school district in Utah is below $6,200 per pupil[2].

The level of school spending for families largely depends on which community they reside in, that also determines the amount of local school taxes that families should pay. Due to this characteristic, public school finance can be understood in the framework of the Tiebout model which suggests that households maximize their utility by sorting across communities to shop for better fiscal packages. It implies that families may change their location choices in response to incentives created by school finance policies, also having other implications for housing market and intergovernmental relations. My dissertation is the empirical study for these issues.

In Chapter I, I estimate the effect of school capital investments on housing values and household location choices. Better school infrastructure may improve student's health, safety,

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[1] Statistics come from the Urban Institute:
http://www.urban.org/policy-centers/cross-center-initiatives/state-local-finance-initiative/projects/state-and-local-backgrounders/state-and-local-expenditures

[2] Statistics come from an article in *the Washington Post*:
https://www.washingtonpost.com/news/wonk/wp/2014/05/23/the-dramatic-inequality-of-public-school-spending-in-america/?utm_term=.c826fe4b6e60

academic learning, and children's satisfaction (e.g., aesthetic appeal of good facilities). If the value of better school infrastructure is greater than the increase in local school taxes, households may have an incentive to move into communities with school capital investments. With this household's potential mobility across communities, the value of better school infrastructure should be capitalized into housing values. Based on the Tiebout model, the aforementioned effects on housing values and household sorting are likely to exist, but empirical evidence is limited. I contribute to the literature by suggesting convincing evidence.

In Chapter II, I explore the relationship between school finance system and the level of school resources. School spending inequalities may be solved by centralizing school finance at the state level and distribute a large and equal grant to each school district. However, there may be a consequence of the centralization for the level of school resources. In the spirit of the Tiebout model, previous theoretical studies suggest the potential trade-off between spending equalities and the level of spending. I suggest evidence for this trade-off through my empirical analysis.

In Chapter III, I answer the question of whether a school finance reform, that aims to equalize school spending, is fiscally beneficial to households. I especially focus on the Michigan school finance reform that dramatically changed the mix of school resources and taxes, having an ambiguous effect on household's utility. I evaluate it by estimating the effect of the reform on housing values, based on the Tiebout model that implies the capitalization of local fiscal attractiveness. We may infer whether the school finance reform is fiscally beneficial to households from the estimated effect on housing values.

Chapter I: The Effect of School Capital Investments on Local Housing Markets and Household Sorting: Evidence from the Interest-Free Construction Bond in California

Introduction

The quality of school infrastructure can have an effect on the various outcomes of children in school. For example, attractive school campus would give children aesthetic pleasure, and modern ventilation system would be helpful for children's healthy school life. Thus, it is obvious for parents to prefer to send their children to schools with better infrastructure. It leads to my hypothesis that the fiscal attractiveness of better school infrastructure affects parents' location choice and housing values.

Despite the potential impact of school capital investments on local housing markets and household sorting, this topic has not been thoroughly investigated in the literature. In order to suggest new evidence for it, this essay estimates the treatment effect of winning entitlement to the interest-free construction bond that was allocated by lottery drawing among districts in California. This interest-free construction bond is called the Qualified School Construction Bond (QSCB) [3]. I consider that winning the QSCB lottery would encourage school districts to invest in school facilities which would not have happened otherwise. Thus, my treatment effect of winning the QSCB lottery can reveal what effect better school facilities would have on housing market and household sorting. To the best of my knowledge, this essay is the first research investigating the effect of the lottery allocation of QSCBs on economic outcomes.

[3] The Qualified School Construction Bond (QSCB) was created by the American Recovery and Reinvestment Act of 2009 and nationally provided to school districts through state education agencies in 2009 and 2010. Under the program, selective districts could issue interest-free bonds for the construction and renovation of school facilities and the purchase of land.

In 2009, the California Department of Education received applications for entitlement to the QSCB from school districts and drew lotteries. As a result, 43 lucky districts received QSCBs out of 226 applicants (districts). Since the QSCB allocation was random among applicants, it can provide a good identification strategy to investigate the effect of school capital investments. However, the existence of an additional non-lottery allocation following the initial lottery allocation makes identification complicated; to be specific, winning the QSCB lottery could discourage districts to apply for the additional non-lottery allocation of QSCBs since many of these districts did not need additional QSCBs. Thus, lottery winners (districts) could be less likely to receive additional non-lottery QSCBs, making my estimates for the effect of winning the bond lottery confounded by the additional non-lottery allocation.

To estimate the correct causal effect in a counterfactual state in which the additional non-lottery allocation would not have existed, this essay develops an empirical model that is able to control for additional non-lottery allocation. This model involves double sample selection and a correction procedure based on the existing literature. Under this correction procedure, it is practically difficult to use the 2SLS method that uses the QSCB lottery as an instrumental variable for school capital investments. Thus, this essay estimates the reduced form regression equation.

In respect of theoretical framework, this essay's topic is closely related to the Tiebout model (Tiebout, 1956; Hamilton, 1975). One of the Tiebout model's implication for the allocation of QSCBs is that it would induce households to sort across communities by their preference for the mixture of local school infrastructure and taxes; for example, households with school-age children might be more likely to move into QSCB-awarded districts than households without children. Households without children may move out of QSCB-awarded districts due to a potential increase in local taxes. Another implication of the Tiebout model is the capitalization of school capital investment into housing values; that is, if the present value of the benefit of better school infrastructure is greater than the present value of the cost of an increase in local taxes, housing values should increase as the difference of the fiscal attractiveness is capitalized into housing values.

In my empirical work, I first investigate what effect QSCBs have on housing values at the district level with the expectation that winning the QSCB lottery would increase housing values. I would view the increase in housing values as the capitalization of better school infrastructure. In addition, I estimate whether the QSCB allocation induces households with children (under 18) to move into QSCB-awarded districts but induce households without children to move out of. The existence of this relocation effect would be evidence for underling Tiebout sorting.

There is the vast volume of empirical literature linking school quality to household sorting and housing values[4]. However, the existing literature focuses on current expenditure, test scores, and school choice restrictions as measures of school quality, leaving the role of school facilities relatively unknown. This may be because school capital expenditure is endogenous to unobserved local factors, resulting in a difficulty in empirical identification.

Recently, a few studies suggest convincing evidence for the effect of school capital investments. Cellini et al. (2010) develop a dynamic regression discontinuity design that compares outcomes between a group of districts that narrowly passed bond referenda and a group of districts that narrowly failed the referenda after controlling for the dynamic effect of bond referenda passage. They find that referenda passage largely increases school capital

[4] For example, for the effect of student performance on housing prices, see Ries and Somerville (2010) and Black (1999). For the relationship between school finance equalization and Tiebout sorting, see Chakrabarti and Roy (2015), Hilber and Mayer (2004), and Aaronson (1999). For the effect of the school choice program on housing values, see Reback (2005).

expenditure and consequently lead to an increase in local housing prices by about 6% in California.

Neilson and Zimmerman (2014) choose a different research design to estimate the effect of school capital investments on test scores and home prices. Using a difference-in-differences framework, they compare schools that had construction projects with schools that did not have them in New Haven, Connecticut. Their results suggest that school construction increases home prices in affected neighborhood by about 10% and raises reading scores by 0.15 standard deviations.

Although this present essay is closely related to those studies, it greatly differs in empirical strategy. A key contribution of this essay is that it proposes additional evidence for the effect of school capital investments on housing values by using an independent identification strategy[5]. I find that QSCB lottery increases school capital expenditure, while it hardly affects school current expenditure. The effect on school capital expenditure peaks in the third year of the QSCB allocation and drops after that. My results also show that winning the lottery increases median housing values at the district level. The estimated effects on household sorting outcomes have desired signs, but they are small and not significant. I also estimate the effect of the lottery on student's academic outcomes, but I find little effect.

In the following sections, I explain the QSCB program, empirical strategy, data, results, and then conclude this essay.

[5] Although previous studies on this topic suggested convincing evidence, their empirical designs are not perfect. This makes this present essay's evidence worthwhile. One limitation of Cellini et al. (2010)'s dynamic discontinuity regression design is that it requires to condition on referendum outcomes and a dummy for bond measures which might be endogenous to local unobserved factors. One limitation of Neilson and Zimmerman (2014) is that they only look at schools in one district, so that their estimates might be easily susceptible to spillovers from neighboring areas within a district.

Allocation of the QSCB in California

The QSCB program was a U.S. federal program created by the American Recovery and Reinvestment Act of 2009. Under the program, selected school districts could issue interest-free bonds for the construction and renovation of school facilities and the purchase of land as the federal income tax credit in lieu of district's interest payments would be given to QSCB lenders (financial institutions). QSCBs of $11 billion were nationally provided in 2009 and 2010 respectively, and the U.S. Department of Treasury allocated QSCBs to state's education agencies. Each state's education agency had discretion on how to allocate entitlement to the QSCB to its school districts.

California education agencies received QSCBs of about $800 million for allocation to its districts in 2009 and about $700 million in 2010. Except a few charter schools, the California Department of Education (CDE) had authority to allocate QSCBs to 962 school districts[6]. The CDE held two rounds to allocate QSCBs. In the first round (2009), the CDE received applications from districts and then drew lotteries out of applications until exhausting all of state's QSCB allocation in the presence of the audience on August 28th, 2009. As a result, QSCBs were given to 43 districts out of 226 applicants; each district receives QSCBs of $16 million on average[7]. I consider this first round allocation as random and want to use it to identity the empirical model. Districts which received first round QSCBs were required to issue them by

[6] The number of school districts is the result of my calculation based on enrollment data from the California Department of Education. I exclude the county offices of education, other special schools from the number of school districts for the purpose of this study.

[7] In the first round, each district can apply for QSCBs of a certain amount with the maximum of $25 million. Districts which won the lottery get the whole requested amount, and the state government did not cut the amount.

July, 2010[8]; otherwise, the remaining QSCBs were recaptured and rolled over to the second round allocation.

In the second round (2011~2012), the CDE received new applications (lottery-awarded districts could also apply if they had issued all allocated QSCBs) and ranked them by the following criteria: 1) the date of postmark, 2) projects with approval from the Division of the State Architect, and 3) the percentage of students who qualified for the federal free and reduced-price meals program in fiscal year 2009. In evaluating the second round applicants, districts were not penalized for winning the first round QSCB lottery. 132 districts applied for the second round assignment. 46 districts out of first round applicants and 32 districts out of first round non-applicants received QSCBs in the second round. Among districts winning the first QSCB lottery, only 3 districts applied for the second round, and 1 lottery-won district received QSCBs in the second round. QSCBs which were not issued within 180 days of the date of the second round allocation were rolled over so that the allocations were not complete until early 2012.

One test for the validity of the QSCB lottery is to check whether socioeconomic variables are balanced between a group of districts winning the first round QSCB lottery and a group of districts losing the lottery. Table 1 presents a test for the difference in means of each predetermined variable by winning/losing status. Data mostly comes from the American Community Survey (ACS) 5-year estimates in 2009. Column (1)-(2) show means and standard deviations of each variable by groups. Column (3) shows the difference in means and its standard error. Although the mean of each variable is not perfectly balanced between groups, the

[8] The CDE required that the first round QSCBs must be issued by July, 2010. According to federal regulations, at least 10% of QSCB proceeds must be spent within 6 months of the issuance of QSCBs, and 100% must be spent within 3 years.

Table 1: Mean of Pre-Treatment Variables by QSCB Lottery Status

Variables	Mean of variables: Districts winning the lottery (1)	Districts losing the lottery (2)	Diff. in means (3)
Median owner-occupied housing value ($) 2005-2009	501,108.883	490,092.763	11,016.120
	[218,381.038]	[223,749.418]	(39,062.672)
# households with own children 2005-2009	10,173.275	9,721.983	451.292
	[9,245.710]	[11,218.036]	(1,908.155)
Avg. school capital expenditure per pupil ($) 2005-2009	1,752.253	1,598.735	153.518
	[2,437.123]	[1,680.168]	(323.151)
Avg. school current expenditure per pupil ($) 2005-2009	5,913.835	6,068.107	-133.764
	[692.818]	[1,358.682]	(191.432)
Median household income ($) 2005-2009	68,935.572	67,329.181	1,606.391
	[22671.534]	[20542.106]	(3673.569)
Unemployment rate (%) 2005-2009	8.713	8.488	0.224
	[2.792]	[3.357]	(0.572)
Median income of families with children ($) 2005-2009	72,928.295	74,073.908	-1,145.612
	[30,323.281]	[29,109.189]	(5,144.080)
% 4-year college graduates 2005-2009	15.214	15.972	-0.759
	[7.833]	[8.490]	(1.468)
% high school graduates 2005-2009	24.486	23.533	0.952
	[5.633]	[5.497]	(0.968)
# avg. enrolled students 2005-2009	12,296.100	10,233.402	2,062.698
	[11,084.316]	[11,171.784]	(1,956.144)
Median number of rooms for owner-occupied housing 2005-2009	5.912	5.879	0.034
	[0.505]	[0.512]	[0.090]
Housing vacancy rate (%) 2005-2009	8.169	9.418	-1.250
	[5.012]	[9.948]	(1.620)
% blacks 2005-2009	3.085	4.017	-0. .932
	[4.206]	[4.312]	(0.753)
% Asians 2005-2009	8.365	11.193	-2.828
	[7.455]	[13.191]	(2.163)
Median age 2005-2009	35.285	35.287	-0.002
	[4.444]	[6.588]	(1.096)
% child population 2005-2009	27.163	26.926	0.236
	[3.839]	[5.817]	(0.966)
% aged population 2005-2009	11.480	11.101	0.379
	[3.041]	[4.621]	(0.767)
Observations	40	174	

Standard deviations are in brackets, and standard errors are in parentheses. The sample consists of school districts that applied for the first round of QSCB allocation. I lose 12 observations due to missing data. Asterisks may indicate significance levels for the difference in means, but none of them are statistically significant in the table.

difference in means is minimal when considering the small sample. I find no variable with the statistical difference in means.

Empirical Strategy

Basic model.

In this essay, I want to estimate the treatment effect of winning the QSCB lottery on outcome variables in a counterfactual state in which the second round allocation has not existed; that is, I purse the causal effect of the QSCB lottery, while withholding the unintended event (second round allocation) which is correlated with the first lottery allocation and also affects outcome variables. The existence of second round allocation is a serious obstacle to obtaining the correct treatment effect since the first round allocation can have an unintended effect on outcome variables through the second round allocation; winning the QSCB lottery can discourage school districts to apply for the second round allocation since many of lottery-won districts may not need additional QSCBs[9]. Consequently, winning the lottery can make districts less likely to receive second QSCBs, creating an unintended effect of the lottery through the second round allocation. Since the difference in means of an outcome by lottery status is confounded by such an unintended effect, it cannot be a correct estimator. Thus,

$$\gamma = E(Y_i^1 - Y_i^0) \neq E(Y_i|Qscb1_i = 1, Pt1_i = 1) - E(Y_i|Qscb1_i = 0, Pt1_i = 1) \quad (1)$$

In (1), i indexes school districts. Y_i^1 is a potential outcome of winning the QSCB lottery when unintended responses do not occur, and Y_i^0 be a corresponding potential outcome of losing the

[9] Data indeed shows that only 3 districts out of 40 lottery-won districts applied for the second round, whereas 71 districts out of 174 lottery-lost districts applied for the second round.

QSCB lottery. Y_i is an observed outcome, $Qscb1_i$ is a dummy for winning the QSCB lottery, and $Pt1_i$ is a dummy for participation in the first round of QSCB allocation. (1) says that the difference in means of an outcome variable by lottery status is not the estimator for the treatment effect that I pursue. The goal of my empirical model is to obtain the treatment effect after taking the unintended effect through the second allocation and sample selection into account, so that we would obtain estimates for a treatment effect close to γ.

In controlling for the unintended effect through the second round allocation, I consider the following OLS regression (as it is, it would not be consistently estimated).

$$Y_i = \gamma Qscb1_i + \alpha Pt2_i + X_i'\beta + v_i \quad if\ Pt1_i = 1 \tag{2}$$

$Pt2_i$ is a dummy for participation in the second round allocation which is included in order to create a counterfactual state in which the second round allocation has not existed by capturing the effect of the QSCB lottery on Y_i through the second round allocation. X_i is controlled variables. Y_i is trends in a housing market and household sorting outcome (e.g. a percent change in median housing values and a percent change in the number of households with children). γ is the treatment effect of winning the QSCB lottery[10].

In estimating equation (2), two issues are raised. First, $Pt2_i$ is potentially endogenous to unobserved local confounders. For example, the decision on participation in the second round is likely to be affected by housing market and household sorting trends and the unobservables such as the current stock of school capital. Second, the model's sample is self-selected as the sample

[10] Treatment is defined here as winning the QSCB lottery. If treatment was alternatively defined as the actual issue of QSCBs, γ would be an estimate for the intention-to-treat effect. QSCB-awarded districts could refuse to issue QSCBs due to the failure of the bond referenda passage or other uncertain reasons. Data shows that 31 districts out of 43 lottery-won districts issued QSCBs.

only includes participants (districts) in the first round. First round participants and non-participants are likely to be different in community traits from each other in many ways, so that the treatment effect would not be very compelling if the effect is estimated only among first round participants. Thus, I pursue the estimation of the treatment effect in the full sample (including all districts) by using the self-selected sample consisting of first round participants. It means that I need to correct sample selection bias when estimating equation (2). In the following section, I set up a model to overcome those two problems and then consistently estimate equation (2).

Double sample selection approach.

In this approach, I want to restrict the sample to school districts which apply for the first round allocation but do not apply for the second round. With the double sample selection, $Pt2_i$ is suppressed in (2), so that we do not need to condition on this endogenous variable any more. The model is expressed by the following system of equations.

$$Y_i = \gamma Qscb1_i + X_i'\beta + v_i \quad if\ Pt1_i = 1\ \&\ Pt2_i = 0 \tag{3}$$

$$Pt1_i^* = X_i'\delta_1 + u_{1i} \tag{4}$$

$$\sim Pt2_i^* = \alpha_1 Qscb1_i + \alpha_2 Pt1_i + X_i'\delta_2 + u_{2i} \tag{5}$$

In equation (3), the sample is selected into districts which apply for the first round but do not apply for the second round ($Pt1_i = 1$ and $Pt2_i = 0$). In participation equation (4) and (5), $Pt1_i^*$ and $\sim Pt2_i^*$ are unobservable latent variables for $Pt1_i$ and $\sim Pt2_i$ respectively in which $\sim Pt2_i$ is defined as a dummy for non-participation in the second round. Since my sample is restricted into second round non-participants (that is, $Pt2_i = 0$) rather than participants, I prefer to use the non-participation dummy in equation (5)[11].

Participation equations (4) and (5) have the recursive structure, so that $Pt1_i$ is on the left side of equation (4) (in the form of a latent variable) and on the right side of equation (5). The coefficient on $Pt1_i$ may not have a clear interpretation in participation equation (5), but $Qscb1_i$ becomes exogenous only if conditioning on $Pt1_i$.

α_1 is the coefficient on $Qscb1_i$ in participation equation (5). I expect the sign of α_1 to be positive as the QSCB lottery discouraged the participation in the second round. I allow the error terms u_{1i} and u_{2i} to be correlated with each other, which indicates a correlation between $Pt1_i$ and u_2 in equation (5). I assume that the vector of error terms $(v_i, u_{1i}, u_{2i})'$ has a trivariate normal distribution with mean zero and the variance-covariance matrix given by

$$\Sigma = \begin{pmatrix} \sigma_v^2 & \rho_{v1} & \rho_{v2} \\ \rho_{v1} & 1 & \rho \\ \rho_{v2} & \rho & 1 \end{pmatrix} \quad (6)$$

Under the assumption of the bivariate normal distribution of u_{1i} and u_{2i}, we can consistently estimate participation equations (4) and (5) (regardless of the endogeneity of $Pt1_i$) by the full maximum likelihood procedure of the bivariate probit model[12].

Under the double sample selection, the estimated parameters of housing market and household sorting equation (3) are not consistent unless both ρ_{v1} and ρ_{v2} are zero in Σ, which is not likely to hold. Therefore, I add sample selection correction terms, $\widehat{\lambda 1}_i$ and $\widehat{\lambda 2}_i$, to housing

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[11] It would lead to equivalent results regardless of which dummy is used between $Pt2_i$ and $\sim Pt2_i$. Existing literature derives the additive correction terms for the sample selection model in terms of participation. In order to lead to equivalent results regardless of the use between $Pt2_i$ and $\sim Pt2_i$, one needs to newly derive the additive correction terms in terms of non-participation. The use of $\sim Pt2_i$ in lieu of $Pt2_i$ allow us to avoid such complication with no harm.

[12] See Greene (2012), Ch. 17.5.5 and Wooldridge (2010), Ch. 15.7.3.

market and household sorting equation (3). $\widehat{\lambda 1}_i$ and $\widehat{\lambda 2}_i$ can be obtained by estimating participation equation (4) and (5). As a result, the equation that I estimate in this essay is

$$Y_i = \gamma Qscb1_i + X_i'\beta + \sigma_{v1}\lambda 1_i + \sigma_{v2}\lambda 2_i + \varepsilon_i \quad if\ Pt1_i = 1\ \&\ Pt2_i = 0 \tag{7}$$

This correction procedure with two additive terms ($\lambda 1_i$ and $\lambda 2_i$) are suggested by Poirier (1980) and Ham (1982) and can be understood as the extension of Heckman's two-stage procedure (Heckman, 1979). The Heckman correction model is generally not valid in the case of double sample selection. The correction model that I use here works properly in the case of double sample selection, the correlation between u_{1i} and u_{2i} (that is, $\rho \neq 0$), and sample selection on the unobservables under the assumption that stated above. The formula for $\lambda 1_i$ and $\lambda 2_i$ are presented in Appendix A.

γ is the treatment effect of winning the QSCB lottery on Y_i. I expect that winning the lottery would affect Y_i through an increase in school capital investments at the district level. However, more school capital investments do not mean immediate better school facilities since construction and renovation take time. Some of QSCB-funded construction projects might not be even complete for the study period. Furthermore, it is difficult to isolate my treatment effect from an effect through interest savings on QSCBs. Thus, my empirical model does not aim to precisely estimate the size of the effect of better school facilities. Instead, I would draw implication of better school facilities from the treatment effect of winning the lottery.

In estimating equation (7), standard errors are not consistent since additive correction terms are just proxies for true $\lambda 1_i$ and $\lambda 2_i$. To overcome this issue, I use asymptotically consistent standard errors under double sample selection, which is suggested by Ham (1982). A key procedure to derive the formula for this standard error is to approximate $\lambda_i - \hat{\lambda}_i$ by first-order Taylor series of $\hat{\lambda}_i$ with respect to parameters. The resulting standard errors is consistent under sample selection and robust to heteroscedasticity. The formula is presented in Appendix B.

Data

I obtain data on QSCB allocations in California from the website of the California Department of Education[13]. The data contains the list of districts that applied for the QSCB

Table 2: Descriptive Statistics

	Mean (1)	S.D. (2)
Panel A: *Outcome variables*		
%Δ median owner-occupied housing values	-30.270	14.163
%Δ# households with own children	-1.221	18.744
%Δ# households without own children	7.572	14.302
Δ housing vacancy rate (%)	0.333	0.359
Log school capital expenditure per pupil+1 2013	5.807	2.257
Inverse hyperbolic sine school capital expenditure per pupil 2013	6.445	2.389
Log school current expenditure for instruction 2013	8.628	0.177
Log CAASPP test score 2015	7.824	0.020
% students who meet a standard of CAASPP test 2015	23.651	6.065
% students who exceed a standard of CAASPP test 2015	14.097	10.328
Panel B. *QSCB variables*		
Dummy for winning the QSCB lottery 2009	0.187	0.391
Dummy for participation in the 2nd round allocation 2011-2012	0.346	0.477
Panel C. *Economic controls*		
Log median household income 5-year estimate 2009	11.075	0.308
Poverty rate 5-year estimate 2009	13.303	7.400
Unemployment rate 5-year estimate 2009	8.530	3.254
Log median family income with children 5-year estimate 2009	11.133	0.398
Poverty rate for families with children 5-year estimate 2009	14.463	9.114
Unemployment rate for parents 5-year estimate 2009	5.955	3.474

(continued)

[13] The webpage about the QSCB allocations was closed and became no longer publicly available since Jan. 27, 2016.

Table 2: Descriptive Statistics *(Continued)*

	Mean (1)	S.D. (2)
Panel D. *Demographic controls*		
% people with graduate degrees 5-year estimate 2009	8.121	6.650
% 4-year college graduates 5-year estimate 2009	15.830	8.358
% people with some college experience 5-year estimate 2009	30.264	6.796
% high school graduates 5-year estimate 2009	23.711	5.522
% blacks 5-year estimate 2009	3.843	4.298
% Asians 5-year estimate 2009	10.665	12.358
% other racial minorities 5-year estimate 2009	17.777	10.048
% children population 5-year estimate 2009	26.970	5.495
% aged population 5-year estimate 2009	11.171	4.365
Log median age 5-year estimate 2009	3.549	0.171
Panel E. *District controls*		
Log number of enrolled students 2005-2009	8.560	1.436
Log current expenditure per pupil 2005-2009	8.693	0.145
Dummy for an elementary school district	0.341	0.475
Dummy for a high school district	0.136	0.343
Panel F. *Housing controls*		
Log median number of rooms 5-year estimates 2009	1.768	0.993
Housing vacancy rate 5-year estimate 2009	9.185	9.231
% single-family homes 5-year estimate 2009	89.188	8.305
Housing ownership rate 5-year estimate 2009	63.276	11.387

A sample includes applicants for the first round allocation (Obs.=214)

allocation and allocation results in the first or second round allocations. Data on school capital and current expenditures is obtained from the LEA Revenue and Expenditure Report SACS Data in the California Department of Education. In this essay, school capital expenditure includes costs of construction, renovation, the purchase of equipment, and the purchase of land; school current expenditure for instruction includes costs of instructor's salaries and benefits as well as costs of class materials.

Data on district-level socioeconomic characteristics comes from the American Community Survey (ACS) 5-year estimates in 2009 and 2014. The 5-year estimates are based on the survey for the last 5 years. For example, the ACS 5-year estimates in 2009 is based on the survey on communities from 2005 to 2009. The 1-year estimates would be more timely but omit a number of small districts because they are noisier.

Table 2 reports descriptive summary statistics for first round applicants. Panel A shows statistics for outcome variables, and the rest of panels present statistics for independent variables. The first four outcome variables are obtained by comparing between the ACS 5-year estimates in 2009 and 2014. In panel A, I do not report variables for school expenditures in 2010-2012 and 2014-2015 to save space. In the rest of panels, independent variables include a dummy for winning the QSCB lottery, economic controls such as median household income, demographic controls such as racial composition, district controls such as log number of enrolled students, and housing controls such as log median number of rooms.

Results

Table 3 presents the estimation results for participation equations (4) and (5) from which additive correction terms are created. The result shows that winning the QSCB lottery discourages districts to participate in the second round allocation; the coefficient on dummy for winning the QSCB lottery is 1.260 in column (2), which implies that winning the lottery decreases the probability of participation in the second round by about 17.6% (average partial effect). It shows that the lottery would have an unintended negative effect on school capital investments through a decrease in participation in the second round.

Before discussing the effect of the QSCB lottery on housing market and household sorting outcomes, we need to check whether winning the lottery increases district's capital investments. If QSCBs had funded school construction projects that would had been funded by

Table 3: Participation in the QSCB Allocation; Recursive Bivariate Probit Model

VARIABLES	Participation in 1st round (1)	Non-participation in 2nd round (2)
QSCB lottery		1.260***
		(0.387)
Participation in the 1st round		0.079
		(0.860)
Log median household income	0.166	0.538
	(0.588)	(0.599)
Poverty rate	0.005	0.016
	(0.021)	(0.019)
Unemployment rate	0.009	-0.003
	(0.019)	(0.023)
Log current school expenditure	0.683**	0.022
per pupil	(0.302)	(0.397)
Log of # enrolled students	0.329***	-0.197**
	(0.059)	(0.088)
% Blacks	-0.020	0.005
	(0.014)	(0.014)
% Asians	0.014**	-0.007
	(0.007)	(0.008)
% Other races	-0.008	-0.008
	(0.009)	(0.010)
% child population	-0.015	0.027
	(0.015)	(0.017)
% aged population	-0.017	0.016
	(0.018)	(0.017)
Observations	921	921
ρ = -0.633 (0.402)		

Additional controls include log median income of families with children, poverty rate for families with children, unemployment rate for parents, educational attainments, log median age, level of school districts, log median number of rooms, housing vacancy rate, percent of single family homes, home ownership rate, and county dummies. Robust standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

other construction bonds anyway, QSCB funding could merely substitute for other bond funding. In this case, winning the lottery could have little effect on school capital investments. With little increase in school capital investment, the effect of the lottery on my outcome variables would only reflect the improved financial solvency for districts. To check this issue, I estimate the treatment effect of winning the lottery on school expenditures by adopting the double sample selection approach. The results are reported in Table 4.

Table 4: Effect of Winning the QSCB Lottery on School Expenditures

Fiscal year	2010 $(t = 0)$ (1)	2011 $(t + 1)$ (2)	2012 $(t + 2)$ (3)	2013 $(t + 3)$ (4)	2014 $(t + 4)$ (5)	2015 $(t + 5)$ (6)
			$A.\ log(Capital\ Expenditure\ per\ pupil + 1)$			
QSCB lottery	0.512	0.205	1.188	2.202*	0.149	-1.362
	(1.097)	(1.286)	(1.380)	(1.254)	(1.004)	(1.108)
			$B.\ sinh^{-1}(Capital\ Expenditure\ per\ pupil)$			
QSCB lottery	0.499	0.237	1.341	2.290*	0.846	-1.453
	(1.164)	(1.354)	(1.464)	(1.333)	(1.071)	(1.178)
			$C.\ log(Current\ expenditure\ for\ instruction\ per\ pupil)$			
QSCB lottery	0.002	-0.073	-0.038	-0.073	-0.066	-0.121
	(0.037)	(0.043)	(0.043)	(0.062)	(0.064)	(0.087)
Observations	140	140	140	140	140	140

Each column is a separate regression. All specifications include economic controls, demographic controls, district controls, housing market controls, county dummies, log capital expenditure per pupil 2005-2009 (panel A and B), and log current expenditure for instruction per pupil 2005-2009 (panel C). For the detail of these covariates, please refer to descriptive statistics in table 2. Standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

In Table 4, outcome variables are log school capital expenditure per pupil and log instructional expenditure per pupil. Each column is a separate regression. Since school expenditures have right- skewed distributions (especially for capital expenditures), I transform the outcome variable by taking the logarithm of them. One problem of this transformation is that a few districts spend no money on school capital in certain years, so that there are zero values. Thus, I adopt the two different approaches. First, I add one to each capital expenditure per pupil when taking the logarithm of it in Panel A. The second approach is that I take the inverse hyperbolic sine function of capital expenditure per pupil in Panel B[14].

The results show an interesting dynamic effect on capital expenditure in both panel A and panel B; the effect on capital expenditure increases in magnitude until the third year (fiscal year 2013) of QSCB allocation and is suppressed after that. In the third year of QSCB allocation, the estimated effect is 2.2 log points and is statistically significant at the 10 percent level in panel A. Panel C reports the effect on school current expenditure for instruction. Winning the QSCB lottery appears to have a negative effect on instructional expenditure, even though estimates are not significant. We can observe that the negative effect on instructional expenditure tends to increase over time. This may be because the increase in school capital expenditure has a substitution effect on instructional expenditure.

Table 5 reports the estimates for the effect of the QSCB lottery on housing market and household sorting outcomes. I use the double sample selection model for estimation. I include all controlled variables in panel A but impose exclusion restrictions in panel B-D. In Panel A, column (1) shows that winning the QSCB lottery increases median housing values by 17.5 percentage points. It implies that the benefit of school capital investments exceeds the expected increase in local taxes in the future. In the context of the Tiebout model, the effect suggests the capitalization of school capital investments. The size of the estimated effect seems to be larger than estimates suggested by other studies; Cellini et al. (2010) find that the passage of construction bond referenda increases housing prices by about 6%, and Neilson and Zimmerman (2014) find that school construction projects increase housing prices by about 10%. However,

[14] The inverse hyperbolic sine function is the approximation of the log function but can take zero and negative values. This function is sometimes used for the transformation of variables with extreme values as well as zeros and negative values in the literature. For the further discussion about the inverse hyperbolic sine function, see MacKinnon and Magee (1990) and Burbidge et al. (1988).

Table 5: Effect of Winning the QSCB Lottery on Housing Market and Household Sorting Outcomes

	%*Δ* median housing value	*Δ* housing vacancy rate (%)	%*Δ* households with own children	%*Δ* households without own children
	(1)	(2)	(3)	(4)
	A. Controlling for all covariates			
QSCB lottery	17.495*	-3.157	4.118	-1.537
	(10.366)	(2.174)	(18.282)	(19.150)
	B. Exclusion restriction 1 (log school expenditure)			
QSCB lottery	17.506**	-3.159	4.098	-1.570
	(7.445)	(1.748)	(15.913)	(8.985)
	C. Exclusion restriction 2 (racial composition)			
QSCB lottery	14.957***	-2.162	10.480	-5.537
	(5.522)	(1.786)	(9.665)	(7.991)
	D. Exclusion restriction 3 (log median age)			
QSCB lottery	14.803**	-3.988*	5.677	2.391
	(6.181)	(2.067)	(13.062)	(13.446)
Observations	140	140	140	140

All specifications include economic controls, demographic controls, district controls, housing market controls, and county dummies. For the detail of these covariates, please refer to descriptive statistics in table 2. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

considering that my estimate somewhat reflects interest savings on QSCBs as well as school capital investment itself, it does not seem to be indefensibly large. In column (2), the estimated effect of the lottery on the change in the housing vacancy rate is negative but not significant. Results for household sorting outcomes show that coefficients have desired signs in column (3)-(4), but they are not significant.

In estimating the double sample selection model in panel A, there is a concern that the inclusion of the additive correction terms $\lambda 1$ and $\lambda 2$ in my regression may cause collinearity among covariates, even though the model is formally identified. The collinearity could occur since all covariates employed to estimate $\lambda 1$ and $\lambda 2$ are also controlled in my regression. The complicated nonlinearity of $\lambda 1$ and $\lambda 2$ would prevent complete collinearity, but concern still remains. The collinearity does not bias my estimates but could make them less precise.

To check whether this potential collinearity has resulted in considerable imprecision of my estimates, I exclude several controls from my regression by imposing exclusion restrictions in panel B-D. These exclusion restrictions are justified if excluded controlled variables are not correlated with an error term when conditioning on other covariates. I exclude log current school expenditure in panel B, racial composition in panel C, and log median age in panel D. With the exclusion of some controls in panel B-D, I find that coefficients are generally similar as in panel A and that standard errors become smaller. Smaller standard errors mean more precise estimates. In panel D, the estimated effect of the lottery on the change in housing vacancy rate becomes significant at the 10% level. Unfortunately, the estimated effects on household sorting outcomes are still not significant in panel B-D.

One may be curious about the effect of school capital investments on student's academic performance, expecting that the improvement of school buildings and equipment may be helpful for student's learning. To check this issue, I estimate the effect of winning the QSCB lottery on student's academic performance by using the double sample selection model. A change in student's academic performance can be caused by household sorting as well as the improvement of buildings and equipment. However, we would have some implications from the results if the effect of household sorting is limited. I measure academic performance by using the California Assessment of Student Performance and Progress (CAASPP) test results in 2015[15]. Dependent variables that I investigate are log of average CAASPP score, the percentage of students who

[15] CAASPP test is a standardized test in English language, arts/literacy, and mathematics for students enrolled in public schools in California. The test began in 2014. The academic outcomes that I use in this essay are based on the average of scores over all grades in public schools within each district.

exceeds a specific standard, and the percentage of students who marginally meet the standard. Table 6 reports the results. It shows that the lottery has little effect on the test results. This is the similar results suggested by Martorell et al. (2016) and Cellini et al. (2010).

Table 6: Effect of Winning the QSCB Lottery on Student's Performance

	Log CAASPP test score 2015	% students met 2015	% students exceeded 2015
	(1)	(3)	(2)
QSCB lottery	0.001	0.524	-1.619
	(0.016)	(5.986)	(6.176)
Observations	139	139	139

One district is dropped from the sample due to a missing value in outcome variables. All specifications include economic controls, demographic controls, district controls, housing market controls, and county dummies. For the detail of these covariates, please refer to descriptive statistics in table 2. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Concluding Remarks

In this essay, I estimate the effect of winning the QSCB lottery on school expenditures, housing values, household sorting, and student's academic outcomes. I find evidence that winning the lottery increases school capital expenditure and median housing values at the district level, but I find little evidence for the effect of the lottery on household sorting and student's academic outcomes. I view the estimated effect of the lottery on median housing values as the evidence for the capitalization of school capital investments into housing values. However, we cannot completely exclude the possibility that a change in median housing values can be driven by the changing composition of house types within a community. This sort of bias would be limited if winning the lottery does not dramatically induce house construction, renovation, and demolition for a relatively short period.

It is worth noting that it takes time to complete the construction of school facilities, so that some of QSCB-funded construction projects might be still on the way beyond the study period. This may be the reason why my estimates for household sorting outcomes are not significant. There is a possibility that household sorting is actually happening but is not fully realized for the study period because of incomplete construction. On the other hand, housing values are likely to quickly respond to school capital investment even before the completion of facilities. Thus, I can find the significant effect on housing values for the study period. Unfortunately, I am not able to identify when each construction or renovation project was complete due to the lack of data.

Chapter II: The Effect of the Centralization of School Finance on School Revenue and Spending: Evidence from a Reform in Michigan

Introduction

In the United States, public education has been traditionally financed by 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. However, the wide disparity in school spending between richer and poorer school districts has been a great concern in this finance system. Since 1970s, many lawsuits and legislative actions have followed across the country, brining significant changes to the existing school finance system in many states. Michigan school finance reform, called Proposal A, was one of these changes in the school finance system in an attempt to reduce school spending inequalities. It centralized school revenue sources, so that the state can equally distribute money to each school district.

Michigan school finance reform is distinguished from most of other state-level school finance court-rulings and legislative actions in important ways. Most court-rulings and legislative actions have brought changes to grant formulas in favor of poorer districts, leaving local discretion on raising revenue relatively intact. As a result, districts have still relied heavily on local-source revenue even after the grant formula change. On the other hand, Michigan's reform sharply reduced local property taxes and introduced the large amount of a foundation grant that accounts for about 60-80% of total school revenue, so that districts have become highly dependent on state funding. Thus, Michigan's reform can be defined as the centralization of school finance with limited local discretion on revenue supplementation (Loeb, 2001).

Fischel (1986 and 1996) suggests that the elimination of the Tiebout-style school finance system (i.e. moving toward the centralization of school finance) breaks the tax-benefit linkage for public education, converting property taxes from local education fees into taxes with a deadweight loss. He argues that it makes the provision of public education would become less popular among residents in rich districts, and eventually decreases mean school spending. Fernandez and Rogerson (1999) estimate structural parameters to investigate what effect the centralization of California school finance has on school spending under the assumption of perfect Tiebout sorting by family income. The results suggest the reduction of school spending by a large amount. In the similar spirit of the Tiebout model, Loeb (2001) develops a theoretical model to examine the effect of the centralization of Michigan school finance, assuming that households perfectly sort into communities by the demand for education inputs. The simulation results suggest that mean per-pupil spending decreases by about $100-$700. In both Fernandez and Rogerson (1999) and Loeb (2001), it is assumed that, under the centralized system of school finance, the level of school spending is determined by the voter with the median demand for education. Since the median demand is generally lower than the average demand, it is intuitive that the centralization would lower the level of school spending.

In the literature, the empirical evidence for the effect of the centralization of school finance on the level of school spending is limited. Silva and Sonstelie (1995) empirically estimate price and income effects of the centralization of California school finance, finding that it decreases mean per-pupil spending by about $1,200. Manwaring and Sheffrin (1997) find that the centralization of California school finance reduces per-pupil spending by about $600-$800 in the long run. Hoxby (2001) focuses on a tax price that is defined as the amount of revenue that need to be raised for an extra dollar school spending. She finds that a higher tax price leads to lower school spending per pupil, implying that the elimination of local discretion on raising extra revenue would result in a reduction in spending. Chaudhary (2009) suggests different results that Michigan school finance reform increases log mean per-pupil spending when compared to trends in Illinois. However, her estimates could be biased by confounded preexisting trends that will be discussed in a later section.

There exists the related literature regarding the effect of school finance court-ruling on school spending. Papers generally find that the court-ruling reduces the inequality in school spending among districts (Card and Payne, 2002; Murray et al., 1998) and increase mean per-pupil spending (Jackson et al., 2016; Lafortune et al., 2016; Sims, 2011a; Sims, 2011b). Some papers study Michigan school finance reform and find that the reform leads to resource equalization among districts (Chakrabarti and Roy, 2015; Roy, 2011; Papke, 2005), but the question about the effect on the level of resources is not clearly answered.

This present essay suggests fresh evidence for the effect of the centralization of school finance on the level of school revenue and spending by using Michigan school finance reform as a policy variation. This new evidence would complement existing empirical evidence that are mostly based on weak identification strategies. This essay uses difference-in-difference (DD) framework that compares districts between Michigan and 4 neighboring states (Illinois, Ohio, Indiana, and Pennsylvania), considering that neighboring states as the valid control group. School districts are grouped together by the pre-reform level of school revenue, and the heterogeneous effect of the Michigan's reform is examined across groups.

My results are consistent with the prediction of the Tiebout model that the diminishing local financing of public education would result in less school revenue and spending as the provision of public education wins less support from residents. I find that Michigan's reform reduces per-pupil revenue in both higher- and lower-revenue districts. The reform equalizes revenue among districts by reducing revenue in higher-revenue districts faster than in lower-revenue districts. I find that the reform also has the similar effect on current spending. I find no evidence for the effect of the reform on school capital spending.

School Finance in Michigan

Before the reform in 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[16]. This system intended to equalize school revenue by guaranteeing that poorer districts had the same power to raise revenue as richer districts had. Before the reform in Michigan, districts below 20th percentile of school revenue had funded about 60% of their revenue on its own, and districts above the 80th percentile had funded 80 % on its own (as seen in Figure 1). These values were substantially higher than values that four neighboring states had, implying that the Michigan's power equalization program had less actively intervened in school finance so had played a smaller role in reducing resource inequalities than neighboring state's equalization programs had[17]. Michigan program's minimum tax base was fairly low, so that not many districts benefited from the power equalization program. In fiscal year (FY) 1994, 39 percent of districts received the positive power equalization grant (excluding a flat grant) from the state government (Courant and Loeb, 1997)[18].

[16] The local property tax base is called the State Equalized Value, which was approximately one-half of market value in Michigan.
[17] Illinois, Ohio, and Indiana had partial foundation aid programs.
[18] 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 larger role of the state government in public education were important factors leading to Michigan's reform in 1994. The reform also was largely motivated by the heavy property tax burden. Before the reform, Michigan's residents had the property tax burden higher than the national average by about 33 percent but had the sales tax burden lower than the national average by about 32 percent (Office of Revenue and Tax Analysis, 2002). There had been a strong demand for adjusting this imbalance between property and sales taxes and the relief of property taxes in Michigan. This resulted in referendum on school finance reform, called Proposal A, which was passed by 69 percent to 39 percent in FY 1994. (this new program became effective in FY 1995).

The reform introduced the foundation aid program in which the state School Aid Fund provided the minimum per-pupil school revenue (called a foundation allowance) to each school district with some limited local discretion on raising extra revenue. The level of the foundation allowance initially depended on district's per-pupil revenue in FY 1994 so that higher-revenue districts received larger foundation allowances at first. The foundation aid program gradually reduced the initial gap in foundation allowances among districts by increasing smaller foundation allowances faster than larger foundation allowances over time. The gradual equalization of foundation allowances is widely reported in the existing literature (e.g. Chakrabarti and Roy, 2015; Roy, 2011).

A school operating tax on owner-occupied housing were eliminated, and the school operating tax rate on nonhomestead properties (such as commercial buildings) was required to drop to 18 mills if the tax rate was above 18 mills[19]. As a result, each district government had less discretion on how much it raises extra local revenue so that they heavily relied on state's

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[19] A cap on the property taxable value was also introduced so that the taxable value growth rate could not exceed both 5% and the inflation rate each year.

foundation grant for school revenue. In order to finance the state School Aid Fund, the reform increased a state general sales tax from 4% to 6% and also newly introduced statewide property taxes of 6 mills[20].

Under the new school finance system, some rich 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 rarely imposed. Thus, we can say that local-source school revenue for school operations was effectively capped by 18 mill school operating tax on nonhomestead properties. Districts still had the full discretion on imposing a school debt millage as well as a sinking fund millage, but revenue from them could only be spent on school capital projects.

Table 7 presents school revenue from federal, state and local sources in pre- and post-reform years. Column (3) shows that per-pupil revenue from state sources increased by about $3,200 in first year of the reform. This increase in revenue from state sources were mostly due to an increase in formula grants (e.g. a foundation grant, a flat grant, and a categorical grant). The higher level of state's grant was offset by a large drop in per-pupil revenue from local sources by about $3,100. The drop in local-source revenue was mainly attributed to a drop in local property taxes.

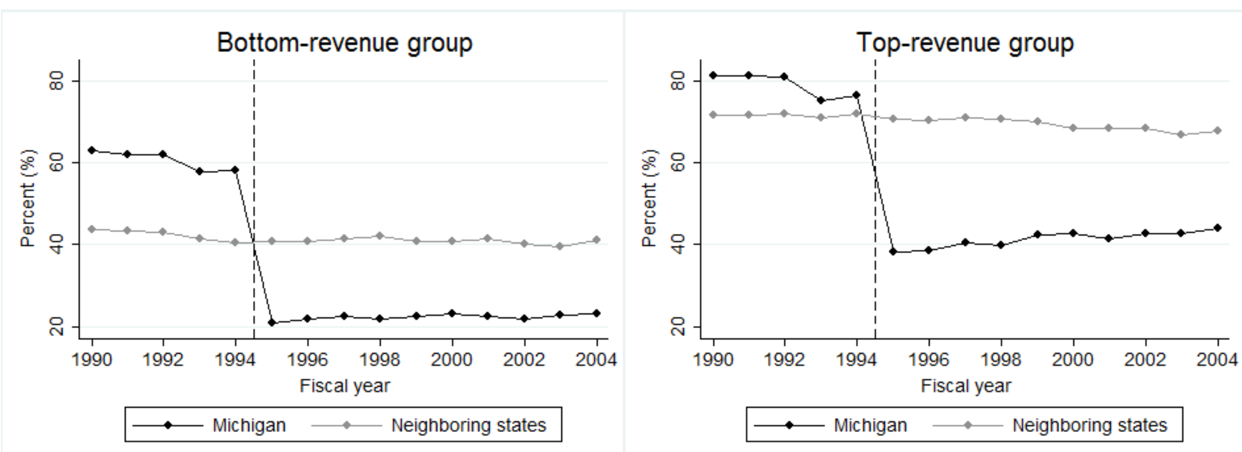
Figure 1 shows trends in the percent of revenue from local sources. The bottom (top) revenue group is defined as districts with per-pupil revenue below the 20th (above 80th) percentile of the per-pupil revenue distribution within each state in FY 1994. These revenue groups are state-specific groups. We can see that the percent of local-source revenue dramatically dropped by about 40% in both bottom- and top-revenue groups in Michigan as a result of the reform. It

[20] 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%.

Table 7: Sources of School Revenue in Michigan

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

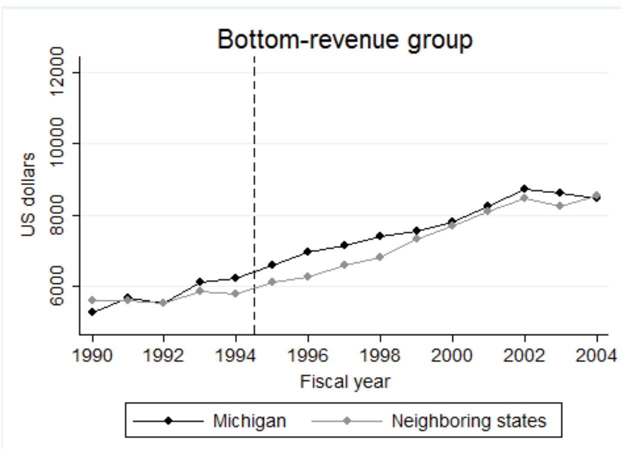
Values are adjusted to 2004 prices by using the CPI. Standard deviations are in brackets, and standard errors are in parentheses.



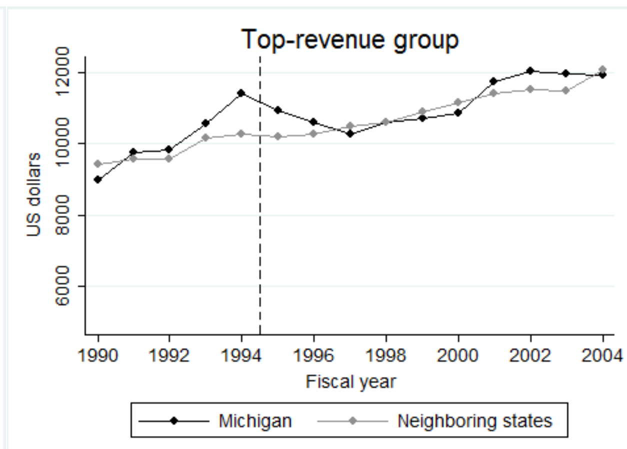
(a) (b)

Figure 1: Trends in the Percent of School Revenue from Local Sources

means that Michigan's school revenue source was substantially centralized as moving from the power equalization program and toward the foundation aid program. On the other hand, there had not been any discontinuous changes in neighboring state (Illinois, Ohio, Indiana, and Pennsylvania)'s trend for our study period. This is because there had not been major changes to the school finance system in neighboring states, supporting the use of neighboring states as the control group for our empirical analysis.



(a)



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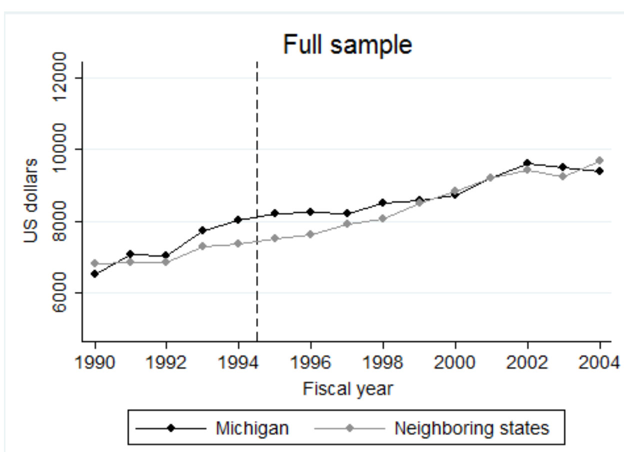


(c)

Figure 2: Trends in Per-Pupil School Revenue by Revenue Group

Figure 2 presents trends in per-pupil revenue of the bottom- and top-revenue groups. There seems to be no noticeable effect of the reform on per-pupil revenue in the bottom-revenue group in Michigan, while the reform seems to have a negative effect on per-pupil revenue in the top-revenue group. This essay wants to examine trends in Michigan school revenue and spending when compared to the counterfactual trend in which the Michigan's reform would had not happened. With regard to this empirical goal, the important question is whether we can consider trends in neighboring state's school revenue and spending as the counterfactual trend and can obtain unbiased estimates by comparing trends between Michigan and neighboring states. Figure 2 shows that Michigan's per-pupil revenue grew faster than neighboring states for a pre-reform period, so that there was likely to be difference in preexisting trends between Michigan and neighboring states (control group). Thus, an important issue of our empirical identification would be how to capture the different preexisting trends between Michigan and neighboring states, which will be dealt with in a section for Empirical Strategy.

Data

Our empirical analysis is based on panel data for the period FY 1990-2004 in Michigan and 4 neighboring states (Illinois, Indiana, Ohio, and Pennsylvania). This data set has multiple sources. I obtain data on district-level revenues, expenditures, and the number of pupils from the Public Elementary-Secondary Education Finance Data of the U.S. Census Bureau for the period FY 1992-2004. Since the Government Census does not provide the data before 1992, I add school finance data for the period FY 1990-1991 that comes from the Common Core of Data (CCD)[21]. Both the Government Census and the CCD data sets are based on common school

[21] School finance data in 1990 comes from the Local Education Agency Finance Survey of the CCD, and the data in 1991 comes from the Longitudinal Fiscal-Nonfiscal data file of the CCD.

Table 8: Description of Variables

Variable	Mean [S.D.]	Description
A. Outcome variables		
Revenue from state sources	3,608.474 [1,698.446]	Per-pupil school revenue from state sources ($)
Revenue from local sources	4,172.011 [2,635.216]	Per-pupil school revenue from local sources ($)
School revenue	8,121.956 [2,397.768]	Per-pupil total school revenue ($)
Instructional spending	4,012.640 [1,103.318]	Per-pupil instructional spending ($)
Supportive services spending	2,430.490 [819.786]	Per-pupil spending for supportive services ($)
Capital spending	863.526 [1,593.825]	Per-pupil capital spending ($)
B. Controlled variables		
Michigan reform, 1st year	0.013 [0.113]	Indicator for the first year of the Michigan school finance reform
Formula changes: court-rulings, 1st year	0.014 [0.119]	Indicator for the first year of changes to state's grant formula caused by court-rulings
Formula changes: legislative actions, 1st year	0.039 [0.194]	Indicator for the first year of changes to state's grant formula caused by legislative actions
% black students	5.222 [14.035]	Percent of enrolled black students
% Hispanic students	2.461 [5.878]	Percent of enrolled Hispanic students
% Asian students	1.156 [2.770]	Percent of enrolled Asian students
% American Indian students	0.428 [2.645]	Percent of enrolled American Indian students
Unemployment rate	5.610 [1.287]	State-level unemployment rate lagged by 6 months
Log per-capita personal income	10.313 [0.092]	State-level log per-capita personal income lagged by 6 months
Log # pupils	7.330 [1.086]	Log number of enrolled students

The unit of observation is school districts in Michigan, Illinois, Indiana, Ohio, and Pennsylvania. The study period is 1990-2004. Obs.= 42,461

finance data that are submitted by each state education agency, so that there is little threat to the consistency of our panel data between two different sources. I adjust revenues and expenditures to 2004 prices by using the Consumer Price Index.

The Public Elementary and Secondary School Universe Survey of the CCD provides school-level racial composition of enrolled students. I aggregate this data at the district level and combine it with our school finance data. Lastly, I complete the construction of our panel data set by including data on state-level unemployment rate and personal income that are obtained from the Bureau of Labor Statistics and the Bureau of Economic Analysis respectively. One problem is that these variables for economic characteristics are reported by the academic year, while school finance data is reported by the fiscal year. To deal with this inconsistency, I lagged variables for unemployment rate and personal income by one year so that these economic variables are lagged by 6-month in our panel data that varies by the fiscal year. Lagged economic variables can be more appropriate for our analysis than contemporary variables in that the school budget is planned in advance.

The Description of variables is presented in Table 8. Instructional spending includes instructor salaries as well as costs of class supplies and materials. Supportive services spending includes health and psychological services costs, administrative costs, cost of school fiscal services, costs of operation and maintenance of plant, and student transportation costs. Capital spending includes costs of construction, renovation, equipment purchase, and land purchase. Instructional, supportive services, and capital spending are exclusive each other.

Empirical Strategy

While Michigan dramatically centralized school revenue sources for our study period, neighboring states did not have not much change to their existing system as seen in Figure 1. Considering districts in Michigan as the treatment group and districts in neighboring states as the control group, we estimate the effect of the reform by using the difference-in-differences (DD) event study framework. As seen in Figure 2, however, the preexisting trend in school resources is likely different between Michigan and neighboring states, which can dispute a key assumption for unbiased DD estimates. In our empirical model, different preexisting trends between the treatment and control groups are captured by including state-specific time trends. We estimate the DD event study equation as follows:

$$Y_{sdt} = \alpha + \sum_{k=1}^{8+} \beta_k Reform_{st}^k + X'_{sdt}\gamma_1 + Z'_{st}\gamma_2 + State\text{-}effects_s$$
$$+Year\text{-}effects_t + \delta_s t + \varepsilon_{sdt} \tag{8}$$

Y_{sdt} is an outcome variable (e.g. per-pupil revenue). Subscript s indexes states, d indexes school districts, and t indexes fiscal years (1990-2004). X_{sdt} is the set of district-level controlled variables such as the racial composition of students, and Z_{st} is state-level controlled variables such as the unemployment rate. Dummies for minor changes to neighboring state's grant formulas are also included in Z_{st} (court-rulings and legislative actions).

$Reform_{st}^k$ is an independent variable of interest, which is an indicator for whether Michigan's reform becomes effective in fiscal year t. Superscript k indicates the kth year of the reform. Thus, $Reform_{st}^1$ is one if the reform becomes effective in state s in fiscal year t ($Reform_{st}^1$ is zero otherwise), and $Reform_{st}^k$ is a $(k-1)$-year lagged variable for $Reform_{st}^1$. By including the set of $Reform_{st}^k$ for $k = 1, \dots, 8$, we want to estimate the dynamic effect of the reform on school revenue and spending. $State\text{-}effects_s$ is state-fixed effects, and $Year\text{-}effects_t$ is year effects. $\delta_s t$ is state-specific time trends that captures unobserved confounders evolving over the time[22]. State-specific time trends are an essential part of this empirical model in order to account for different preexisting trends between the treatment and control groups.

My DD event study model is differentiated from the standard DD model (using a single treatment dummy) in that my DD event study model estimates the dynamic response of outcomes. My model is more suited than the standard DD method for this research since

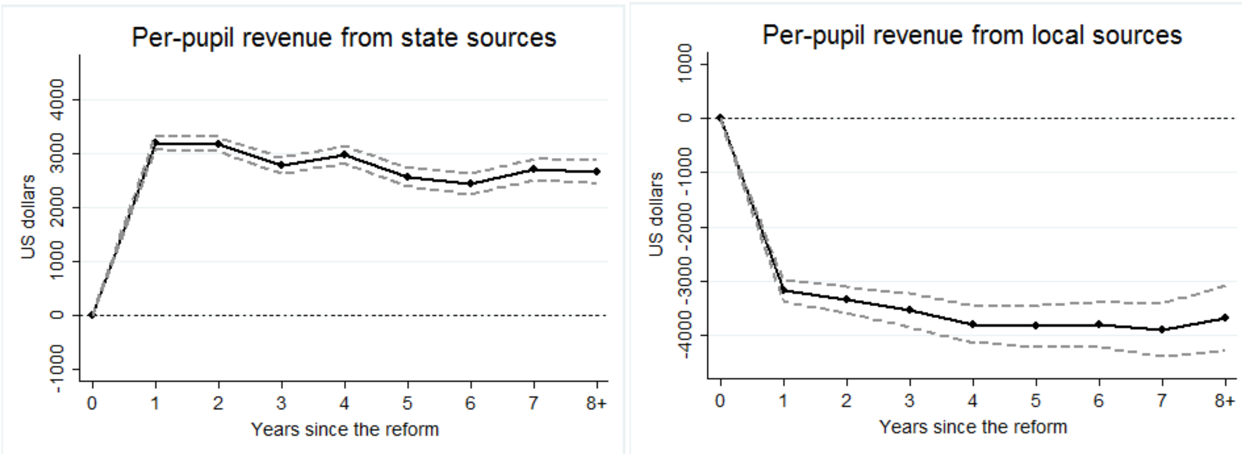
[22] It is a typical practice to account for different preexisting trends between the treatment and control groups, which has been employed by many other studies using the difference-in-differences estimator (e.g. Wen et al., 2015; Chaudhary, 2009).

Michigan's reform intended to be gradually equalizing school spending over time rather than suddenly cutting grants to richer districts and increasing grants to poorer districts all at once. When the magnitude of the treatment effect changes over time, state-specific time trends are likely to capture the dynamic effect 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 simply remedy for this problem is using the DD event study model and then estimating the dynamic effect of treatment. In this essay, standard errors are bootstrapped by school district with 500 replications.

Results

Figure 3-6 present the effect of the reform on mean per-pupil school revenue and spending by using the full sample (these estimates are also reported in Table A3 in Appendix D). I use the DD event study model to obtain estimates in these figures. Figure 3 shows that the reform increases per-pupil revenue from state sources by about $2,500-3,200 but decreases per-pupil revenue from local sources by about $3,200-$3,900, indicating that the reform substantially centralizes revenue sources. The decrease in per-pupil revenue from local sources is greater than the increase in per-pupil revenue from state sources, that could lead to the drop in per-pupil school revenue. Figure 4 shows such a drop in school revenue. The estimated effect on per-pupil revenue has a downward trend from the beginning, but its trend stabilizes after the sixth year of the reform. In the long run, the reform reduces per-pupil revenue by about $1,120.

Figure 5 presents what effect the reduction in school revenue has on current spending under the reformed system. It suggests that the reduction in school revenue leads to the reduction in school current spending. Per-pupil instructional spending appears to increase in the first three



(a) (b)

Figure 3: Effect of the Reform on Revenue Sources

I use the DD event study model with state-specific trends in order to estimate reform's effect. All district- and state-level variables are controlled in regressions.

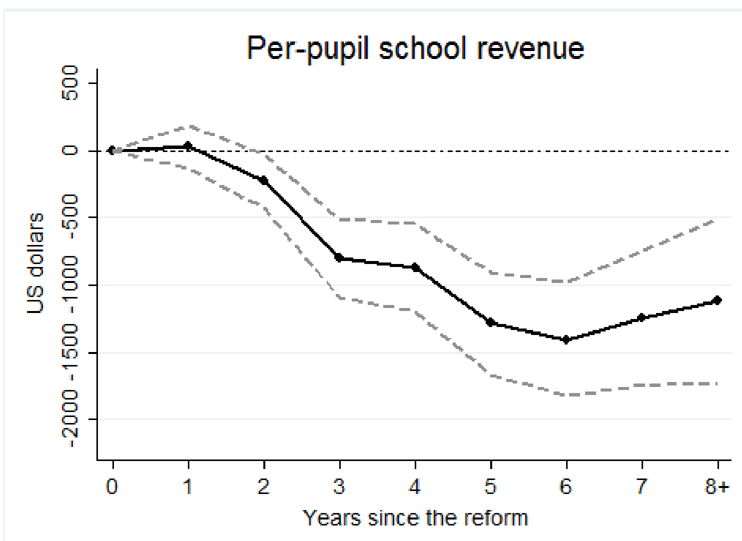
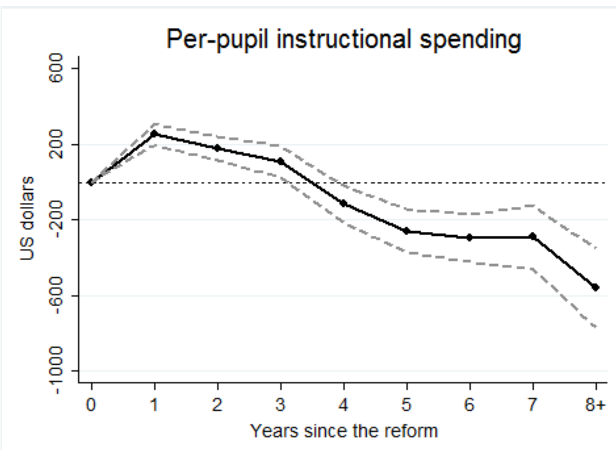


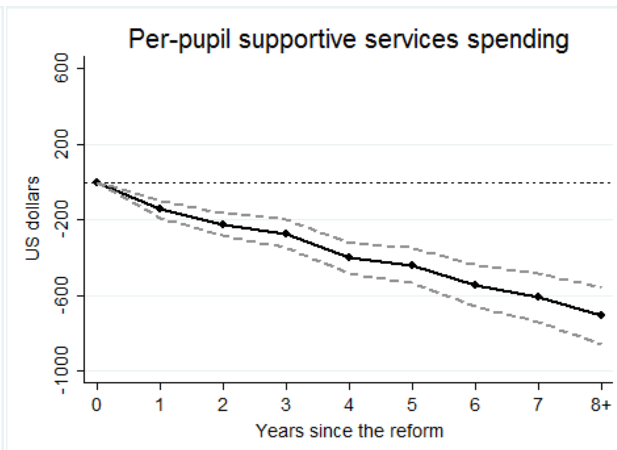
Figure 4: Effect of the Reform on School Revenue

I use the DD event study model with state-specific trends in order to estimate reform's effect. All district- and state-level variables are controlled in this regression.

year of the reform but eventually decreases in the long run. Both effects on instructional and supportive spending have downward trends, so that the negative effects become larger over the time. In the eighth year and onwards, my results suggest that the reform reduces per-pupil instructional spending by about $560 and per-pupil supportive spending by about $700. The total amount of the reduction in current spending (about $1,260) is not very different from the amount of reduction in school revenue (about $1,120).



(a)



(b)

Figure 5: Effect of the Reform on Current Spending

I use the DD event study model with state-specific trends in order to estimate reform's effect. All district- and state-level variables are controlled in regressions.

Figure 6 presents the effect of the reform on per-pupil school capital spending. In Michigan, school current spending and school capital spending are financed by different taxes at the local level. Although the reform centralizes revenue sources for school current spending, it does not change local taxes for capital spending. Thus, the reform may not affect school capital spending. As expected, I find no evidence for the effect of the reform on per-pupil capital spending. The figure shows that the effect on per-pupil capital spending has a neither upward nor downward trend.

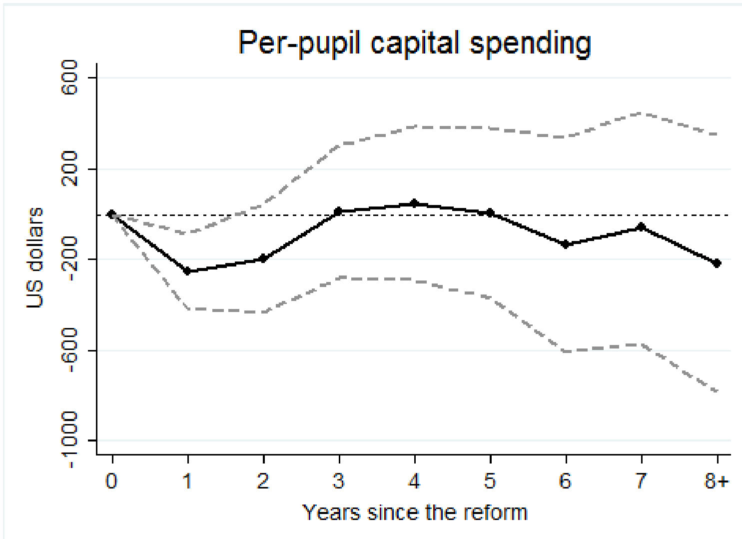


Figure 6: Effect of the Reform on Capital Spending

I use the DD event study model with state-specific trends in order to estimate reform's effect. All district- and state-level variables are controlled in this regression.

If we use the standard DD model (using a single treatment dummy) with state-specific time trends instead of the DD event study model with state-specific time trends, state-specific time trends can incorrectly capture post-reform downward trends in revenue and spending 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-specific time trends, biasing the DD estimator toward a positive value. Using districts in Illinois as the control group, Chaudhary (2009) estimates the standard DD model with state-specific time trends and concludes that Michigan's reform increases log per-pupil expenditures, but her results may not be free from the confounded state-specific time trends. In order to check whether the use of the standard DD estimator significantly changes my results, I re-estimate Figure 4-6 by using the standard DD model with state-specific time trends instead of the DD event study model and obtain very different results (reported in Table A4 in Appendix D). The estimated effect on per-pupil revenue and instructional spending are now positive (about $180 and $430 respectively) and significant. It supports the importance of using the DD event study model when the outcome variable has a dynamic response.

Considering that the reform intended to equalize school spending among districts by increasing lower-revenue district's foundation allowance faster, it would be interesting question whether the reform achieved this goal. Therefore, this essay investigates heterogeneous effects of the reform across revenue groups. I divide districts into five state-specific revenue groups based on per-pupil revenue in FY 1994; the first (quantile) revenue group is districts with mean per-pupil revenue below the 20th percentile of state-specific revenue distribution, the second revenue group is districts with mean per-pupil revenue between 20th and 40th percentile of state-specific revenue distribution, and so on. Thus, first revenue group has the lowest level of per-pupil revenue for a pre-reform period, and fifth revenue group has the highest level of per-pupil revenue. I estimate the DD event study model with revenue-group subsamples instead of the full sample[23]. Results are presented in Table 9-12.

Table 9 reports the effect of the reform on per-pupil school revenue by revenue group. Each column is a separate regression using a subsample. The effect on per-pupil revenue has downwards trends in all revenue groups. The negative effects peak in sixth year of the reform

[23] In Table A5 in Appendix D, I compare the mean of school revenue, spending, and control variables between Michigan and neighboring states by revenue group. It shows that my revenue groups are reasonably comparable between Michigan and neighboring states.

and are a bit suppressed after that. The higher-revenue group experiences a larger drop in revenue than the lower-revenue group, meaning that the reforms levels down per-pupil revenue by reducing per-pupil revenue faster in higher-revenue group. For example, in the eighth year and onwards, the reform reduces per-pupil revenue by about $560 in the bottom revenue group, about $700 in the middle-revenue group (third revenue group), and about $1,920 in the top-revenue group.

Table 9: Effect of the Reform on Per-Pupil School Revenue by Revenue Group

Year since the reform	1st revenue group (1)	2nd revenue group (2)	3rd revenue group (3)	4th revenue group (4)	5th revenue group (5)
1st year	31.605	107.828	284.005***	223.372*	-379.601
	(80.281)	(86.566)	(87.644)	(121.387)	(349.841)
2nd year	99.166	34.387	107.754	-87.535	-1,194.933***
	(106.628)	(113.760)	(104.276)	(144.500)	(409.566)
3rd year	-276.208**	-527.951***	-403.249***	-625.606***	-2,007.649***
	(109.803)	(114.169)	(122.576)	(189.941)	(692.847)
4th year	-359.613***	-516.679***	-399.712***	-711.626***	-2,185.160***
	(113.870)	(116.535)	(126.753)	(191.598)	(767.481)
5th year	-846.151***	-967.508***	-877.347***	-1,087.465***	-2,446.603***
	(133.784)	(142.498)	(161.070)	(227.438)	(896.652)
6th year	-902.032***	-1,061.258***	-986.536***	-1,175.685***	-2,669.002***
	(183.060)	(186.982)	(203.356)	(274.455)	(984.385)
7th year	-785.632***	-1,070.586***	-937.703***	-1,052.898***	-2,082.988*
	(192.900)	(202.879)	(198.677)	(291.405)	(1,237.673)
8th year +	-564.922**	-873.805***	-703.127***	-1,067.228***	-1,920.533
	(224.047)	(194.904)	(203.561)	(306.547)	(1,584.415)
Observations	8,448	8,444	8,447	8,415	8,398
R-squared	0.595	0.612	0.584	0.525	0.184

Each column is a separate regression using a different subsample. All regressions include state fixed effects, year effects, district-level and state-level covariates, and state-specific time trends. 1st revenue group is school districts with mean per-pupil revenue below 20th percentile of the state-specific revenue distribution, 2nd revenue group is school districts with mean per-pupil revenue between 20th and 40th percentile of the state-specific revenue distribution, and so on. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10 presents the effect of the reform on instructional spending per-pupil by revenue group. Except the top-revenue group, the reform appears to increase per-pupil instructional spending in the short run, but these effects eventually disappear and become negative in the long run. Similar to per-pupil revenue, the reform levels down instructional spending by reducing the spending faster in the higher-revenue group. For example, in the eighth year and onwards, the reform decreases per-pupil instructional spending by about $260 in the bottom-revenue group but about $600 in the middle-revenue group.

Table 10: Effect of the Reform on Per-Pupil Instructional Spending by Revenue Group

Year since the reform	1st revenue group (1)	2nd revenue group (2)	3rd revenue group (3)	4th revenue group (4)	5th revenue group (5)
1st year	278.175***	319.155***	248.235***	316.880***	137.826
	(30.798)	(36.528)	(42.270)	(43.888)	(105.426)
2nd year	234.121***	253.154***	179.953***	219.410***	39.411
	(40.165)	(45.082)	(47.666)	(52.994)	(146.040)
3rd year	202.332***	188.076***	121.037**	127.462**	-29.556
	(42.816)	(50.046)	(55.350)	(61.709)	(201.569)
4th year	6.028	-12.146	-69.903	-108.795	-324.190
	(57.005)	(57.030)	(61.380)	(72.238)	(244.796)
5th year	-112.029*	-144.844**	-244.273***	-238.639***	-479.319*
	(66.333)	(69.788)	(77.884)	(90.457)	(282.820)
6th year	-115.662	-162.455*	-298.890***	-240.521**	-589.219**
	(82.564)	(85.407)	(95.052)	(115.240)	(285.092)
7th year	-95.400	-211.426**	-345.356***	-276.014**	-443.701
	(90.747)	(88.417)	(101.937)	(125.057)	(431.217)
8th year +	-261.928**	-466.079***	-599.019***	-688.677***	-633.046
	(102.838)	(90.781)	(103.964)	(131.469)	(569.529)
Observations	8,448	8,444	8,447	8,415	8,398
R-squared	0.745	0.731	0.691	0.617	0.232

Each column is a separate regression using a different subsample. All regressions include state fixed effects, year effects, district-level and state-level covariates, and state-specific time trends. 1st revenue group is school districts with mean per-pupil revenue below 20th percentile of the state-specific revenue distribution, 2nd revenue group is school districts with mean per-pupil revenue between 20th and 40th percentile of the state-specific revenue distribution, and so on. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11 reports the effect of the reform on per-pupil supportive services spending by revenue group. We can see that the reform decreases per-pupil supportive services spending in all revenue groups and equalizes the spending among groups by reducing it faster in the higher-revenue group. In the bottom-revenue group, the reform decreases per-pupil support services spending by about $430. The higher-revenue group experiences larger drops in the spending (about $970 for the top-revenue group). This is a similar equalization pattern that we see in the case of revenue and instructional spending.

Table 11: Effect of the Reform on Per-Pupil Supportive Services Spending by Revenue Group

Year since the reform	1st revenue group (1)	2nd revenue group (2)	3rd revenue group (3)	4th revenue group (4)	5th revenue group (5)
1st year	-86.913***	-92.305***	-129.096***	-112.760***	-268.692***
	(30.356)	(31.763)	(34.450)	(41.662)	(89.838)
2nd year	-150.817***	-149.717***	-207.692***	-163.392***	-437.810***
	(38.460)	(35.913)	(39.189)	(49.621)	(114.137)
3rd year	-115.176**	-146.573***	-243.736***	-246.580***	-571.002***
	(45.104)	(45.135)	(45.144)	(55.062)	(151.491)
4th year	-222.418***	-263.798***	-383.980***	-411.666***	-691.679***
	(49.471)	(47.022)	(49.601)	(62.224)	(180.684)
5th year	-272.820***	-310.226***	-422.551***	-413.289***	-759.189***
	(59.816)	(54.012)	(58.342)	(74.846)	(195.518)
6th year	-344.222***	-383.542***	-535.459***	-519.046***	-922.667***
	(69.479)	(65.829)	(77.261)	(89.214)	(229.558)
7th year	-397.596***	-476.727***	-646.492***	-600.014***	-887.290***
	(77.015)	(69.734)	(80.801)	(98.164)	(276.448)
8th year +	-425.220***	-553.468***	-766.223***	-721.532***	-968.203***
	(80.632)	(74.631)	(85.664)	(100.887)	(339.209)
Observations	8,448	8,444	8,447	8,415	8,398
R-squared	0.475	0.499	0.420	0.375	0.170

Each column is a separate regression using a different subsample. All regressions include state fixed effects, year effects, district-level and state-level covariates, and state-specific time trends. 1st revenue group is school districts with mean per-pupil revenue below 20th percentile of the state-specific revenue distribution, 2nd revenue group is school districts with mean per-pupil revenue between 20th and 40th percentile of the state-specific revenue distribution, and so on. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12 shows the effect of the reform on per-pupil capital spending by revenue group. I find neither downward trends nor equalization patterns among revenue groups. It seems that the reform has negative effects on per-pupil capital spending in second revenue groups and has positive effects in the first revenue groups. Except that, the reform hardly affects per-pupil capital spending. There is little consistent pattern of the effect across revenue groups in the case of per-pupil capital spending.

Table 12: Effect of the Reform on Per-Pupil Capital Spending by Revenue Group

Year since the reform	1st revenue group (1)	2nd revenue group (2)	3rd revenue group (3)	4th revenue group (4)	5th revenue group (5)
1st year	-123.017	-421.153**	-104.081	-375.955**	-197.216
	(155.140)	(211.923)	(173.530)	(185.485)	(249.856)
2nd year	171.888	-397.384	97.989	-328.311	-462.950
	(240.794)	(299.368)	(251.508)	(261.315)	(328.625)
3rd year	397.200	-331.845	351.743	20.089	-388.290
	(278.215)	(359.430)	(298.908)	(334.182)	(426.662)
4th year	479.553	-355.215	403.483	254.620	-589.186
	(294.671)	(400.902)	(348.692)	(412.196)	(484.241)
5th year	527.752	-653.780	311.856	136.698	-326.651
	(350.929)	(437.830)	(385.531)	(440.089)	(558.547)
6th year	172.550	-985.336*	31.709	-179.155	326.551
	(411.556)	(513.985)	(476.900)	(546.474)	(768.422)
7th year	202.035	-805.869	-126.804	82.489	449.539
	(472.571)	(599.935)	(507.770)	(642.243)	(793.465)
8th year +	-57.550	-831.779	-484.863	295.555	144.262
	(518.768)	(614.169)	(508.285)	(806.144)	(843.097)
Observations	8,448	8,444	8,447	8,415	8,398
R-squared	0.080	0.067	0.071	0.056	0.041

Each column is a separate regression using a different subsample. All regressions include state fixed effects, year effects, district-level and state-level covariates, and state-specific time trends. 1st revenue group is school districts with mean per-pupil revenue below 20th percentile of the state-specific revenue distribution, 2nd revenue group is school districts with mean per-pupil revenue between 20th and 40th percentile of the state-specific revenue distribution, and so on. Standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Concluding Remarks

This essay estimates the effect of the centralization of Michigan school finance on the level of school revenue and spending. I find that the centralization decreases per-pupil revenue and current spending in all revenue groups in the long run. The per-pupil revenue and current spending are equalized among districts as they decrease faster in higher-revenue group than in lower-revenue groups. This can be the consequence of the elimination of the Tiebout-style school finance system, making the provision of public education winning less support. It can be understood in the typical framework of the underprovision of public goods as people are reluctant to support and finance public goods that benefit herself as well as others. Thus, there would be clear tradeoff between the level of spending and spending equality among districts. Especially, the elimination of local discretion on raising extra revenue is expected to result in a huge drop in the level of spending as it completely breaks the tax-benefit linkage for public education. Thus, a reasonable compromise would be changes to state's grant formula in favor of poorer districts allowing the local ability to raise extra revenue. Most school finance court-rulings belong to this category in the United States.

Chapter III: Evaluating the Fiscal Attractiveness of the Michigan School Finance Reform

Introduction

Michigan school finance reform in 1994 was one of the most dramatic school finance reforms in the United States. The reform sharply reduced local property taxes that had financed public education services. The following school district's revenue loss was offset by a large state grant which was primarily funded by 2% increase in a state general sales tax. Thus, we can say that the reform substituted the local property tax with the state sale tax. The state government set school district's minimum revenue per pupil and granted it to district governments, aiming to reduce spending inequalities among districts in Michigan.

Considering the substantial change in Michigan's fiscal policy under the reform, a natural research question that could be raised is how the reform's fiscal package is valued by households and whether it is beneficial to them. One efficient way to investigate this research question is to use the fact that local fiscal attractiveness is capitalized into housing values. Based on the Tiebout model, the value of reform's fiscal package should be (de)capitalized into housing values as households could move across districts beyond the state border to look for the best fiscal package. Thus, by investigating how the reform affects local housing values in Michigan, we can infer whether the reform brought an attractive fiscal package to households.

The issue of whether the reform is fiscally attractive to households is complicated for the following four factors. First, the reform's tax policy change (substitution of the local property tax with the state sales tax) might affect household's tax burden. It is generally known that the reform reduced net tax revenue in Michigan[24]. However, we are not sure about how the tax

[24] Office of Revenue and Tax Analysis (2002) suggests that the reduction in local property taxes was outpaced by the increase in state taxes by $17 billion over 10 years since the reform.

change was shifted beyond the state border. Both the local property tax and the state sales tax could be partially passed on non-residents[25]. Therefore, the effect of the reform on household's tax burden is ambiguous. Second, the reform might change consumer surplus through the changed level of consumption; the change in property and sales taxes might change the level of consumption of goods and housing.

Third, the reform might reduce the level of local public education services. The reform broke a tax-benefit link so that the level of public education services provided for households did not depend any more on how much tax they chose to pay[26]. This centralization of school finance might make the public provision of education less popular in wealthy communities, consequently reducing the level of public education services (Loeb, 2001; Fernandez and Rogerson, 1999; Fischel, 1996).

Fourth, the reform might have heterogeneous effects between higher- and lower-revenue districts. There is convincing evidence that the reform equalized school revenue among districts in Michigan (Chakrabarti and Roy, 2014; Roy, 2011). This revenue equalization would benefit lower-revenue districts, while it would hurt higher-revenue districts. However, it is not sure whether the reform's tax changes were favorable to lower-revenue districts as well[27]. Therefore, depending on how revenue equalization and tax changes are compared among districts, the reform would bring a greater benefit (or smaller loss) to either higher- or lower-revenue districts.

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[25] For example, the increase in the state sales tax is passed on tourists and out-of-state producers. The across-state incidence of the property tax is ambiguous. In the capital tax view, a property tax is a tax on capital and falls on the national capital owner. In the benefit tax view, a property tax is a fee for local public services and is paid by local residents.

[26] Under the system of local financing of public education, households can choose school districts that provide the best fiscal package for them. It makes local property taxes closely linked to the quality of education services. From a benefit tax view, the local property tax is considered as a fee for public education services and does not cause a deadweight loss.

[27] Sales tax is known as regressive. Property tax, meanwhile, on housing is regarded as proportional, but the local property tax structure was regressive as the tax rate is generally higher in poorer school districts (Oates and Fischel, 2016).

In sum, the reform may bring the change in household's tax burden, the change in consumption of goods and housing, the lower level of school services, and heterogeneous fiscal benefits between higher- and lower-revenue districts. This essay studies how households value the reform's effects by estimating the effect of the reform on housing values in Michigan. For empirical identification, I use the difference-in-differences (DD) method that compares school districts in Michigan with districts in neighboring states (Indiana, Ohio, and Pennsylvania) over time. Since there was no major change in fiscal policies in neighboring states for the study period, I consider these states as the control group of this research. To investigate the reform's different effect in higher- and lower- revenue districts, I divide districts into five groups within each state according to the level of pre-reform school revenue and then estimate heterogeneous effects of the reform among revenue groups.

My research is firmly based on the voluminous literature on the capitalization of fiscal variables. The literature suggests clear evidence that local taxes/expenditures are capitalized (e.g. Stadelmann and Billion, 2015; Bai et al., 2014; Rosen, 1982), educational resources are capitalized (e.g. Brunner et al., 2002; Dee, 2000), central government grants are capitalized (Hilber et al., 2011; Barrow and Rouse, 2004). Based on this literature, I expect that the value of the Michigan reform's fiscal package should be reflected by a change in housing values in the context of the Tiebout model in which households could choose residential communities beyond the state border. Regarding the school finance reforms of U.S. states, there is the extensive literature on the effect of the reforms on school revenue/spending and education outcomes (e.g. Jackson et al., 2016; Sims, 2011; Papke, 2005; Card and Payne, 2002). Nevertheless, there is little literature studying whether these reforms are fiscally attractive to households beyond its effect on school resources and education outcomes. This present essay fills this gap by investigating how the reform's fiscal policy change is valued by households in Michigan.

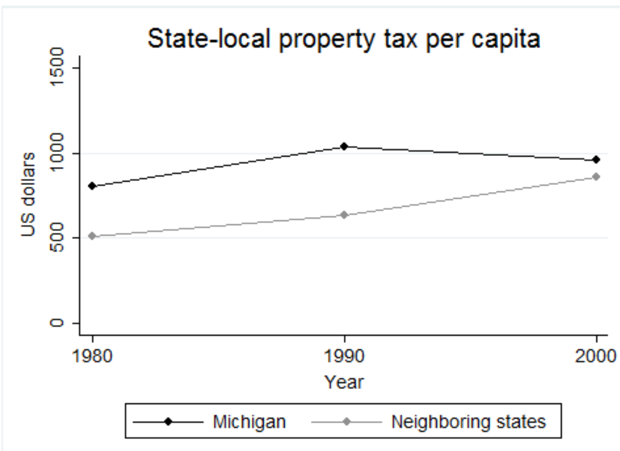
Michigan School Finance Reform

The state of Michigan was one of states with the heaviest property tax burden before the school finance reform in the fiscal year 1995[28]. The public demand for the lower property tax burden provoked the debate on the school financing reform and eventually ended up a state referendum in which the reform was passed in 1994[29]. The reform removed a local school operating tax on homestead properties (e.g., owner-occupied housing) and required the local school operating tax on nonhomestead properties (e.g., commercial buildings) to drop to 18 mills. The statewide property tax of 6 mills was introduced, but this new property tax revenue could not sufficiently cancel out the revenue loss when considering that the average local school operating tax rate was about 34 mills before the reform[30]. The reform was designed to offset the revenue loss primarily by the 2% increase in a state general sales tax. The reform also increased a cigarette excise tax by 50 cents per pack and introduced a new sales tax of 16% on tobacco products other than cigarettes.

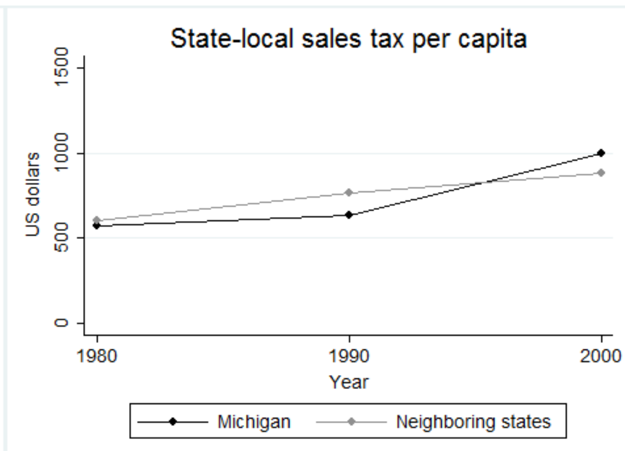
Figure 7 shows trends in state-local tax revenue per capita for the period of 1980-2000. The black line indicates the trend in Michigan tax revenue, and the gray line indicates the trend in neighboring states (Indiana, Ohio, and Pennsylvania) tax revenue. We can clearly see that there were marked changes in the tax trend between 1990 and 2000. It seems that the reform

[28] In 1993, property taxes accounted for 4.6% of personal income on average in Michigan, which was the 7th highest share among 50 states (Office of Revenue and Tax Analysis, 2002).
[29] The reform was passed through a state referendum by 69 % to 39 % in 1994. The reform became has been in effect since the fiscal year 1995.
[30] See Office of Revenue and Tax Analysis (2002), p6.

decreased property tax revenue but increased sales tax revenue as expected. Comparing tax revenue trends between Michigan and neighboring states, state-local property tax revenue per capita relatively decreased in Michigan by $307.0 between 1990 and 2000. On the other hand, state-local sales tax revenue per capita relatively increased in Michigan by $249.3 for the same period.



(a)



(b)

Figure 7: Trends in State-Local Property and Sales Taxes per Capita

In these figures, the year refers to the fiscal year. Dollar amounts are adjusted to values in 2000. Tax values for neighboring states are the average of state-level property/sale taxes per capita over states in the control group.

To investigate a question of whether the reform's tax policy change was revenue-neutral, I create the counterfactual trend in state-local total tax revenue per capita of Michigan by using the synthetic control method (Abadie et al., 2010). I compute state-level weights given to neighboring states, by which the weighted average outcome and predictor variables of neighboring states are best matched with the outcome and predictor variables of Michigan for the pre-reform period[31]. Then, synthetic Michigan is created by computing the weighted average of state-local tax revenue of neighboring states. In implementing this method, variables are indexed to 1 in 1980. Figure 8 shows trends in state-local total tax revenue of Michigan and the synthetic group. As clearly seen, Michigan's tax revenue increased slower than the synthetic group between 1990 and 2000, which can be attributed to the effect of the reform. My estimates imply that Michigan had lower state-local tax revenue than the synthetic group by 8.2% in 2000.

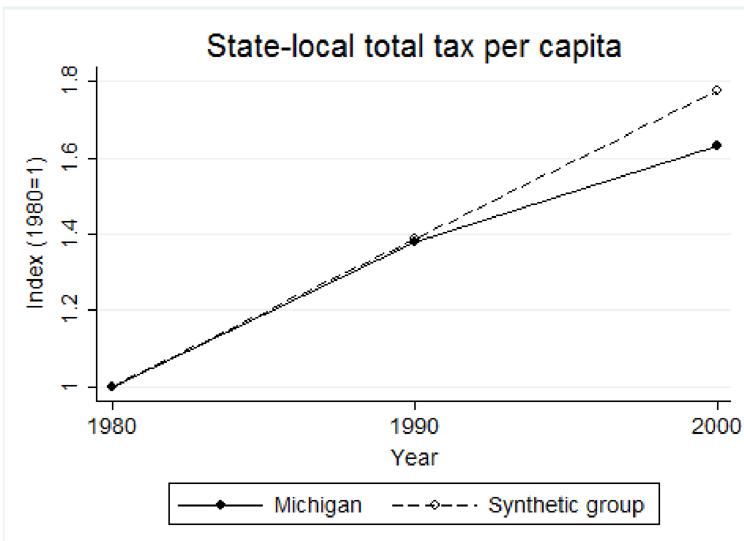


Figure 8: Trends in State-Local Tax per Capita; Using the Synthetic Control Method

As explained earlier, a question of how the fiscal package of the reform are valued by Michigan households could be answered by investigating the change in housing values. Figure 9 presents trends in the average of district-level median housing values for the period of 1980-

[31] The outcome variable is state-local tax revenue per-capita. For predictor variables. I use state-level GDP per capita and the state-level unemployment rate.

1990. It seems that housing values in Michigan substantially outpaced housing values in neighboring states between 1990 and 2000. I consider that the reform caused this increase in housing values in Michigan.

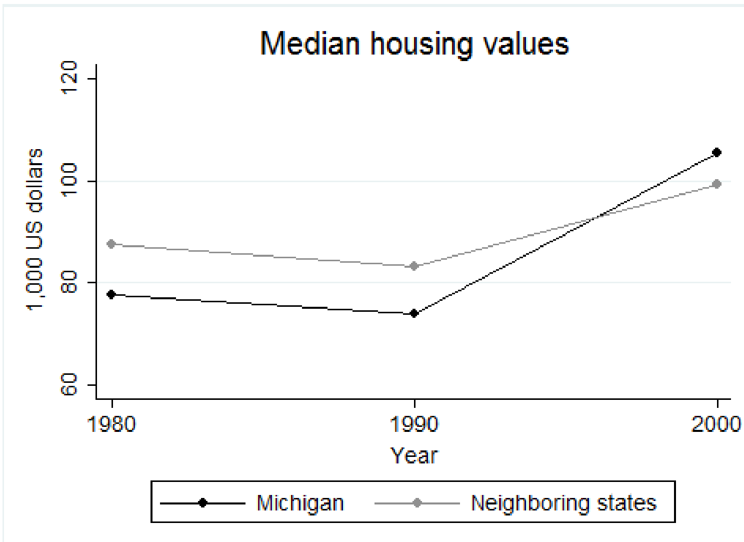


Figure 9: Trends in Median Housing Values

In this figure, the year refers to the fiscal year. Dollar amounts are adjusted to values in 2000. Median housing values for neighboring states are the average of district-level median housing values over districts in the control group.

The reform centralized the school finance system by giving the state government extra tax revenue as well as the authority to allocate this tax revenue to school districts. The amount of the new state grant, called the foundation allowance, initially depended on the pre-reform revenue level in each school district, so that school districts with higher revenue received larger grants at first. However, the state government gradually equalized the grants among districts, resulting in decreased school revenue inequalities over time in Michigan. How the revenue equalization was compared to the incidence of tax changes can determine whether the reform's fiscal package was favorable to higher-revenue districts. Since it is clear whether the reform brings a greater benefit (or smaller loss) to higher- or lower- revenue districts, it is an empirical question to be answered in this essay.

Table 13: Description of Variables

Variables	Mean (S.D.)	Description
	Panel A: Outcome variables	
Median housing values	88,814.072 (36,133.013)	Median owner-occupied housing values ($)
Per-pupil local property tax	2,511.555 (1,875.914)	Local property tax revenue per pupil ($)
Per-pupil school revenue	6,250.311 (2,318.950)	School district's revenue per pupil ($)
	Panel B: Control variables	
Reform	0.092 (0.289)	Indicator for Michigan reform in effect
Blacks	2.871 (8.027)	Percent of black residents
American Indians	0.370 (1.199)	Percent of American Indian residents
Asians	0.531 (0.874)	Percent of Asian residents
Other races	0.907 (1.633)	Percent of other races residents
College graduates	14.086 (9.906)	Percent of adults (age≥25) with college degrees or above
College experiences	19.752 (7.732)	Percent of adults (age≥25) with some college-level training
HS graduates	41.303 (7.893)	Percent of adults (age≥25) with high school degrees
Log # pupil	7.616 (0.940)	Log number of enrolled students
Unemployment rate	5.450 (1.376)	State-level unemployment rate
Log per-capita personal income	10.046 (0.159)	Log state-level per-capita personal income
Single-family homes	86.209 (9.395)	Percent of owner-occupied single-family homes
Townhomes	3.189 (8.135)	Percent of owner-occupied townhomes
Mobile homes	8.354 (6.752)	Percent of owner-occupied mobile homes, boats, etc.
Homes aged 0-5	9.406 (6.093)	Percent of owner-occupied homes built 5 years ago or after
Homes aged 5-10	8.352 (5.078)	Percent of owner-occupied homes built 10 years ago or after
Homes aged 10-20	16.302 (8.041)	Percent of owner-occupied homes built 20 years ago or after

The sample includes school districts in Michigan, Indiana, Ohio, and Pennsylvania. The study years are 1980, 1990, and 2000. Dollar amounts are adjusted to values in 2000. Obs.=5,607

Data

I obtain data on district-level characteristics from the Decennial U.S. Census in 1980, 1990, and 2000. The Decennial Census provides variables for housing characteristics such as median housing values and the age of homes and variables for population characteristics such as racial composition and education attainments. I combine this Census data set with the Annual Survey of Governments data (1980 and 1990) and the Public Elementary-Secondary Education Finances data of the U.S. Census Bureau (2000) that provide government finance statistics such as property tax revenue, sales tax revenue, and expenditure by category. The Annual Survey of Governments was discontinued in 1992 and was replaced by the Public Elementary-Secondary Education Finances for district-level finance statistics.

Therefore, this essay uses decennial panel data for the period of 1980-2000. The sample includes school districts in Michigan, Indiana, Ohio, and Pennsylvania. The panel data is strongly balanced with 1,869 school districts and 5,607 observations. Michigan has 514 districts, and neighboring states have 1,355 districts. The description of all variables is presented in Table 13.

Empirical Strategy

To estimate the effect of the reform on housing values and fiscal variables in Michigan, I consider districts in neighboring states (Indiana, Ohio, and Pennsylvania) as the control group as these states did not have major fiscal policy changes for the study period. Furthermore, they are geographically closer to Michigan so that unobserved factors may not evolve very differently between Michigan and those neighboring states over time. Nevertheless, we cannot completely exclude the possibility of different preexisting trends in the unobserved that is correlated with the passage of the reform, which would bias my estimates. To deal with this issue, I control for pre-reform time trends by taking advantage of the panel structure of my data. In this essay, I use the difference-in-differences (DD) method to estimate the effect of the reform as follows:

$$\begin{gathered} Y_{sdt} = \alpha + \beta Reform_{st} + X'_{sdt}\gamma_1 + Z'_{st}\gamma_2 + County\text{-}effects_{sd} \\ +Year\text{-}effects_t + \delta_s t + \varepsilon_{sdt} \end{gathered} \tag{9}$$

Subscripts s, d, and t indicate states, school districts, and years respectively. Y_{sdt} is an outcome variable (median housing values, or local property tax per pupil, or school district's revenue per pupil). $Reform_{st}$ is a variable of interest, which is one if the reform is in effect (year of 2000) in Michigan and zero otherwise. β represents the treatment effect of the reform on the outcome variable. In the above equation, both county fixed effects and year effects are included, which are an essential part of the DD method. I also include state-specific time trends $\delta_s t$ in the above equation. It allows different preexisting trends in the unobserved factors, that are correlated with $Reform_{st}$, between the treatment and control groups, assuming that the unobserved factors evolve at constant rates for the study period. The DD method with this specification is well known and is frequently used in the literature (e.g. Chakrabarti and Roy, 2015; Wen et al., 2015; Chu, 2014). X_{sdt} is district-level control variables such as racial composition and educational attainments, and Z_{st} is state-level control variables including log per-capita personal income and unemployment rate. I bootstrap standard errors with 1,000 replications at the district level.

Results

Table 14 reports the effect of the reform on per-pupil local property tax revenue and per-pupil school revenue. Column (1) and (2) show the effect on per-pupil local property tax, and the specification is with and without state-specific time trends. We can see that the estimated effects are not greatly affected by control for state-specific time trends, supporting that the difference in preexisting trends in unobserved factors is not very significant. With controlling state-specific time trends, my results say that the reform reduces per-pupil local property tax revenue by $3,089.9. As discussed in the above, the decreased local property tax revenue is not likely to be fully canceled out by an increase in other tax revenue and result in lower state-local total tax revenue in Michigan.

The lower tax revenue should lead to the reduction in state-local expenditure in Michigan. I expect that school district's revenue is an expenditure category with the reduction. In column (3) and (4), I estimate the effect of the reform on per-pupil district's revenue. My results show that the reform decreases the per-pupil revenue by $1257.3 in column (4) with controlling for state-specific time trends. The estimated effects are not sensitive to whether state-specific time trends are controlled. The negative effect on the school revenue is consistent with the existing evidence that the centralization of school finance decreases the level of school resources (Hoxby, 2001; Silva and Sonstelie, 1995).

Table 14: Effect of the Reform on Local Property Taxes and School Revenue

	Per-pupil local property taxes ($)		Per-pupil school revenue ($)	
	(1)	(2)	(3)	(4)
Reform	-2,795.879***	-3,089.917***	-1,237.495***	-1,257.265***
	(131.762)	(293.719)	(164.457)	(400.642)
State-specific time trends	N	Y	N	Y
Observations	5,607	5,607	5,607	5,607
R-squared	0.593	0.598	0.651	0.655

Regressions include county-fixed effects and year effects. Additional controls include racial composition, educational attainments, log number of enrolled students, state-level unemployment rate, and state-level log per-capita personal income. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Effect of the Reform on Median Housing Values

	Median housing values ($)			
	(1)	(2)	(3)	(4)
Reform	15,981.426***	12,910.608***	16,122.079***	13,099.406***
	(852.784)	(1,317.026)	(793.635)	(1,400.973)
Housing controls	N	N	Y	Y
State-specific time trends	N	Y	N	Y
Observations	5,607	5,607	5,607	5,607
R-squared	0.833	0.835	0.871	0.872

Regressions include county-fixed effects and year effects. Additional controls include racial composition, educational attainments, and log number of enrolled students. Housing controls include percent of single-family homes, percent of townhomes, and percent of mobile homes, and variables for the age of structures. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 15 presents the effect of the reform on median housing values. I include housing controls such as housing type (e.g. percent of single-family homes) and the age of homes in column (3) and (4) in order to block a casual channel through the change in housing quality, while I exclude them in column (1) and (2). With controlling for housing characteristics as well as state-specific time trends, I find that the reform increases median housing values by $13,099.4 in column (4). This effect on housing values can be interpreted as the capitalization of the reform's fiscal effects. The changed tax burden and consumption along with the lower level of school services turn out to give fiscal benefits to households, and that leads to an increase in housing values under the circumstance that households are able to move to other districts beyond the state border to look for more attractive fiscal packages. The table shows that the estimated effects are pretty robust to housing controls as well as state-specific time trends, so that my results for median housing values are not significantly driven by either different time trends among states or a change in housing quality.

To investigate the heterogeneous effect of the reform between higher- and lower-revenue districts, I divide school districts into five groups by the pre-reform (year of 1994) level of per-pupil school district's revenue within each state. Thus, 1st revenue group consists of school districts with pre-reform per-pupil school revenue below 20th percentile of the revenue distribution within each state; 5th revenue group consists of school districts with pre-reform per-pupil school revenue above 80th percentile of the revenue distribution within each state. Since the level of district's revenue is strongly correlated with district's property wealth, a higher revenue group would have higher property wealth.

Table 16: Effect of the Reform on Local Property Taxes and School Revenue by Revenue Group

	Per-pupil local property taxes ($)		Per-pupil school revenue ($)	
Samples	(1)	(2)	(3)	(4)
1st revenue group	-1,868.510***	-2,086.059***	-867.856***	-404.538
	(126.397)	(144.334)	(280.578)	(359.774)
2nd revenue group	-2,165.106***	-2,423.118***	-1,247.156***	-1,038.069**
	(127.408)	(162.768)	(269.940)	(388.163)
3rd revenue group	-2,598.684***	-2,856.055***	-1,227.509***	-1,223.621***
	(144.217)	(182.187)	(230.544)	(356.885)
4th revenue group	-3,365.204***	-3,214.840***	-1,372.764***	-905.467**
	(209.158)	(270.565)	(236.217)	(362.241)
5th revenue group	-3,319.055***	-4,434.186***	-1,257.777**	-2,215.145
	(509.362)	(1,325.346)	(605.127)	(1,565.657)
State-specific time trends	N	Y	N	Y

A different revenue-group subsample is used in each row. Regressions include county-fixed effects and year effects. Additional controls include racial composition, educational attainments, log number of enrolled students, state-level unemployment rate, and state-level log per-capita personal income. Standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 16 reports the effect of the reform, by revenue group, on local property taxes in column (1)-(2) and on school district's revenue in column (3)-(4). My regression compares the trend in 1st revenue group in Michigan with the trend in 1st revenue group in neighboring states, compares the trend in 2nd revenue group in Michigan with the trend in 2nd revenue group in neighboring states, and so on. A different revenue-group subsample is used in each row. I find that the reform reduces the property tax revenue of districts with higher pre-reform school revenue by a larger amount. It is obvious results in that districts with higher pre-reform revenue has greater property wealth. I also find that the reform equalizes per-pupil school revenue through leveling-down.

It is difficult to have the complete understanding of the heterogeneous effects of the reform between higher- and lower-revenue districts only from Table 16 since we do not know how these estimated effects are compared in terms of household's fiscal benefits. To fully investigate the heterogeneous effects, I estimate the effect of the reform on median housing values by revenue group in Table 17. The specification is with and without housing controls and state-specific time trends. I find that the reform has greater positive effect on housing values in districts with higher pre-reform revenue. For example, the reform increases median housing values in the bottom revenue group by $8,706.8, in the middle revenue group by $12,327.7, and in the top revenue group by $23,130.7. All these estimated effects are significant at 1% level. These estimates indicate that reform brings benefits to all revenue groups but brings greater benefits to revenue groups with higher property wealth. It may be because the tax policy changes were favorable to wealthier households, and revenue equalization effect is exceeded by the tax policy changes. The decrease in school revenue may not be a great disadvantage for wealthier households since they less rely on public education. In Appendix E, Table A7 presents the annual amount of capitalization by using four different discount factors.

Table 17: Effect of Reform on Median Housing Values by Revenue Group

	Median housing values ($)			
Samples	(1)	(2)	(3)	(4)
1st revenue group	10,401.582*** (1,559.368)	8,277.984*** (1,913.952)	10,630.694*** (1,451.031)	8,706.847*** (1,805.154)
2nd revenue group	13,027.436*** (1,534.747)	8,246.040*** (1,968.778)	13,725.652*** (1,487.604)	8,714.690*** (1,808.676)
3rd revenue group	15,514.788*** (1,517.829)	11,874.060*** (2,189.764)	14,029.590*** (1,342.800)	12,327.696*** (2,067.609)
4th revenue group	17,586.281*** (1,558.784)	13,299.315*** (2,423.997)	16,790.650*** (1,534.747)	13,419.203*** (2,347.033)
5th revenue group	19,498.802*** (2,447.247)	22,048.985*** (5,006.853)	21,957.990*** (2,260.674)	23,130.744*** (4,560.265)
Housing controls	N	N	Y	Y
State-specific time trends	N	Y	N	Y

A different revenue-group subsample is used in each row. Regressions include county-fixed effects and year effects. Additional controls include racial composition, educational attainments, and log number of enrolled students. Housing controls include percent of single-family homes, percent of townhomes, and percent of mobile homes, and variables for the age of structures. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To answer the question of how the effect of the reform on housing values varies according to other characteristics than the pre-reform level of school revenue, I divide districts into five quantile groups by (a) percent of students enrolled in public school out of total population, (b) housing vacancy rate, and (c) median household income. The results are reported in Table 18. I find that the reform tends to increase median housing values in districts with the lower percent of enrolled students by a larger amount. This may be because districts with fewer enrolled students are less hurt by the reduction in school revenue, giving higher fiscal benefits to such districts. I also find that the reform generally has a greater positive effect on median housing values in districts with a lower housing vacancy rate. With vacant housing, upward shift in demand for housing may reduce housing surplus instead of increasing housing values so that districts with a higher vacancy rate may experience a smaller increase in housing values.

Table 18: Effect of the Reform on Median Housing Values by Percent of Enrolled Students, Housing Vacancy Rate, and Median Household Income

	Median housing values ($)		
Samples	Grouped by % enrolled students (1)	Grouped by housing vacancy rate (2)	Grouped by median household income (3)
1st quantile group	17,163.798***	18,332.647***	4,987.279***
	(4,160.512)	(2,949.876)	(1,920.454)
2nd quantile group	17,824.575***	16,701.031***	4,981.469**
	(2,707.819)	(3,230.244)	(1,940.727)
3rd quantile group	9,458.959***	6,498.980**	7,518.571***
	(2,338.053)	(2,737.991)	(1,833.178)
4th quantile group	12,188.519***	9,035.019***	17,817.859***
	(2,233.932)	(1,944.033)	(2,579.340)
5th quantile group	10,649.910***	8,580.656***	27,990.596***
	(2,018.863)	(2,725.709)	(3,845.776)

A different subsample is used in each row. Regressions include county-fixed effects, year effects, and state-specific time trends. Additional controls include racial composition, educational attainments, log number of enrolled students, and housing controls. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Concluding Remarks

In this essay, I study how the reform's fiscal policy change is valued by households in Michigan. I find that the reform increases median housing values and that the increase is greater in districts with higher pre-reform revenue. It can be interpreted that the reform brings a beneficial fiscal package to households on average in Michigan and that wealthier households receive greater fiscal benefits from the reform.

Appendix A: Formulas for Additive Correction Terms in Chapter I

In this appendix, I want to present the formula for additive correction terms discussed in Chapter I. These correction terms are suggested by Poirier (1980) and Ham (1982). Every notation for variables used here is same as notations used throughout this chapter. Let define $X_{1i}^* = X_i$, $X_{2i}^* = (Qscb1_i\ Pt1_i\ X_i')'$, $\alpha_1^* = \delta_1$, and $\alpha_2^* = (\alpha_1\ \alpha_2\ \delta_2')'$. Then,

$$\lambda 1_i = \frac{\phi(X_{1i}^*\alpha_1^*)\Phi\left((X_{2i}^*\alpha_2^* - \rho X_{1i}^*\alpha_1^*)/(1-\rho^2)^{\frac{1}{2}}\right)}{F(X_{1i}^*\alpha_1^*, X_{2i}^*\alpha_2^*; \rho)} \qquad \text{(A1)}$$

$$\lambda 2_i = \frac{\phi(X_{2i}^*\alpha_2^*)\Phi\left((X_{1i}^*\alpha_1^* - \rho X_{2i}^*\alpha_2^*)/(1-\rho^2)^{\frac{1}{2}}\right)}{F(X_{1i}^*\alpha_1^*, X_{2i}^*\alpha_2^*; \rho)} \qquad \text{(A2)}$$

where $\phi(.)$ is the normal density function, $\Phi(.)$ is the normal distribution function, and $F(.)$ is the bivariate normal distribution function.

Appendix B: Consistent Variance-Covariance Matrix in Chapter I

In this appendix, I want to explain how to compute the consistent variance-covariance matrix under double sample selection in Chapter I. This variance-covariance matrix was suggested by Lee, Maddala, and Trost (1980) in the case of single sample selection and was generalized by Ham (1982) in the case of double sample selection. Every notation for variables used here is same as notations used throughout this chapter. Let $\mu = (\delta_1' \; \alpha_1 \; \alpha_2 \; \delta_2' \; \rho)'$ be a $L \times 1$ vector of parameters from the recursive bivariate probit model (4) and (5). Then, the difference between a true selection correction term and an estimated selection correction term is approximated by the first-order Taylor series with respect to μ such that

$$\lambda 1_i - \widehat{\lambda 1}_i = \frac{\partial \lambda 1_i{}'}{\partial \mu} (\mu - \hat{\mu}) \tag{A3}$$

$$\lambda 2_i - \widehat{\lambda 2}_i = \frac{\partial \lambda 2_i{}'}{\partial \mu} (\mu - \hat{\mu}) \tag{A4}$$

Let define $C_i = \sigma_{\upsilon 1} \frac{\partial \lambda 1_i{}'}{\partial \mu} + \sigma_{\upsilon 2} \frac{\partial \lambda 2_i{}'}{\partial \mu}$. Then, $C = (C_1 \; C_2 \; ... \; C_{N-1} \; C_N)'$ is a $N \times L$ matrix. Let $X^* = \left(Qscb1 \; X \; \widehat{\lambda 1} \; \widehat{\lambda 2}\right)$ be a $N \times K$ matrix of variables, and let $\beta^* = (\gamma \; \beta' \; \sigma_{\upsilon 1} \; \sigma_{\upsilon 2})'$ be a $K \times 1$ matrix of coefficients. Then,

$$\beta^* - \widehat{\beta^*} \overset{d}{=} \left(X^{*\prime} X^*\right)^{-1} X^{*\prime} \left(\varepsilon + C(\mu - \hat{\mu})\right) \tag{A5}$$

where ε is a vector of error terms from housing market outcome equation (5). In estimating the variance-covariance matrix, we can ignore covariance between ε and $C(\mu - \hat{\mu})$. Then, the variance-covariance matrix is

$$Var\left(\widehat{\beta^*}\right) = \left(X^{*\prime} X^*\right)^{-1} X^{*\prime} (\varepsilon \varepsilon' + C(\mu - \hat{\mu})(\mu - \hat{\mu})' C') X^* \left(X^{*\prime} X^*\right)^{-1} \tag{A6}$$

In this chapter, I compute the following variance-covariance matrix.

$$\widehat{Var(\hat{\beta}^*)} = \left(X^{*\prime}X^*\right)^{-1}X^{*\prime}\left(diag(e_i^2) + C\left(\widehat{Var(\hat{\mu})}\right)C'\right)X^*\left(X^{*\prime}X^*\right)^{-1} \tag{A7}$$

where $diag(.)$ is a diagonal matrix, e_i is an estimated error term, and $\widehat{Var(\hat{\mu})}$ is the estimated variance-covariance matrix of $\hat{\mu}$.

Appendix C: Additional Tables for Chapter I

Table A1: Effect of Winning the QSCB Lottery on Housing Market and Household Sorting Outcomes; Basic OLS Regression with Single Sample Selection

VARIABLES	%*Δ* median housing value (1)	*Δ* housing vacancy rate (%) (2)	%*Δ* households with own children (3)	%*Δ* households without own children (4)
QSCB lottery	3.642**	-0.013	1.433	1.427
	(1.499)	(0.501)	(3.189)	(2.204)
Participation in the 2nd round	1.088	-0.194	1.397	1.277
	(1.326)	(0.432)	(2.604)	(2.257)
Observations	214	214	214	214
R-squared	0.748	0.591	0.472	0.418

All specifications include economic controls, demographic controls, district controls, housing market controls, and county dummies. For the detail of these covariates, please refer to descriptive statistics in table 2. Robust standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table A2: Effect of Winning the QSCB Lottery on Housing Market and Household Sorting Outcomes; not Controlling for Correction Terms with Double Sample Selection

VARIABLES	%*Δ* median housing value (1)	*Δ* housing vacancy rate (%) (4)	%*Δ* households with own children (2)	%*Δ* households without own children (3)
QSCB lottery	5.708**	-0.244	-0.464	1.252
	(2.332)	(0.700)	(4.868)	(3.354)
Observations	140	140	140	140
R-squared	0.759	0.712	0.549	0.515

All specifications include economic controls, demographic controls, district controls, housing market controls, and county dummies. For the detail of these covariates, please refer to descriptive statistics in table 2. Robust standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Appendix D: Additional Tables for Chapter II

Table A3: Effect of the Reform on Revenue and Spending; Full Sample

Year since the reform	Per-pupil school revenue (1)	Per-pupil instructional spending (2)	Per-pupil supportive spending (3)	Per-pupil capital spending (4)
1st year	29.642	250.961***	-143.827***	-252.810***
	(76.741)	(30.109)	(22.987)	(86.235)
2nd year	-223.536**	179.047***	-223.347***	-195.270
	(98.558)	(34.234)	(29.249)	(125.509)
3rd year	-799.048***	108.108**	-271.331***	9.073
	(152.054)	(43.182)	(37.095)	(156.383)
4th year	-871.472***	-116.724**	-401.622***	46.580
	(164.929)	(51.180)	(42.670)	(178.461)
5th year	-1,282.723***	-257.191***	-440.560***	3.761
	(193.710)	(59.695)	(48.662)	(197.602)
6th year	-1,403.552***	-297.181***	-546.661***	-134.214
	(215.767)	(66.338)	(56.868)	(241.056)
7th year	-1,244.588***	-291.330***	-610.394***	-60.784
	(260.310)	(87.818)	(65.906)	(278.640)
8th year +	-1,121.788***	-560.621***	-707.352***	-216.971
	(317.867)	(110.619)	(78.110)	(305.617)
Observations	42,461	42,461	42,461	42,461
R-squared	0.331	0.402	0.256	0.049

Each column is a separate regression using a different subsample. All regressions include state fixed effects, year effects, district-level and state-level covariates, and state-specific time trends. Standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table A4: Effect of the Reform on School Revenue and Spending; Standard DD Method with State-Specific Time Trends

	Per-pupil school revenue (1)	Per-pupil instructional spending (2)	Per-pupil supportive spending (3)	Per-pupil capital spending (4)
Michigan's reform	183.451**	426.101***	-15.546	-9.972
	(75.496)	(34.326)	(21.740)	(92.038)
Formula changes:	337.520***	-97.021***	-62.649***	-78.823
court-rulings	(58.497)	(17.210)	(12.076)	(74.922)
Formula changes:	497.939***	133.271***	50.692***	254.234***
legislative actions	(31.063)	(11.974)	(9.466)	(43.300)
% black students	28.484***	12.062***	13.263***	-3.656***
	(2.477)	(1.152)	(0.973)	(0.738)
% Hispanic students	9.877	5.738**	0.921	-0.751
	(6.464)	(2.750)	(2.332)	(1.654)
% Asian students	272.811***	120.945***	93.255***	38.328***
	(27.182)	(10.874)	(10.302)	(5.946)
% American Indian	35.752***	19.500**	9.946**	9.312
students	(13.803)	(7.602)	(4.321)	(5.894)
Unemployment	-349.599***	-45.825***	-60.990***	-173.485***
rate	(25.705)	(11.571)	(6.693)	(29.597)
Log per-capita personal	-12.582***	-2.004***	-0.299	-4.925***
income*1,000	(1.206)	(0.377)	(0.253)	(1.529)
Log # pupils	-232.271***	-76.036*	-57.628**	32.427**
	(76.445)	(39.391)	(23.369)	(16.137)
Observations	42,461	42,461	42,461	42,461
R-squared	0.329	0.401	0.255	0.048

All regressions include state fixed effects, year effects, and state-specific time trends. Standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Mean of School Revenue, Spending, Racial Groups, and the Number of Pupils for the Pre-Reform Period

	1st revenue group (1)	2nd revenue group (2)	3rd revenue group (3)	4th revenue group (4)	5th revenue group (5)	Full sample (6)
			A. Mean of per-pupil school revenue			
Michigan	5,769.250	6,267.285	6,701.845	7,572.193	10,110.669	7,280.982
Neighboring states	5,680.391	6,100.287	6,459.931	7,160.968	9,812.949	7,042.356
			B. Mean of per-pupil current spending			
Michigan	5,178.654	5,592.196	5,931.695	6,583.116	8,323.280	6,316.234
Neighboring states	4,967.229	5,290.085	5,575.065	6,115.197	8,213.819	6,032.535
			C. Mean of per-pupil capital spending			
Michigan	465.361	489.879	593.169	643.499	912.578	615.472
Neighboring states	353.583	441.889	454.068	578.185	885.501	541.725
			D. Mean of % black students			
Michigan	1.148	1.095	1.357	7.140	9.640	4.279
Neighboring states	1.213	2.134	3.446	7.161	9.057	4.587
			E. Mean of % hispanic students			
Michigan	2.067	1.920	2.174	2.400	2.302	2.186
Neighboring states	0.770	1.243	1.904	2.358	2.792	1.808
			F. Mean of % Asian students			
Michigan	0.392	0.503	0.570	0.923	1.581	0.785
Neighboring states	0.354	0.507	0.678	1.023	3.026	1.120
			G. Mean of % American indian students			
Michigan	1.129	1.114	1.569	1.867	2.074	1.672
Neighboring states	0.069	0.070	0.079	0.077	0.109	0.081
			H. Mean of # pupils			
Michigan	1,503.657	1,832.454	2,207.031	5,022.660	4,203.722	2,921.112
Neighboring states	1,710.191	1,783.799	2,096.067	3,333.645	4,622.718	2,701.753

Appendix E: Additional Tables for Chapter III

Table A6: Effect of the Reform by Revenue Group; Using Log of Outcome Variables

	Log Per-pupil local property taxes		Log Per-pupil school revenue		Log Median housing values	
Samples	(1)	(2)	(3)	(4)	(5)	(6)
Full sample	-1.364***	-1.180***	-0.206***	-0.154***	0.159***	0.176***
	(0.040)	(0.042)	(0.019)	(0.027)	(0.007)	(0.014)
1st revenue group	-1.294***	-1.183***	-0.164***	-0.072	0.141***	0.171***
	(0.082)	(0.090)	(0.041)	(0.052)	(0.015)	(0.022)
2nd revenue group	-1.446***	-1.284***	-0.193***	-0.138**	0.155***	0.147***
	(0.082)	(0.079)	(0.040)	(0.050)	(0.012)	(0.020)
3rd revenue group	-1.374***	-1.311***	-0.207***	-0.189***	0.142***	0.142***
	(0.085)	(0.087)	(0.036)	(0.049)	(0.014)	(0.023)
4th revenue group	-1.404***	-1.143***	-0.228***	-0.145***	0.157***	0.156***
	(0.086)	(0.093)	(0.038)	(0.048)	(0.015)	(0.027)
5th revenue group	-0.906***	-0.909***	-0.202***	-0.197**	0.191***	0.227***
	(0.092)	(0.106)	(0.054)	(0.082)	(0.016)	(0.043)
State-specific time trends	N	Y	N	Y	N	Y

A different sample is used in each row. Regressions include county-fixed effects, year effects, and state-specific time trends. Additional controls include racial composition, educational attainments, log number of enrolled students, and housing controls. Standard errors are in parentheses.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table A7: Annual Amount of Capitalization ($) by Discount Rate

	$r = 0.04$	$r = 0.05$	$r = 0.06$	$r = 0.07$
Sample	(1)	(2)	(3)	(4)
Full sample	523.976	654.970	785.964	916.958
1st revenue group	348.274	435.342	522.411	609.479
2nd revenue group	348.588	435.735	522.881	610.028
3rd revenue group	493.108	616.385	739.661	862.939
4th revenue group	536.768	670.960	805.152	939.344
5th revenue group	925.230	1,156.537	1,387.845	1,619.152

For the calculation of the annual amount of capitalization, I use estimates for capitalization in column (4) of Table 15 and 17. r refers to discount rates. Let A be an estimated effect on housing valaues, and r be a discount rate. Using the standard method, the annual amount of capitalization is Ar.

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