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Abstract

This paper examines the effects of constraints in a Tiebout framework applied to school finance reforms. We use data from Michigan, which enacted a comprehensive school finance reform in 1994 that, in effect, ended local discretion over school spending. This scenario affords us a unique opportunity to study the implications of imposing limits on local government's control over the quality of local public goods. We find that the reform was successful in overturning existing trends toward increased disparities. However, the reform also constrained the highest spending districts and was associated with negative effects on their subsequent educational outcomes. These results survive several sensitivity checks. Going behind the "black box" to look at whether the reform affected incentives and responses, we find that loss of discretion appeared to act as a strong disincentive to high-spending districts and, more generally, across the board. The performance improvements of the lowest spending districts were likely related to relative increases in spending rather than higher effort. This same finding is corroborated by results from an alternative strategy, which exploits differences in the nature of incentives faced by districts in more competitive areas versus those in less competitive areas.

Key words: Tiebout, school finance, incentives, competition

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1 Introduction

Local financing of public schools has been one of the distinguishing features of the K-12 educational system in the United States. A substantial share of the total funds for educational expenditures is raised at the local school district level, primarily by taxes levied on property. Local residents choose the property tax rate through elections, just as they choose members of local school board.[1] The other sources of school revenue have been state aid and federal aid. State aid has been the more important of the two, with federal aid generally accounting for only around 10 percent of the average per pupil spending and has remained more or less unchanged at that level for the past few decades.

This reliance on local tax revenues leads to a bundling of two distinct choices - residential choice and school choice. Parents in the United States often choose their residences on the basis of the quality of schools in the locality. Because, as is often argued, demand for (and affordability of) a good education increases with parental income and educational attainment, a Tiebout type sorting often results. Families with similar demands congregate, a pattern that leads to economic and demographic segregation across school districts within a state. Households "voting with their feet" have led to the formation of property-rich and property-poor communities, with the result that wealthy school districts have been able to spend more money per student than poor districts.

School finance reform, loosely interpreted as an equalization of school finances within state boundaries, can in principle weaken this link between residence choice and demand for schooling. While such reforms have mostly been implemented following adverse court decisions that ruled existing financing systems unconstitutional, state legislatures have also sometimes initiated them. Such measures typically increase the state share of K-12 revenues, mostly by giving large sums of money to the lowest-spending school districts, in an effort to reduce the prevailing disparities in per pupil spending across districts within the state.

One important aspect of many of these school reforms is their effect on local discretion over school

[1] For example, in 1999-2000, the share of revenues for public elementary and secondary schools that was raised locally was 43.2% (National Center for Education Statistics, 2003, Table 156). This percentage would have been even greater in earlier years, when most states had not passed school finance reforms.

spending. In an effort to reduce spending inequalities across districts, several states have implemented school finance reforms that either severely limit local discretion or end it altogether. This diminished local control over local government activities – schooling in this case – has significant implications for the provision of public education. The original Tiebout framework highlights the fact that centralization of public services at higher levels may reduce efficiencies associated with providing these services at the local level, possibly undermining the conformity between citizen preferences and the services provided.

We examine the effects of constraints in a Tiebout framework in the context of school finance reform in Michigan, which enacted a comprehensive school finance reform in 1994. The reform, called Proposal A, practically ended local control over the amount of school spending. This scenario affords us a unique opportunity to study the implications of limiting local government control over the quality of local public goods. With school finance reforms an important feature of the K-12 educational landscape in the country, understanding the full range of consequences of such reforms-particularly when they impinge on local discretion by imposing restraints on educational spending-is imperative.

The results are intriguing. We find that the school finance reform in Michigan was instrumental in significantly increasing the growth rates of spending in the lowest-spending districts:–in fact, the reform overturned trends toward increased disparities in spending evident in the immediate prereform period. However, the reforms also constrained the highest-spending districts in the state. These districts had been increasing their per pupil spending at significantly higher rates than their counterparts at the time of the reform. But Proposal A limited their discretionary power over school spending, and, consequently, the subsequent growth rates of spending in these highest-spending districts were considerably below those for the other districts. We provide evidence that the restrictions imposed on the highest-spending districts have been associated with adverse moves in their educational outcomes, as measured by student performance on standardized tests administered by the state. Our results are robust to various robustness checks, including checking and accounting for differential exodus from the public school system to private schools, differential movements of demographic and socioeconomic groups across districts, voluntary contributions, economies of scale, control groups from other states,

alternative classification strategies, and mean reversion.

We next try to go behind the black box to look at whether the changes in student performance might be due to changes in effort caused by changes in the incentives generated by school finance reform. The loss of discretion in high-spending districts might act as a discouragement and induce them to reduce effort. The low-spending districts also faced a loss of discretion,– but at the same time they enjoyed increases in revenue and might have been able to relax previously existing financial constraints. We use detailed data on disaggregated spending to throw light on whether and how incentives generated by school finance reform mattered. We find suggestive evidence that loss of discretion acted as a severe disincentive for high-spending districts and in general for districts across the board. Loss of discretion may have helped induce the high-spending districts to lower their efforts as measured by the share of productive expenditures. These districts also saw a major decline in teacher salaries, which might indicate the hiring of lower-quality teachers. In contrast, little indicates that the improvement in performance of the lowest-spending districts was driven by an increase in effort;–it was more likely the outcome of increased spending made possible by the increase in state aid. We also use an alternative strategy, exploiting the location of districts in more competitive versus less competitive areas, to examine the role of incentives and effort in explaining the changes in performance observed after Proposal A. The intuition is that if high-spending districts do respond to incentives (loss of discretion) by changing effort, one would expect high-spending districts in more competitive areas to exert more effort or, equivalently, to reduce effort by a smaller margin. The results suggest that high-spending districts in more competitive areas indeed responded to incentives by exerting more effort; however, there is not much indication that low-spending districts also behaved the same way.

To summarize, we find that imposition of constraints on a Tiebout system in the form of limits on local discretionary power induced high-spending districts to reduce their effort, as evidenced by their lowering the share of resources allocated to basic instruction as well as by declines in teacher salaries. Some evidence indicates that the loss of discretion acted as a general disincentive for all districts. High-spending districts did indeed respond to incentives, as is further evidenced by the greater response in

districts facing more competition than in high-spending districts facing less competition. The influx of money into low-spending districts does not appear to have induced them to increase effort. Rather, the disincentive effects of loss of discretionary power seem to have affected districts across the board. The improvement in the low-spending districts was more likely due to increases in money that probably allowed them to relax previously existing constraints.

This study is most closely related to the strand of literature in public finance and economics of education that deals with the effects of school finance reforms. The empirical studies undertaken so far fall under two broad groups:–those that deal cross-sectionally with many school finance reforms and those that study individual states. Among the former, Murray, Evans, and Schwab (1998) conclude that court-mandated finance reforms have had a large positive effect on equalization of school resources. Card and Payne (2002) study not only the relative equalization in spending across districts but also its consequences for academic performance. They find that reforms that were successful in reducing interdistrict disparities in spending also led to a convergence in SAT scores across family background groups.[2] Corcoran and Evans (2007) look at the effects of the recent court-financed reforms and argue that spending increases due to court mandates increased spending at the 5th, 50th (median), and 95th percentiles by comparable amounts.

Among the individual state studies, results from earlier studies of the effect of school finance reform in Michigan can be summarized as follows. Roy (forthcoming) finds that the Michigan school finance reform was quite successful in reducing interdistrict spending disparities and that there was also a significant positive effect on student performance in the lowest-spending districts as measured in state tests. These results are similar to those of Papke (2005), who finds that the increases in spending in Michigan had significant effects on mathematics pass rates and that the effects were largest for schools that were initially lagging behind. Cullen and Loeb (2004) also look at the effect of Proposal A on spending equalization and test scores, but their analysis, while impressive in its broad scope, is

[2] SAT is the acronym for Scholastic Aptitude Test, which, along with ACT, is the most popular college prep test in the United States

less rigorous on these particular issues. However, their study highlights interesting facts and concerns relating to Proposal A–like disparities in spending before Proposal A, changes in tax burdens as a result of Proposal A, and future prospects for Proposal A-that have otherwise been neglected in the literature.[3] However, none of these studies-either for Michigan or any other state-focuses directly on the nature of the incentives faced and the corresponding responses of districts located at different points of their pre-program spending distribution (specifically, low-spending and high-spending districts). This study tries to shed light on this important but hitherto neglected issue.

This study is also related to the literature that analyzes the effects of broader tax and spending limits-not only school finance reforms-on student performance. Figlio (1997) uses detailed school-level data from 49 states to analyze the effects of tax-revolt era property tax limitations-defined as limitations passed during the "local property tax revolt" of the late 1970s and early 1980s-on school services. He finds that limitations are associated with larger student-teacher ratios, lower starting salaries for teachers, and lower student performance. Downes and Figlio (1997) similarly find that tax or spending limits on local governments lead to a significant decline in average student performance at the state level, although there is considerable heterogeneity across states, particularly in terms of whether the reforms have been mandated by the courts.

The present study differs from both these studies in some fundamental ways. First, the questions posed here are somewhat different. While Figlio (1997) and Downes and Figlio (1997) are interested in the overall effect of the broader tax limitation measures, we are interested in analyzing the impact of school finance reforms, particularly those that constrain local discretion, on outcomes in affected districts. In this exercise, we are particularly interested in how the reforms affected outcomes in districts at different parts of the spending distribution,–not only districts at the low end of the prereform distribution but also those at the high end. It is also arguable that a school finance reform will have a more direct impact on educational outcomes than a general tax limitation policy. Second, because

[3] Among other studies, Roy (2009) shows that in Michigan Proposal A has been responsible for increases in the value of housing stock in the lowest- spending school districts, and for improvements in several socioeconomic indicators in these districts, implying a decline in neighborhood sorting. However, there is continued high demand for residence in the highest- spending communities.

the Figlio and Downes and Figlio studies are national-level studies, the authors have to classify states into those with limitations and those without. Such a classification is often not unique,–and different classifications often yield different conclusions. Third, the analyses in these studies look only at the effects of tax limitation measures in the late 1970s and early 1980s, using data that end in the early 1990s. This study, in contrast, analyzes the effect of the school finance reform in Michigan implemented in the mid-1990s,–and hence is able to illuminate the effects of more recent school finance reforms.

2 Michigan School Finance Reform

Unlike most comprehensive school finance reforms, the Michigan program was not a response to any adverse court ruling or to a sudden rise in public concern over inequalities.[4] It was rather a consequence of the prevailing debate over high property taxes, whose main purpose was supporting local schools. In 1994, just before the program, Michigan's property tax burden was the seventh highest in the country, and Michigan was fourth among U.S. states in the share of school spending financed locally (61 percent).[5] In March 1994, Michigan voters overwhelmingly ratified Proposal A, which reduced the reliance of school revenues on property taxes, replacing them primarily by an increase in the sales tax from 4 to 6 percent. This change led to a more than doubling of the state share of K-12 spending, and state aid was used to equalize per pupil spending across districts.[6]

At the time of the reform, Michigan's state aid was based on a district power-equalizing (DPE) formula, whereby districts are allocated state funds based on their tax efforts. The objective was to make the system wealth-neutral,[7] leaving the choice of millage rates (property tax rates) to the local

[4] Two court cases in the previous two decades, Milliken vs. Green in 1973 and East Jackson Public Schools vs. Michigan in 1984, had both found the existing finance system constitutional. For more detailed descriptions of the Michigan reform, see Addonizio, Kearny, and Prince (1995), Courant, Gramlich, and Loch (1995), and Courant and Loeb (1997).

[5] Michigan ranked after New Hampshire (86 percent), Illinois (62 percent) and Vermont (61 percent);subsequently, in 1997, both Illinois and Vermont overhauled their school finance programs.

[6] Taxes on homestead property came down from an average of 34 mills to a uniform statewide rate of 6 mills. The tax on nonhomestead property was reduced too but kept at 24 mills. The share of the state in K-12 spending went up quickly, from 31.3 percent in 1993 to 77.5 percent in 1997.

[7] The idea behind wealth neutrality is that high tax wealth in a district should not lead to high revenues except through a higher tax effort. However, preferences for school spending are generally increasing in income and educational attainment, and the wealth-neutrality principle per se does not equalize per pupil expenditures across districts (see Feldstein 1975).

districts but supplementing revenues in districts with a low property tax base per pupil. However, the equalizing power of DPE had considerably eroded over the years. As Cullen and Loeb (2004) note, there was no limit to the amount of tax effort that the state would match through its guaranteed tax base. The state also did not recapture excess funds from wealthy districts. In addition, over time, the guaranteed base did not rise as rapidly as property values so that the share of off-formula districts rose throughout the 1970s and 1980s. In 1994, about one-third of all districts were too rich to be affected.

The new school spending plan, effective from 1994 to 1995, worked as follows. First, the 1993-94 level of spending in each district was taken as its base and came to be called the district's foundation allowance. Second, future increases in all districts' foundation allowances were governed entirely by the state legislature. The lowest-spending districts were allowed to increase spending at much faster rates than their richer counterparts so that the spending gap across districts could be progressively closed. Furthermore, all districts, however rich, were held harmless with no absolute decline in per pupil spending in any district.

Local discretion over spending was largely abolished following Proposal A; future increases in spending were dictated solely by the state.[8] This change has interesting implications for the effect of the program on the high-spending districts. In these districts, per pupil spending barely kept pace with inflation after the reform and rose by much less than had been the case just before the reform. For example, Bloomfield School District (a high-spending district) could increase its nominal spending by only about 10 percent between 1994 and 2001. Since prices went up by about 20 percent during this period,[9] many of these districts suffered a stagnation, if not an actual fall, in their real per pupil spending.

[8] For the three years immediately following Proposal A (1995-97), districts had the option of levying up to three additional mills for operating expenditure. This option ended in the 1997-98 school year. A bill was introduced in 2001 calling for a revision to the law that would allow districts to raise up to one mill for school operating costs with voter approval. However, that bill did not pass, due to fears that it might undermine the initial reform itself (Cullen and Loeb 2004).

[9] The consumer price index for Midwest Urban went up by 21 percent between 1994 and 2001.

3 Discussion: Effects of School Finance Changes on Incentives and Responses of School Districts

In this section, we discuss the basic intuition behind the effects of a school finance reform on school district effort and student performance. In particular, do school finance reforms differentially affect school districts situated at different levels of the pre-program spending distribution? Specifically, are high spending and low spending districts differentially affected? The typical school finance system in the United States is a classic example of the Tiebout setup: it is characterized by local discretion and flexibility and school districts have the ability to affect local revenue through their impacts on property values. For example, an increase in school district effort can plausibly lead to higher public school quality (higher student achievement), which can increase the demand for the respective schools and for the housing in the neighborhood and thereby increase property values and local revenue (given the tax rate). In other words, the public school districts have the ability and power to affect local revenue.

School finance reforms lead to a drastic centralization of school finances. The state typically sets the per pupil expenditures, and the districts have virtually no discretion, unlike earlier. Another important feature is that the low-income districts see their per pupil revenue increasing at a very high rate, while the high-end districts see their per pupil revenue barely rise.

How might these changes affect the incentives and responses of public school districts? Because low-income districts face a large increase in per pupil revenue after school finance reform, they might have an incentive to increase effort to attract students and increase revenue. But at the same time, they lose their local discretionary power in the sense that increasing effort to improve school district quality would no longer increase local property tax revenue. This situation would have an adverse effect on effort and thus render the total effect on school district effort ambiguous. It is worth noting here that, independent of effort, an increase in resources in low-spending districts can by itself affect student test scores. For example, increased resources might relax financial constraints faced previously and thus lead to improvement in performance, even in the absence of any change in effort.

High-expenditure districts, in contrast, face a very different situation. Unlike the low-spending

districts, they do not face an influx of money. But they do face a loss of discretion, given that their revenue and expenditure are determined by the state. Typically, per pupil revenue goes up by very little in such districts. These districts, therefore, have less incentive to exert effort, and one would expect school finance reform to have an adverse effect on effort in high-income districts.

Note, though, that while one might expect the above incentives and responses to work in the absence of other forces, these may be diluted or partly offset if there are other related changes, often triggered by school finance programs themselves. First, school finance equalizations might lead to differential movements to private schools across districts. For example, imposition of constraints might make high-expenditure districts less attractive and might induce parents in these districts to move their children to private schools. If the more motivated parents choose to move away, then we might expect a further decline in outcomes (achievements) in high-expenditure districts. On the other hand, if the low-performing students in such districts feel that the reduction in resources will cause the school districts to focus more on the easier-to-teach high-performing students and thus leave fewer resources for them, however, then this group might shift to private schools. This trend will bias upward achievement in high-expenditure districts. Similarly, low-expenditure districts may also be affected by such differential moves. A related question is whether there was entry into private schools after the reform and whether this movement varied across the different groups. For example, school finance reforms may make high-spending districts less attractive, thus opening up demand for private schools in these districts. It is an empirical question whether there were differential entries of private schools into different districts and/or differential movements of students to the private schools. We investigate these later in the paper.[10]

Still another factor is the differential mobility of demographic and economic groups across districts. If school finance reforms lead to a movement of more advantaged groups to low-spending districts (because of their relative attractiveness), then this outcome would further reinforce the positive effects

[10] Since during the period under consideration, there was a rapid growth of charter schools in Michigan, we also account for the differential spread of charters across the various groups. This is important, as charter schools can not only directly affect performance outcomes at neighboring regular public schools by enrolling students selectively but also can indirectly affect school performance through increasing parental choice and the competition for students.

of reforms in the low-spending districts. In contrast, with school finance reforms, the good schools in high-spending districts may become more affordable, which in turn may lead more motivated parents to move into districts they were previously unable to afford. This result would tend to counteract the negative achievement effects in high-spending districts.

Another factor is donations or voluntary contributions. Especially in high-spending districts, parents may engage in voluntary contributions to partly offset the constraining effect of school finance reforms. Such contributions would likely affect achievement favorably in high-spending districts, thus to some extent counteracting the convergence expected above.

Still another issue is economies of scale. School finance reforms make low-spending districts more attractive because of a large injection of resources, perhaps resulting in movement of students into these districts. Thus, low-spending districts might face an economies-of-scale advantage that could lead to improvement in student performance, independent of the factors mentioned above.

Not only can these factors differentially affect high- and low-spending districts, but also they can play out differently in different districts and hence can temper or reinforce the effects discussed above. Which factors did indeed come into play, how they affected districts, and what is their potential to bias the estimates are finally empirical questions that we address below in section **??**.

4 Data

We use data from multiple sources in the analysis that follows. Most of these come from the Michigan Department of Education (henceforth, MDE). The revenue and expenditure figures, as well as those on K-12 enrollment and teacher salaries, are taken from the Bulletin 1014s, published annually.[11] The disaggregated data on different spending categories basic instruction, administration, support services also come from this source, as do the data on property tax base of the school districts.

The data on ethnic and gender compositions and free lunch eligibility come from the Pupil Head-

[11] We mainly use data on general fund revenues and general fund expenditures - these are the most appropriate and widely used measures of spending. The exact definitions are available in the Bulletin 1014s. We also use current operating expenditures as a validity check on our results.

count Files and the Food and Nutrition Files of the MDE K-12 database.[12] The MDE K-12 Database is also the source of the data on student achievement in state tests. We use student achievement data for reading and mathematics in the fourth grade.[13] The data used here span the period 1990-2001, which straddle 1994, the last year before reform.[14] This time span allows us to adequately capture differences in pre-reform trends across districts and also to capture program effects that may occur only with a lag.

The data on median household income of the school districts in 1989 come from the 1990 census, as published in the School District Data Book. The School District Data Book is also the source of several other variables from the 1990 census that we use. In addition, we use data from the 1980 decennial census and the 2000 decennial census, both obtained from the Census Bureau.

For our analysis involving private schools, we rely on the data on private schools collected by the National Center for Education Statistics (NCES) of the U.S. Department of Education. The NCES administers the Private School Survey (PSS) every other year, which collects information on every private school in the nation. We obtained private school location data from the PSS for the years 1990-2000. For our analysis relating to competition, we obtained public school district maps from the census.

The data on Indiana and Ohio come from the respective departments of education. For Ohio, these have been extracted from the annual local report cards, available for each district since 1996. For Indiana, the K-12 School Data is the repository of much of the information. The measures of spending we use to classify districts into different groups are general expenditures for Indiana and total expenditures for Ohio, which are broadly similar to general fund expenditures in Michigan.[15] For test

[12] Some of the data on ethnicity and free lunch eligibility for the early years come from the Common Core of Data, a statistical database maintained by the National Center for Education Statistics.

[13] The state assessment in Michigan is known as the Michigan Educational Assessment Program, or MEAP. Henceforth we will refer to these tests as MEAP tests. The exact proficiency measure is the percentage of students scoring at or above the satisfactory levels.

[14] Henceforth in the paper, we refer to school years by the calendar year of the spring term; for example,, 1990 refers to academic year 1989-90, and so on.

[15] For Ohio, due to a change in definition, comparable figures are not available pre-1995. The expenditure data have been generously provided by Jim Brown of the Ohio Department of Education.

scores, I use results from third grade reading and mathematics in Indiana and fourth grade reading and mathematics in Ohio.[16]

5 Estimating Program Effects

5.1 Effect on School Spending

To examine the effect of Proposal A on school spending in Michigan, we classify the 524 K-12 districts into five equal groups based on the 1993-94 level of per pupil spending.[17] Some summary statistics on these groups of districts are shown in table 1.

First, using preprogram data, we look for the existence of differential trends between the different groups before Proposal A. We run the following fixed-effects regression using data from the five years immediately preceding the reform (1990-94):

$$Y_{sgt} = \alpha_0 + \sum_{g \in \{1,...,5\}} \alpha_{1g} * (D_g * t) + \alpha_2 * X_{sgt} + \varepsilon_{sgt} \qquad (1)$$

where $g \in \{1, ..., 5\}$, Y_{sgt} is the per pupil revenue or expenditure of district s in group g in year t, t denotes time trend, α_s is the district fixed effect, and X_{sgt} are the time-varying characteristics (controls).[18] D_gs are the dummy variables for the respective groups of districts.

The results are in table 2, where we report results from two samples: the first includes all 524 districts, and the second excludes Detroit.[19] The first two columns show the results for per pupil revenues; the third and fourth columns show the results for per pupil expenditures. The table shows a significant hierarchy in spending growth rates at the time of the reform. Both per pupil revenues and per pupil expenditures were increasing at the highest rates in Group 5 districts, followed by districts

[16] For Indiana, the exact measure is total mean NCE (normal curve equivalent) score. For Ohio, this is the percentage of students scoring at or above the level of proficiency.

[17] This classification follows Roy (forthcoming). There are an additional 31 non-K-12 districts in Michigan; however, most of these are very small.).

[18] Since free lunch data for 1990 and 1991 are either not available or not reliable because of small and inconsistent values, we have included only enrollment and racial composition in X_{sgt}. Running the regression on a subsample when data on all controls are available does not change the qualitative results.

[19] Detroit is the biggest school district in Michigan, alone accounting for about 10 percent of all Michigan K-12 students.

in Group 4 and so on. Conversely, districts in Group 1 were lagging behind all other districts. These data show that existing inequalities had been widening in the years just before the reform.

We next document the effect of Proposal A. Figure 1 shows the distributions of foundation allowances across school districts in Groups 1 and 5 in 1994 and 2001.[20] There has been a significant convergence between these groups in the postreform period, both in absolute and in relative terms.

To estimate the trends in spending in the different groups in the postreform period, after controlling for their respective prereform trends, we run the following fixed-effects (FE) regression:

$$Y_{sgt} = \beta_0 + \sum_{g\in\{1,\ldots,5\}} \beta_{1g} * (D_g * t) + \sum_{g\in\{1,\ldots,5\}} \beta_{2g} * (D_g * reform) + \sum_{g\in\{1,\ldots,5\}} \beta_{3g} * (D_g * reform * t) + \beta_4 * X_{sgt} + \varepsilon_{sgt} \qquad (2)$$

Here $reform$ is a binary variable that takes the value of 0 in the prereform period (1990-94) and 1 afterward (1995-2001). The variable t represents the time trend. X_{sgt} includes the racial and gender composition of students and the percentage of students eligible for free or reduced-price lunches in the regressions. The variables $reform$ and $reform * t$ respectively control for postreform common intercept and trend shifts. The coefficients on the interaction terms ($D_g * reform$) and ($D_g * reform * t$) estimate the program effects: β_{2g} captures the intercept shifts, while β_{3g} captures the trend shifts of different groups of districts. However, as mentioned earlier, the reform was staggered over several years, and hence the immediate increase in spending was not large. The estimated intercept effects are consequently very small, and below we focus on trend shifts in the postprogram period.

Table 3 reports results obtained from estimation of the above model, showing the effect of the Michigan program on per pupil revenues and expenditures. As can be seen, both revenues and expenditures grew at a considerably higher rate in the low- expenditure districts than in the high-expenditure districts. In fact, the hierarchy seen earlier in table 2 almost completely reversed itself. Controlling for prereform trends, Group 1 districts increased their spending at the highest rates in the postreform

[20] These show the kernel smoothed plots of foundation allowances in the two groups of districts. All figures have been weighted by district enrollment. For 2001, the foundation allowances for all districts in Group 1 were at $6000.

period, followed by those in Group 2 and so on. Furthermore, the coefficients are not only economically different but also statistically different as well.

Thus, the evidence points to a substantial program effect on equalization of school finances across the high- and low-expenditure districts. We next estimate the effects of the Michigan reform on academic performance.

5.2 Effect on Academic Performance

Figure 2 shows the distributions of the changes in fourth-grade proficiency results (reading and mathematics) in Groups 1 and 5 between 1995 and 2001. The top panel compares the change in reading proficiency, while the bottom panel compares mathematics proficiency. In both subjects, there is a relative shift of the lowest-spending line to the right after the program, suggesting convergence in the postreform period, The lowest-spending districts significantly narrowed the achievement gaps between them and the highest-spending districts.

Table 4 reports the effects of the program on fourth-grade MEAP reading and math tests. As earlier, we show results for two samples: the first includes all 524 districts; the second excludes Detroit. The achievement results mirror those obtained for revenue and expenditure above. The first two columns report results for reading and the next two columns for mathematics. The first column in each set includes all districts, while the second column excludes Detroit. Results for all districts show relative improvement of low-expenditure districts in reading. In math, while there seems to have been a deterioration in all groups after the reform, the low-spending group shows improvement relative to the other groups. (Note that the increased difficulty of the math test may have contributed to the general decline in math scores.) Exclusion of Detroit leads to a slight moderation of the effects, but the effects are qualitatively similar. In contrast, high-expenditure districts show deterioration in both reading and math after the reform. These findings are consistent with and support the intuition outlined in section **??**. They suggest that loss of discretion, as occurred in Michigan following Proposal A, can lead to a decline in performance for the high-expenditure districts. We pursue this hypothesis in more detail in

sections ?? and ??.

6 Sensitivity Checks

6.1 Mean Reversion

In general, the low-spending districts in Michigan were also low performing, and the high-spending districts were high performing (see the summary statistics on fourth-grade test scores in table 1). Thus, a potential concern is whether mean reversion-the statistical tendency whereby high- and low-scoring schools tend to score closer to the mean subsequently-can explain part of the results. To investigate this issue, we examine mean-reverting trends from the preprogram period. Instead of classifying districts on the basis of their spending in 1993-94, the last preprogram year, we classify them on the basis of spending in 1992-93 and compare the subsequent evolution in their test scores. If indeed we find the 1992-93 low-spending group improving in the next year and the high-spending group deteriorating, then these changes can be attributed to mean reversion (since this was in the preprogram period).

The results of this analysis are presented in table 5. Columns (1)-(4) show the results of classifying districts based on general fund expenditures, while columns (5)-(8) show the results of classifying the districts based on general fund revenues. Not much differential change across the different groups of districts thus classified is evident. In particular, no evidence suggests that the low-spending districts improved at a higher rate than their counterparts, either in reading or in mathematics. In fact, the coefficients are all negative, although they are not generally significant. The same is true for the high-spending districts, although there is some evidence of a relative decline in mathematics for this group. But this decline is small and barely significant at the 10 percent level. To summarize, no evidence indicates mean reversion for the low group. For the high group, mean-reverting trends, if present, were weak and unlikely to explain much of the change seen in table 4.

6.2 Were differential movements to private schools across school districts important?

One important factor that could potentially bias our results is whether there were any differential trends in movement to private schools between the different groups of districts following the school finance reform. It is possible, for example, that the constraints on local spending imposed by a school finance reform on the highest-spending districts induced some families to exit the public sector and enroll their children in private schools. In this case, depending on whether the students leaving for private schools are high performing or low performing, the relative deterioration in student performance in these highest-spending districts may be more a composition or sorting effect than the direct effect of limits on local discretion and spending.

The existing evidence on whether school finance reforms and tax or spending limits in general lead to an exodus of students from the public sector to the private one is mixed. Sonstelie (1979) and Sonstelie, Brunner, and Ardon (2000) argue that the move from local to state finance had little impact on private school enrollments in California. However, Downes and Schoeman (1998) argue that not controlling for unobserved heterogeneity, among other things, makes results of previous studies of questionable value. Based on data from the 1970 and 1980 decennial censuses for California, they find that school finance reform during the 1970s led to a significant rise in the share of enrollment in private schools.[21] Looking at general tax and revenue limits and using a cross-section of metropolitan statistical areas, Schmidt (1992) finds that state-imposed limits on revenue increases were positively related to increases in private school enrollment. Similar conclusions are reached by Hoxby (2001), who argues that large-scale school finance reforms that limit spending in richer districts can increase private school attendance by as much as 3 percent.

We use the decennial census data to look at the changes in private school enrollment across Michigan school districts between 1990 and 2000. The results are presented in table 6. Overall, no significant

[21] In raw numbers, the number of private schools in California went up from 1,505 in 1970 to 2,130 in 1980. Furthermore, after a decline in the early 1970s, the number of students in private schools in California increased by 22.4 percent between 1974 and 1980, while the number of students in public schools fell by 6.6 percent (Downes and Schoeman 1998).

change in private school enrollment appears to have taken place in Michigan during this decade. There is not much evidence of differential trends in either the lowest-spending districts or in the high-spending districts (or the others). The coefficients are always small and never significant. Overall, it looks highly unlikely that changes in private school markets are driving the results in Michigan.

6.3 Was there differential private school entry?

A related question is whether students entered private schools in greater numbers in the aftermath of the school finance reform. As mentioned above, the constraints on additional discretionary spending imposed by Proposal A might induce some families to exit the public sector and enroll their children in private schools, particularly in the highest-spending districts.

We use data on private schools collected by the National Center for Education Statistics of the U.S. Department of Education. We obtained private school location data (street addresses) for the years 1990 through 2000 from the PSS, and used ArcGIS to geocode each private school address. The resulting private school map was then overlaid on a map of Michigan school districts obtained from the Census Bureau, and the number of private schools in each school district was counted using ArcGIS. Using data from 1990 through 2000, we next determine whether there were differences in private school entry trends across the different groups of districts. The results, shown in table 7, do not indicate any differential trends across the various groups of districts. In particular, private schools do not seem to have mushroomed in the highest-spending districts, which would have felt most constrained by Proposal A.

6.4 Are differential mobilities into school districts driving results?

Another concern relates to the possibility that particular demographic or socioeconomic groups might move into various districts at different rates and that such mobility could affect student performance and lead to the above patterns Table 8 investigates differential mobility across groups of school districts in Michigan after the reform. We focus on the most important indicators of socioeconomic status-household incomes and educational attainment-with three separate variables for the latter: the

percentage of people in the district who are high school dropouts, the percentage of people who have some college education, and the percentage of college graduates (those with a bachelor's degree or higher). We use data from the last three decennial censuses-1980, 1990, and 2000-using the 1980 and 1990 censuses to control for differences in preexisting trends, if any.

Little evidence indicates substantial differences in mobility patterns across the various groups. Few of the coefficients are significant, and there are no general trends. There might have been a small influx of high-income and college-educated people in the lowest-spending districts; however, the coefficient for household incomes is only marginally significant. No evidence shows that high-income residents were leaving the high-spending district. The percentage of college-educated people may have declined slightly, but the magnitude of that decline is small and unlikely to explain the decline in performance of the higher-performing groups seen above. Overall, the trends suggest that differential mobility can explain only a small part of the results we found, if any.

6.5 Were economies of scale a major factor?

School finance reform led to an injection of money into the low-spending districts, thus making them more attractive. These additional resources might have led to an influx of students into these districts. Consequently, one might argue that economies of scale rather than the reform itself are driving the improvements obtained in the low-spending districts above.

To analyze this effect in more detail, we look at the trends in enrollment across the different groups. The results are in table 9. Because the lowest-spending districts actually lost students on average,[22] it does not seem that the improvements in Group 1 districts are due to economies of scale made possible by a larger student body. For the high-spending districts, there is no evidence of much change in the postreform period: although the coefficients are positive, they are never statistically significant.

[22] The districts in Group 2 also appear to have lost some students; however, the effect is much smaller and either insignificant or only marginally significant.

6.6 Relationship of per pupil spending in school district with Median household income and property tax base

The basic starting point of the empirical exercise is classifying and characterizing the different groups of districts, so that we can compare and contrast the evolution of different indicators in the postreform period across the various groups. For Michigan, we classified school districts on the basis of their per pupil spending in 1993-94, the last year before the reform, as state aid after the reform was based on that year's spending and thus it defined the natural treatment categories. However, it is important to ascertain that the results are robust to alternative forms of classification. An important feature of school financing in the United States that troubles lawmakers, judges, and educators alike is that per pupil spending depends largely on the affluence of the local school district. Many court cases, beginning with Serrano I in California (1971), have relied on this wealth-expenditure relationship as a yardstick of existing inequality. To check the robustness of our analysis as well as to investigate and compare the strength of this relationship in pre- and postreform Michigan, we do the following exercise.

We proxy school district affluence by two leading indicators-the median household income in the district and the per pupil property tax base in the district. The data on the former are drawn from the 1990 census, while the data on the latter come from Michigan's Bulletin 1014.[23] We estimate the following regression for median incomes; the regression for the property tax base is identical, with median income replaced by property tax income:

$$Spending_{st} = \gamma_0 + \gamma_1 * D_{94} + \gamma_2 * D_{98} + \gamma_3 * D_{01} + \gamma_4 * Income + \gamma_5 * (Income * D_{94}) + \gamma_6 * (Income * D_{98}) + \gamma_7 * (Income * D_{01}) + \gamma_8 * X_{sgt} + \varepsilon_{st} \quad (3)$$

where $Spending_{st}$ denotes per pupil spending in district s in year t, $t \in \{1990, 1994, 1998, 2001\}$, and $Income$ denotes median household income of the district in 1989 (1990 census). $D94$, $D98$ and

[23] Often, per pupil spending in a district is a function of district property wealth, as most revenues come from the local property tax. Generally speaking, the district property tax base and district incomes reflect socioeconomic status and are strong predictors of the desirability of a district as a potential place of residence.

$D01$ are year dummies for 1994, 1998, and 2001.[24] The coefficient γ_0 measures the strength of the income-expenditure relationship in 1990, the initial year in our analysis, while γ_1 gives the change in this gradient between 1990 and 1994, the last year before the program. The coefficients γ_2 and γ_3 show postprogram changes; compared with γ_1, they give an idea of the effectiveness of the program in narrowing spending inequalities.

The results are in table 10. The top panel shows the results for median income, while the bottom panel shows the results for the property tax base. As is evident, a large and positive relationship existed between district income and school spending in 1990, which was only slightly attenuated between then and 1994. Postreform, however, that relationship has weakened very significantly, suggesting that Proposal A was instrumental in substantially equalizing school resources across districts in Michigan. The results with respect to the property tax base are similar. There was a significant dilution of the spending-property wealth relationship in the postreform period, even though there were no preexisting trends toward equalization: the coefficients on the property tax base interacted with the 1994 dummy are all small and insignificant. However, the results suggest that the equalization with respect to median incomes has been more prominent than the equalization with respect to property wealth. Overall, the results are similar to those obtained above and suggest that our results are not sensitive to the particular classification strategy we employ.

6.7 Investigating the role of charter schools and interdistrict choice

Michigan passed a charter school law in the mid-1990s, along with the sweeping changes in school financing. Once introduced, charter schools spread quite rapidly in Michigan, and it is possible that failure to account for this rapid spread could bias some of the results. Many commentators believe that the beneficial effect of charter schools will spill over to students who remain in traditional public schools by increasing their productivity in the face of intense competition for students. Hoxby (2003) shows that in Michigan this is exactly what happened: districts that had a larger percentage of students in

[24] The controls include racial composition, location of the district (proportion rural), and size; however, the results are very similar if I omit these variables.

charter schools increased their productivity at a faster rate than those not similarly threatened.

However, even though charter schools have proliferated in Michigan, they still serve only a fraction of overall K-12 students. In addition, charter schools are not evenly spread out through the state. The lowest-spending groups (Groups 1 and 2) are predominantly rural, while charter schools in Michigan mostly serve urban children and are located in the higher-spending districts (table 11). Thus, the presence of charter schools is unlikely to bias the results for the low-spending districts. However, this competition effect would be strongest in districts in Groups 4 and 5. Therefore, the results for test scores in the high-spending districts might be underestimates if it were true that competition from charter schools indeed encouraged increased effort in the public schools close to them. As we have seen earlier, a decline in student performance is evident in the high-spending districts in Group 5; that decline would possibly have been greater if indeed charter schools spurred their neighboring regular public schools to put in greater effort than otherwise.

Moreover, geographically, most of the charter schools are located in southeast Michigan, particularly in Wayne County, where they service mostly students living in the poorer suburbs or inner-city Detroit. This concentration is highest in 10 school districts (including Detroit), all located in Wayne County. The results reported above are robust to excluding these 10 school districts. To save space, we have not reported on them here, but they are available on request.

Michigan also had an interdistrict choice program. However, it was very small: only about 1 percent and 1.5 percent of Michigan public school students enrolled in public schools outside their home district in 2000 and 2001, respectively (see Arsen, Plank, and Sykes 2001). As is generally the case for charter schools, public school choice too is concentrated mainly in and around Detroit. As Cullen and Loeb (2004, 242) note, "Student participation in schools of choice has largely been a Detroit phenomenon, with more than one-third of all transfers taking place within the Detroit metropolitan area." The results obtained above are robust to the exclusion of the Detroit metropolitan area.

6.8 Were voluntary contributions important?

Another factor is voluntary contributions. In some states with school finance reforms, most notably California, high-spending districts saw a large increase in their donations or voluntary contributions after the reforms. For example, Brunner and Sonstelie (2003) report that in 1994 local educational foundations, PTAs, and booster clubs raised nearly $200 million in California for public schools. The purpose was to offset, to some extent, the constraints imposed by school finance equalization. However, voluntary contributions seem to have played a negligible role in postreform Michigan. Addonizio et al. (1995) found some evidence that private foundations were more prevalent in those Michigan districts with higher average incomes, smaller fractions of minorities, larger enrollments, higher spending, and higher achievement. The magnitude of these private fund-raising activities is supposed to be quite modest, however; for one reason, probably unlike some other states (like California) Michigan did not witness a leveling down of school expenditures, and all school districts were held harmless regardless of their spending levels. As Downes and Steinman (2007) note, such private foundations have not raised sufficient revenues to affect significantly the efforts to equalize the distribution of per pupil spending.[25] A caveat is that voluntary contributions measured on tax returns of local education foundations might understate total parental contributions to schools. Note, though, that such contributions, either monetary or nonmonetary support, are more likely to be forthcoming in the high-spending districts that are also more affluent, and thus the decline in student performance in these districts might be even larger in the absence of these contributions. That is, if such contributions are an important facilitator of improved student performance, our results will only underestimate the true effects of the school finance reform in the high-spending districts.

6.9 Using Other States as Control Groups

One can argue that during the period under consideration other important changes were going on and that some of the effects we find may not be unique to Michigan. In this section, we compare the

[25] Even in California, Brunner and Sonstelie (2003) argue that the contributions "were not large enough to substantially undermine the effects of school finance reform."

Michigan experience to those of two neighboring states, Indiana and Ohio, to check for any potential bias in the results.[26] As in Michigan, we divide Indiana and Ohio districts into five groups each, based on their 1995 spending.[27]

The results in table 12 show the trends in student performance across different groups of districts in each state during 1995-2000. Michigan's unique experience, both with respect to the low-spending districts and the high-spending districts, is immediately evident. In Indiana and Ohio, the lowest-spending groups move mostly in line with their richer counterparts or actually exhibit a relative decline. In neither subject and in neither state is there any evidence of relative test score gains of the low-spending group that match those in Michigan. Similarly, no evidence whatsoever suggests that the highest-spending districts in either Indiana or Ohio suffered a relative fall. These findings strongly imply that Proposal A was responsible for at least a major part of the trends in test scores seen in post-1995 Michigan.

7 The role of Incentives and School District Effort: Investigating the Impact of the Reform on Resource Allocation in Different Categories

The results obtained above show that school finance reform led to a deterioration of performance in the high-spending districts and an improvement in performance in the low-spending districts. Were these changes driven by changes in effort induced by changes in incentives generated by school finance reform? For example, did the loss of discretion in high-spending districts act as a discouragement and induce them to reduce effort? Although the low-spending districts also faced a loss of discretion, at the same time they received increases in revenue. Was their improvement driven by an increase in effort or

[26] In choosing appropriate comparison states, we confined ourselves to the Midwestern states neighboring Michigan, Illinois, Indiana, Minnesota, Ohio, and Wisconsin. The obvious criteria for selecting control states among these five are first, the absence of any important educational initiatives during the 1990s, particularly the second half, and second, the availability of test score data similar to Michigan's. Based on these, Indiana and Ohio fit the bill best. There have not been any major school-related programs in either state during this period (except the school voucher program in Cleveland-our results are robust to exclusion of Cleveland-and another small Ohio program that tried to tie base funding levels to spending in districts in which students scored well on state tests). Furthermore, both states have test score data for grades and subjects similar to Michigan's.

[27] For Ohio, due to a change in definition, comparable figures on test scores are not available before 1995.

by a mere change in spending opportunities that relaxed previously existing financial constraints? We investigate these questions using detailed data on revenue allocation obtained from Michigan's Bulletin 1014. It includes data on different expenditure categories such as basic instruction, administration, support service, and average teacher salaries. Resources allocated to these categories after the reform and especially the shares (percentage contributions) of these categories give us some sense of how the school districts responded. For example, if we observe a fall in the amount and share of expenditures allocated to basic instruction, then that might indicate a reduction of school district effort. Using model 2 and each of these variables and their percentage contributions as dependent variables, we investigate how the different groups of school districts responded to the incentives created by the reform.

The results are presented in table 13. The first two columns report the changes in the money spent on basic instruction. The resources devoted to basic instruction generally fell, with the exception of districts in Group 1, which showed no change. Also, the groups follow a strict hierarchy, with Group 5 districts reducing the resources devoted to basic instruction the most and Group 1 districts showing no change. From the general fall, the loss of discretion apparently had a disincentive effect on the school districts, which was associated with a reduction of resources allocated to basic instruction. The hierarchy is consistent with the hierarchy in revenue changes experienced by the different groups of districts and may have been contributed by it. The results in columns (3)-(4), which look at the percentage of general fund expenditure allocated to basic instruction, show that this measure fell in each group and, interestingly, that these falls are comparable or similar across the different groups. The across-the-board decrease in the percentage of expenditures allocated to basic instruction further indicates that loss of discretion (common across all districts) served as a disincentive to the schools and was associated with lower resources devoted to basic instruction, which might be taken as implying reduced effort. No evidence suggests that the increased resources available to low-spending districts impelled them to increase effort: the percentage of expenditures devoted to basic instruction did not increase for this group. Rather, the loss of discretion seems to have been the dominant factor.

Resources devoted to administration also show a general hierarchy, with high-spending districts

reducing the resources spent in this category (although not in a statistically significant way) and low-spending districts increasing it. As for the percentage of expenditures on administration, no evidence implies any change in this category, and the percentages across the various groups remain similar. Resources devoted to support services also show the same hierarchy, with low-spending districts increasing expenditure in this category the most. In terms of percentage spent on support services, low-income districts seem to devote more resources to support services than the other groups, especially compared to the high-spending districts.

Average teacher salaries fell in all groups. Again, a strict hierarchy prevailed in teacher salary cuts across the different groups, with the highest-spending districts undertaking the largest cut. The huge salary declines in the high-spending districts might indicate that they chose to hire relatively low-performing teachers, which might also indicate a fall in school district effort. In percentage terms, resources devoted to teacher salaries remain comparable across groups.[28]

From this discussion, the loss of discretion in high-spending districts was indeed associated with decreases in measures of their effort, as most notably reflected in a decline in resources devoted to basic instruction, both in monetary terms and as a percentage of total spending. The drastic fall in teacher salaries seems to reinforce this hypothesis further. In fact, loss of discretion seems to have been a dominant discouraging factor across the board. Little indicates that the improvement in performance of the lowest-spending districts was associated with increases in measures of effort, unless increase in the resources devoted to support services can be viewed as reflecting a purposeful action to relax a crucial bottleneck. Rather, relatively larger amounts of money (as compared to the other groups) spent on different categories seemed to have made the difference. Note, though, that the size of the effect in improvement in the low-spending districts [0.08] was pretty small. To put this estimate into perspective, Krueger (1999) reported that smaller class sizes in the Tennessee STAR project resulted in an increase of about 0.28-0.22 standard deviations in test scores per year. Guryan (2001) finds that in

[28] Note that the fall in teacher salaries along with an increase in percentage share of teacher salaries might seem inconsistent at first sight. But they are not: there was a concomitant increase in teacher counts in all groups of districts, which explains this apparent inconsistency. In fact, school districts went for larger numbers of lower-salaried (and probably lower-quality) teachers.

Massachusetts fourth-grade test scores increased by about 0.50 standard deviation, due to an increase of about $1,000 in spending. Hoxby (2002), using the same MEAP data in her study of charter schools in Michigan, finds that a significant threat of charter school competition increases fourth-grade reading scores in public schools annually by 0.23 standard deviation.

Figure 3 graphs the distribution of resources (in monetary amounts) in lowest- and highest-spending groups before and after reform. As expected, the patterns are consistent with those obtained above, and there is a general convergence between the two groups in each of the categories.

8 Were there Differential Responses in School Districts Facing More Private or Public Competition? Further Investigating the Role of Incentives and Efforts

The above analysis seems to indicate that high-spending districts responded to the incentives (loss of discretion) by reducing effort. But was that really the case? In this section, we use an alternative strategy to investigate whether high-spending districts indeed responded to incentives by changing effort.[29] Consider two high-spending districts, one located in an area with many private schools or with other school districts where families might send their children and another in an area that provides families with much less choice. In other words, the former district faces much competition for its students, while the latter district faces only a little competition. However, both districts suffer the same loss of funding and discretion, despite the differences in the amount of competition they face. In this scenario, the district facing more competition from private schools and other public school districts would be reluctant to reduce effort by as much as its counterpart in a less competitive area, given that the same degree of reduced effort would drive away a much larger number of students in the more competitive district. That is, if high-spending districts indeed responded to incentives by changing effort, one would expect such districts in more competitive areas to exert more effort or, equivalently, to reduce effort by a smaller margin.

[29] We are grateful to David Figlio for suggesting this strategy as well as the strategy in section **??**.

In this section, we empirically investigate whether changes in competition (private and public) was associated with changes in measures of effort in high-spending districts (as well as other districts). Figures 4 and 5 respectively show the extents of and variation in public and private competition across Michigan. As is seen in figure 4, public competition varies considerably, with southeast Michigan offering significant public school choice opportunities and northern Michigan offering very few such possibilities. Figure 5 shows similar variation in the extent of private competition. School districts in southeast Michigan face the most private competition, while northern districts face the least.

Next, we consider measures of public and private competitiveness. To construct a measure of private competitiveness, we use private school data (1990-94) obtained from the biennial Private School Surveys. We first geocode the private school addresses in each year using ArcGIS. This map was overlaid on a Michigan school district map obtained from the U.S. Census Bureau. Then we calculated the number of private schools within each school district. The number of private schools in a school district serves as a measure of private school competition for that district. We use three variants of this measure of private competition: the number of private schools in a district in 1990, the number of private schools in a district in 1992, and the number of private schools in a district in 1994. Note that all three are prereform years, as Proposal A was implemented beginning the 1994-95 school year. The results are robust to the particular preprogram year considered. Below we report results only for the 1994 measure of competition; the results for the other measures are similar and available on request.

We also use multiple measures of public competition: (1) the number of public school district centroids within 10, 20, and 50 miles of a school district centroid; (2) the number of public school districts (any intersection) within a 2-mile, 5-mile, or 10-mile buffer (ring) drawn around the boundary of a district; (3) the number of public school district centroids within a 2-mile, 5-mile, or 10-mile buffer drawn around the boundary of a district. We use ArcGIS to compute these measures. The results reported here are robust to the type of measure we consider. For lack of space, we report only results from measure (2); the results for the other measures are similar and available on request. We estimate the following specification where competition denotes public or private competition as defined above:

$$Y_{sgt} = \delta_0 + \delta_{1g} * t + \delta_{2g} * reform + \delta_{3g} * (reform * t) + \delta_{4g} * (competition * t) +$$
$$\delta_{5g} * (competition * reform * trend) + \delta_{6g} * X_{sgt} + \varepsilon_{sgt} \qquad (4)$$

for groups $g \in \{1, 5\}$. The results are in table 14. Panel A shows the results for math scores, while panel B shows the results for reading scores. As shown, districts in Group 5 facing greater public or private competition exhibited larger improvement in test scores than those that did not face as much competition. This result holds true for both math and reading and for both private and public competition, although the effect of public competition on these high-spending districts in reading test scores is not significantly different from zero. Conversely, however, no evidence indicates that among low-spending districts those that were located in more competitive areas witnessed any larger improvement than their counterparts in less competitive areas. This result holds true both for math and reading and for both types of competition, public and private.

In other words, while scores in high-spending districts deteriorated after the school finance reform, this fall was less severe in school districts that faced more private or public competition. In contrast, low-spending districts do not appear to have responded to competition after the program. This finding further suggests that high-spending districts responded to incentives by changing effort and reinforces the hypothesis that the high-spending districts do in fact respond to incentives. A reduction in effort, caused by constraints imposed by loss of discretion, likely explains the deterioration of performance in high-spending districts.

9 Conclusion

Over the past 40 years, school finance reforms have become a ubiquitous feature of the K-12 education system in the United States. The direct motivation for these reforms is generally the desire to reduce disparities in per pupil spending across districts within a state and to lessen the burden of local property taxes. As a result of these reforms, low-spending districts typically receive significant increases in state

aid. However, another important consequence of such reforms, much less studied in the literature, is their effect on local discretion over school spending. With a view toward eliminating or drastically reducing disparities in per pupil spending, states with school finance reforms often impose significant constraints on the highest-spending districts.

This analysis is aimed at advancing our understanding of the effect of school finance reforms, particularly as they operate through their restraints on increases in local school spending. We argue that this diminished local control over local government activities-schooling in this case-has significant implications for the provision of the same, which can be viewed as a local public good. In a Tiebout framework, centralization of public services at higher levels of government reduces efficiencies associated with providing these services locally. This finding suggests that a school finance reform may have adverse incentive effects on school districts, although for low-spending districts such an effect might be tempered by the large increase in per pupil revenue that typically follows such a reform.

Using Proposal A in Michigan, which ranks as one of the most important and comprehensive school finance reforms undertaken over the past four decades, we empirically examine whether those reforms indeed have such effects on school districts. We put particular focus on the heterogeneity of effects across districts located at different points of the prereform spending distribution.

We find that Proposal A was instrumental in significantly increasing the growth rates of spending in the lowest-spending districts, as was its stated objective. In fact, the reform overturned trends toward increased disparities in spending evident in the immediate prereform period. At the same time, however, school finance reform significantly restrained local discretion over school spending, resulting in much lower growth rates of spending in the high-spending districts than before reform. We find that these restrictions affected subsequent educational outcomes in these high-spending districts. Low-spending districts improved their relative performance after the reform, while high-spending districts suffered a significant deterioration. Various sensitivity checks that we conducted-including controlling for differential mobility of various demographic groups across districts after the reform, differential movement to private schools, the role of voluntary contributions, mean reversion, using other states as

control groups and alternative classification strategies-confirm the robustness of our results.

We next looked beyond these results to see what might have caused these patterns. In particular, we investigated whether changes in measures of effort by school districts were associated with the changed incentives incentives following the reform. For this purpose, we looked at the effects on resource allocation in various categories. Disaggregating overall per pupil spending among its various individual components, we found that loss of discretion may have acted as a disincentive for high-spending districts and was associated with reductions in productive expenditures, such as lowered percentages of its expenditures on basic instruction and on average teacher salaries. In fact, the loss of discretion seems to have worked as a deterring factor across the board. Little evidence suggests an increase in measures of effort by the low-spending districts:–the improvement in their performance seems to have been driven more by the increase in spending. Results from an alternative strategy, which exploits differences in the nature of incentives faced by districts located in more competitive areas versus those located in less competitive areas, corroborate the same finding. Our data show that high-spending districts indeed responded to incentives. High-spending districts in more competitive areas exhibited larger responses than those in less competitive ones. In contrast, measures for low-spending districts do not seem to have responded to competition.

To the best of our knowledge, these results are novel in the literature. They have important policy implications. They highlight the fact that there may be unintended consequences of school finance reforms that impose restraints on local control over school expenditures in a bid to equalize per pupil spending across districts.

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Table 1: Summary Statistics for Different Groups of Michigan School Districts, 1994

	Group 1 Lowest Spending Group	Group 2 Lower Middle Group	Group 3 Middle Group	Group 4 Upper Middle Group	Group 5 Highest Spending Group
Ethnicity (Percentage)					
Whites	93.59	91.06	93.29	55.13 (77.26)	82.37
Blacks	1.49	3.65	1.68	38.32 (15.49)	12.04
Hispanics	1.96	1.74	1.72	2.80 (3.16)	0.77
Free Lunch Eligibility	23.62	18.43	16.10	31.64 (22.97)	14.68
Pupil-Teacher Ratio	23.00	23.16	23.07	24.54 (23.88)	22.43
4th grade Reading (MEAP)	40.59	43.66	42.72	40.36 (40.95)	49.44
4th grade Math (MEAP)	60.01	63.51	64.04	56.08 (59.82)	67.45

For Group 4, the figures in parentheses correspond to the statistics when I leave out Detroit. Detroit is the largest school district in Michigan, alone accounting for about 10% of the total student population in the state. All figures have been weighted by enrollment of the districts in 1994. The results for MEAP scores correspond to 1995, as tests in Michigan during that time were administered in early fall.

Table 2: Pre-reform Trends in Per Pupil Revenues and Expenditures across Michigan School Districts

	Per Pupil Revenue		Per Pupil Expenditure	
	(1)	(2)	(1)	(2)
Group 1 * t	212***	212***	227***	227***
	(6)	(6)	(5)	(5)
Group 2 * t	249***	249**	248***	245***
	(8)	(8)	(8)	(8)
Group 3 * t	263***	264***	246***	245***
	(9)	(9)	(7)	(7)
Group 4 * t	304***	298***	293***	247***
	(16)	(14)	(27)	(13)
Group 5 * t	353***	357***	287***	287***
	(13)	(13)	(12)	(11)
Observations	2603	2598	2603	2598
R-squared	0.96	0.96	0.96	0.96

Columns marked (1) include all 524 school districts, while columns marked (2) exclude Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). Results are obtained from estimation of model 1. All regressions are weighted by district enrollment, include district fixed effects, and control for enrollment and ethnicity. Revenue relates to general fund revenues and expenditure to general fund expenditures. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 3: Effect of Michigan School Finance Reform on Per Pupil Revenues and Expenditures

	Per Pupil Revenue		Per Pupil Expenditure	
	(1)	(2)	(1)	(2)
Group 1 * reform * t	126***	126***	101***	101***
	(9)	(9)	(9)	(9)
Group 2 * reform * t	26**	27**	35***	35***
	(12)	(12)	(11)	(11)
Group 3 * reform * t	-36***	-35***	-16*	-16*
	(11)	(11)	(10)	(10)
Group 4 * reform * t	-20	-37**	-10	17
	(27)	(19)	(40)	(18)
Group 5 * reform * t	-124***	-123***	-33*	-35*
	(21)	(20)	(20)	(19)
Observations	6269	6257	6269	6257
R-squared	0.96	0.96	0.96	0.96

Columns marked (1) include all 524 school districts, while columns marked (2) exclude Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). Results are obtained from estimation of model 2. All regressions are weighted by district enrollment, include district fixed effects, and control for enrollment and ethnicity. Revenue relates to general fund revenues and expenditure to general fund expenditures. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 4: Effect of Michigan School Finance Reform on Grade 4 Reading and Mathematics Tests

	Reading		Mathematics	
	(1)	(2)	(1)	(2)
Group 1 * reform * t	1.29**	1.28**	-4.93***	-4.94***
	(0.53)	(0.53)	(0.62)	(0.61)
Group 2 * reform * t	0.54	0.54	-6.69***	-6.69***
	(0.59)	(0.59)	(0.58)	(0.58)
Group 3 * reform * t	1.03**	1.04**	-5.21***	-5.21***
	(0.50)	(0.50)	(0.59)	(0.59)
Group 4 * reform * t	-1.63*	0.12	-6.75***	-6.04***
	(1.00)	(0.43)	(0.98)	(0.58)
Group 5 * reform * t	-2.39***	-2.35***	-8.02***	-7.99***
	(0.67)	(0.67)	(0.64)	(0.64)
Observations	4678	4671	4678	4671
R-squared	0.82	0.83	0.85	0.85

Columns marked (1) include all 524 school districts, while columns marked (2) exclude Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). Results are obtained from estimation of model 2. All regressions are weighted by district enrollment, include district fixed effects, and control for enrollment and ethnicity. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 5: Is Mean Reversion Driving Results?

	Classification based on school year 1992-93 expenditures				Classification based on school year 1992-93 revenues			
	Reading		Math		Reading		Math	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
93Group 1 * t	-1.43	-1.43	-0.95	-0.95	-2.40**	-2.40**	-1.69	-1.69
	(1.21)	(1.21)	(1.31)	(1.31)	(1.21)	(1.21)	(1.32)	(1.31)
93Group 2 * t	-1.79*	-1.79*	-1.12	-1.12	-0.27	-0.27	-0.65	-0.65
	(1.02)	(1.02)	(1.22)	(1.22)	(1.15)	(1.15)	(1.21)	(1.21)
93Group 4 * t	1.35	1.35	0.46	0.46	1.15	1.15	0.08	0.08
	(1.23)	(1.23)	(1.38)	(1.38)	(1.11)	(1.11)	(1.32)	(1.32)
93Group 5 * t	-0.008	-0.008	-2.07*	-2.07*	0.41	0.41	-2.28*	-2.28*
	(0.91)	(0.91)	(1.12)	(1.11)	(1.11)	(1.11)	(1.22)	(1.22)
Observations	1028	1027	1028	1027	1028	1027	1028	1027
R-squared	0.93	0.93	0.93	0.93	0.93	0.93	0.93	0.93

Odd numbered columns include all 524 school districts, while even numbered columns exclude Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). All regressions are weighted by district enrollment, include district fixed effects, and control for size and ethnicity. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 6: Changes in Private School Enrollment
(Michigan School Districts, 1990 and 2000 Censuses)

	(1)	(2)
Year 2000 Dummy	0.12	0.12
	(0.48)	(0.48)
Group 1 * Yr 2000	0.76	0.76
	(0.58)	(0.58)
Group 2 * Yr 2000	-0.28	-0.28
	(0.65)	(0.65)
Group 4 * Yr 2000	-0.73	-0.07
	(1.01)	(0.61)
Group 5 * Yr 2000	-0.87	-1.04
	(0.66)	(0.63)
R-squared	0.92	0.92
Observations	1038	1028
Districts	519	514
Weighted	Y	Y
Exclude 5 Biggest Districts	N	Y

The dependent variable is the percentage of enrolled students in a school district who attends private schools. Group 3, the middle quintile of districts in the pre-reform spending distribution, is the omitted category. The five biggest districts are Detroit, Grand Rapids, Lansing (all in Group 4), Flint and Utica (both in Group 5). The regressions are weighted by the enrollment of the district. Robust standard errors are in parentheses. *, **, *** denote significance at the 10, 5, and 1 percent levels.

Table 7: Was there Differential Private School Entry?

Dependent Variable = Number of Private Schools		
	(1)	(2)
reform * trend	-0.20	-0.39
	(0.11)	(0.33)
Group 1 * reform * trend	0.03	0.02
	(0.12)	(0.12)
Group 2 * reform * trend	0.06	0.12
	(0.12)	(0.11)
Group 4 * reform * trend	-2.00**	-0.21
	(0.83)	(0.24)
Group 5 * reform * trend	-0.27	-0.21
	(0.19)	(0.15)
Observations	3126	3120
R-squared	0.99	0.96

*, **, *** denote significance at the 10, 5, and 1 percent levels. This table uses private school location data obtained from the private school surveys of the Common Core of Data, NCES. The private school addresses were geocoded using ArcGIS. This map was overlaid on a Michigan school district map obtained from the Census and the number of private schools in each polygon (school district) was counted using ArcGIS. Column marked (1) includes all 524 school districts, while column marked (2) excludes Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). The table reports results corresponding to model 2 (with group 3 omitted) where the dependent variable is number of private schools. All regressions are weighted by district enrollment, include district fixed effects, and control for enrollment and ethnicity. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 8: Investigating Differential Mobilities into School Districts (1980, 1990, and 2000 Censuses, FE Regressions)

	Median Household Income		Percentage with less than 12th grade		Percentage with at least some College Education		Percentage with Bachelor's Degree or more	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform * time trend (t)	1010***	1010***	1.66***	1.66***	-7.90***	-7.90***	1.48***	1.48***
	(2.70)	(2.70)	(0.28)	(0.28)	(0.62)	(0.62)	(0.23)	(0.23)
Group 1 * reform * t	1092*	1092*	-0.35	-0.35	3.03***	3.03***	-0.06	-0.06
	(652)	(652)	(0.39)	(0.39)	(1.07)	(1.07)	(0.37)	(0.37)
Group 2 * reform * t	164	164	0.08	0.08	0.95	0.95	0.31	0.31
	(596)	(596)	(0.37)	(0.37)	(1.09)	(1.09)	(0.35)	(0.35)
Group 4 * reform * t	192	-426	0.29	0.57	-1.88	-2.24	-0.03	-0.33
	(1246)	(493)	(0.47)	(0.39)	(1.39)	(1.00)	(0.58)	(0.39)
Group 5 * reform * t	-1375	-590	0.17	0.13	-2.16**	-2.04*	0.53	0.54
	(875)	(624)	(0.41)	(0.43)	(0.99)	(1.08)	(0.64)	(0.67)
Observations	1559	1544	1559	1544	1559	1544	1559	1544
R-squared	0.93	0.94	0.97	0.97	0.98	0.98	0.97	0.97
Districts	523	518	523	518	523	518	523	518
Exclude 5 Biggest Districts	N	Y	N	Y	N	Y	N	Y

Group 3, the middle quintile of districts in the pre-reform spending distribution, is the omitted category. The regressions are weighted by the population of the school district. All regressions include district fixed effects, time trend and interactions of time trend with group dummies, not reported for brevity. Robust standard errors are in parentheses. *, **, *** denote significance at the 10, 5, and 1 percent levels. The regressions in columns (2), (4), (6) and (8) exclude the five biggest districts - Detroit, Grand Rapids, Lansing (all in Group 4), Flint and Utica (both in Group 5).

Table 9: Are Economies of Scale a Major Factor? Investigating Differential Changes in Enrollment

Dependent Variable = School District Enrollment	(1)	(2)
Group 1 * reform * trend	-19.31***	-18.31**
	(5.83)	(7.33)
Group 2 * reform * trend	-11.98*	-12.27
	(7.26)	(7.48)
Group 3 * reform * trend	-9.46	-9.87*
	(6.04)	(5.86)
Group 4 * reform * trend	-17.95	1.22
	(28.55)	(17.39)
Group 5 * reform * trend	10.46	8.06
	(17.42)	(16.48)
Observations	6269	6257
R-squared	0.998	0.993

Column marked (1) includes all 524 school districts, while column marked (2) excludes Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). Results are obtained from estimation of model 2 where the dependent variable is school district enrollment. All regressions include district fixed effects, and control for size and ethnicity. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 10: Relationship of Median Household Income and Property Tax Base in a District with its Per Pupil Spending
(Michigan, 1990, 1994, 1998 and 2000)

Panel A	Per Pupil General Fund Expenditure			Per Pupil General Fund Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Median Income	0.031***	0.052***	0.047***	0.026***	0.044***	0.043***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Median Income * Year 1994	-0.005	-0.010***	-0.004	0.002	0.002	0.003
	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)
Median Income * Year 1998	-0.025***	-0.026***	-0.024***	-0.013**	-0.011***	-0.011**
	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)
Median Income * Year 2001	-0.037***	-0.042***	-0.032***	-0.034***	-0.035***	-0.032***
	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.005)
Observations	2079	2079	2075	2079	2079	2075
R-squared	0.74	0.82	0.80	0.75	0.83	0.81
Weighted	N	Y	Y	N	Y	Y
Exclude Detroit	N	N	Y	N	N	Y

Panel B	Per Pupil General Fund Expenditure			Per Pupil General Fund Revenue		
	(7)	(8)	(9)	(10)	(11)	(12)
Property Tax Base	0.0098***	0.0148***	0.0138***	0.0101***	0.0138***	0.0134***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Property Tax Base * Year 1994	0.0006	-0.0013**	-0.0002	0.0007	0.0006	0.0009
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Property Tax Base * Year 1998	-0.0019***	-0.0050***	-0.0043***	-0.0012*	-0.0010	-0.0007
	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Property Tax Base * Year 2001	-0.0016**	-0.0062***	-0.0045***	-0.0013*	-0.0049***	-0.0040***
	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Observations	2079	2079	2075	2079	2079	2075
R-squared	0.83	0.89	0.88	0.85	0.90	0.89
Weighted	N	Y	Y	N	Y	Y
Exclude Detroit	N	N	Y	N	N	Y

Robust standard errors are in parentheses. Results are obtained from estimation of model 3. In each set, first column reports unweighted estimates, and the second and third columns show weighted estimates. The last column in each set excludes Detroit. All regressions control for racial composition, location of the district (proportion rural) and size. *, **, *** denote significance at the 10, 5, and 1 percent levels.

Table 11: Concentration of Charter Schools across Different Groups, Michigan 1996-2001

	Percentage of Students in Charter Schools					
	1996	1997	1998	1999	2000	2001
Lowest Spending Group	0.10	0.29	0.58	0.84	0.96	1.07
Lower Middle Group	0.10	0.41	0.61	0.95	1.24	1.46
Middle Group	0.25	0.63	1.14	1.60	2.13	2.45
Upper Middle Group	0.27	0.78	1.33	1.91	2.63	3.03
Highest Spending Group	0.31	0.71	1.41	2.03	2.77	3.26
Michigan	0.25	0.68	1.25	1.95	2.71	3.40

Table 12: Trends in Reading and Math Achievement, Ohio and Indiana, 1995-2000

	Ohio		Indiana	
	Reading	Math	Reading	Math
	(1)	(2)	(3)	(4)
Group 1 * t	0.030***	0.043***	-1.09***	-0.51***
	(0.002)	(0.002)	(0.15)	(0.14)
Group 2 * t	0.028***	0.037***	-1.16***	-0.26
	(0.002)	(0.002)	(0.17)	(0.17)
Group 3 * t	0.028***	0.042***	-0.89***	-0.13
	(0.002)	(0.002)	(0.17)	(0.12)
Group 4 * t	0.025***	0.037***	-1.01***	-0.13
	(0.001)	(0.002)	(0.19)	(0.18)
Group 5 * t	0.029***	0.041***	-0.92***	-0.02
	(0.002)	(0.002)	(0.19)	(0.24)
Observations	3635	4241	1758	1744
R-Squared	0.89	0.88	0.62	0.63

All regressions are weighted by enrollment and control for ethnicity. Robust standard errors are in parentheses. *, **, *** denote significance at the 10, 5, and 1 percent levels.

Table 13: Trying to go Inside the Black Box: Investigating the Effect of Michigan School Finance Reform on Resource Allocation

	Basic		% Basic		Administration		% Administration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group 1 * reform * t	0.74	0.64	-1.08***	-1.08***	22.66**	22.62**	0.20	0.19
	(9.06)	(9.07)	(0.15)	(0.15)	(9.07)	(9.01)	(0.18)	(0.18)
Group 2 * reform * t	-22.76***	-23.08***	-0.80***	-0.80***	3.67	4.25	0.04	0.05
	(8.10)	(8.04)	(0.16)	(0.16)	(7.28)	(7.27)	(0.10)	(0.10)
Group 3 * reform * t	-43.84***	-43.96***	-0.71***	-0.71***	2.72	2.93	0.13	0.14
	(7.61)	(7.67)	(0.14)	(0.14)	(5.13)	(5.16)	(0.10)	(0.10)
Group 4 * reform * t	-56.29**	-40.77***	-0.95**	-0.95***	-11.59	0.30	-0.18	-0.06
	(28.52)	(10.09)	(0.44)	(0.18)	(13.77)	(9.37)	(0.25)	(0.17)
Group 5 * reform * t	-78.95***	-80.11***	-1.02***	-1.03***	-1.77	-2.20	0.02	0.02
	(13.48)	(13.44)	(0.16)	(0.16)	(12.84)	(12.76)	(0.15)	(0.15)
Observations	5745	5734	5745	5734	5221	5211	5221	5211
R-squared	0.95	0.95	0.89	0.86	0.84	0.83	0.64	0.64
	Support Services		% Support Services		Teacher Salaries		% Teacher Salaries	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Group 1 * reform * t	50.74***	50.65***	0.55***	0.55***	-698.21***	-688.40***	0.008***	0.008***
	(9.62)	(9.59)	(0.15)	(0.15)	(116.65)	(114.95)	(0.001)	(0.001)
Group 2 * reform * t	22.12*	23.29*	0.55***	0.57***	-828.95***	-845.53***	0.008***	0.008***
	(13.22)	(13.17)	(0.16)	(0.16)	(115.42)	(115.44)	(0.001)	(0.001)
Group 3 * reform * t	-0.74	-0.38	0.30*	0.31*	-1098.78***	-1109.19***	0.012***	0.012***
	(11.95)	(11.86)	(0.18)	(0.18)	(105.53)	(105.88)	(0.001)	(0.001)
Group 4 * reform * t	-13.70	19.52	0.21	0.38*	-1600.57***	-1745.85***	0.015***	0.015***
	(45.77)	(24.12)	(0.84)	(0.23)	(318.88)	(122.99)	(0.003)	(0.001)
Group 5 * reform * t	-2.67	-2.69	0.33**	0.35**	-1590.13***	-1645.46***	0.010***	0.010***
	(19.23)	(18.90)	(0.16)	(0.16)	(157.97)	(147.36)	(0.001)	(0.001)
Observations	5221	5211	5221	5211	6266	6254	6266	6254
R-squared	0.94	0.95	0.81	0.85	0.87	0.90	0.83	0.80

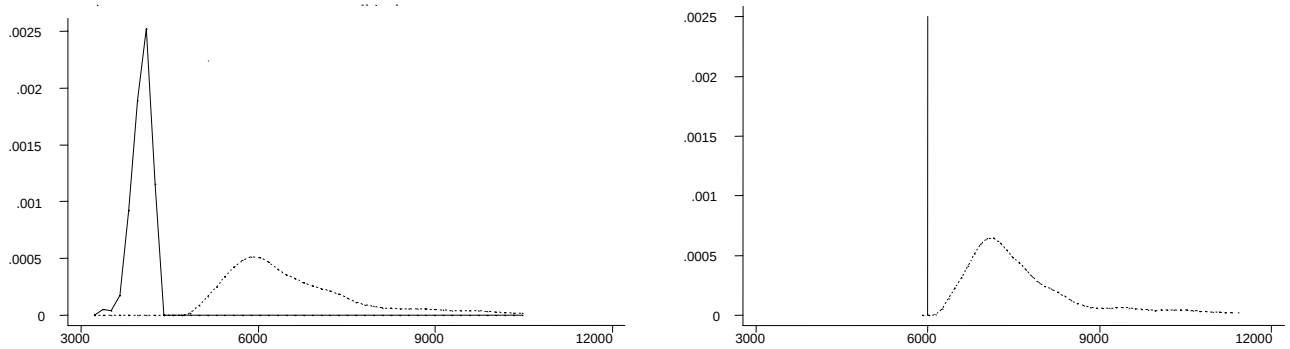
Odd numbered columns include all 524 school districts Even numbered columns exclude Detroit, which is the largest district in the state (accounting for about 10% of the total number of students in the state). Results are obtained from estimation of model 2 with the above variables as dependent variables. All regressions are weighted by district enrollment, include district fixed effects, and control for enrollment and ethnicity. *, **, *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 14: Investigating the Effects of Private and Public Competition on Test Scores, Groups 1 and 5

Panel A	Effects on Math			
	Private Competition		Public Competition	
	Group 1	Group 5	Group 1	Group 5
Private competition * reform * t	0.02	0.32**		
	(0.03)	(0.15)		
Public competition * reform * t			0.10	0.10***
			(0.08)	(0.03)
Observations	938	923	938	923
R-squared	0.79	0.89	0.79	0.89

Panel B	Effects on Reading			
	Private Competition		Public Competition	
	Group 1	Group 5	Group 1	Group 5
Private competition * reform * t	-0.16	0.18**		
	(0.10)	(0.09)		
Public competition * reform * t			0.08	0.16
			(0.30)	(0.29)
Observations	938	923	938	923
R-squared	0.75	0.85	0.75	0.86

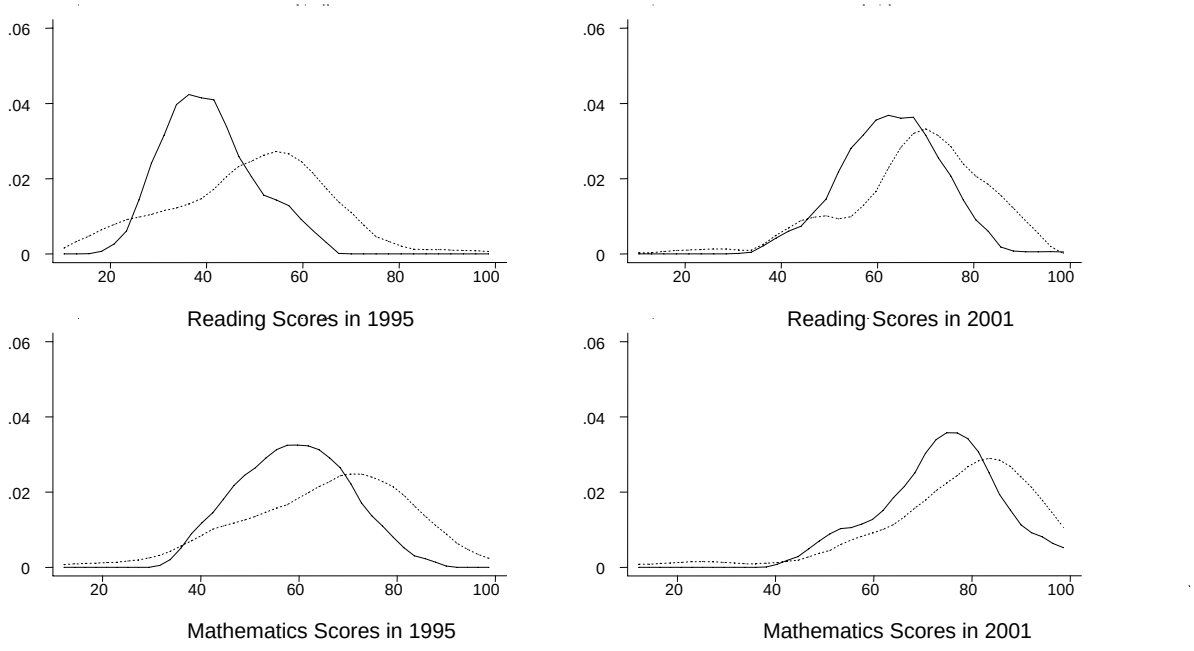
Private competition faced by a district is proxied here by the number of private schools in the district in the immediate pre-reform period (1993-94). Public competition faced by a district is measured by the number of districts that fall within a 2 mile buffer (ring). Results are obtained from estimation of model 4. The results are robust to several alternative definitions of public and private competition. See text. *, **, *** denote significance at the 10, 5, and 1 percent levels.



____ Lowest spending group …… Highest spending group

Figure 1. Distribution of Foundation Allowances in 1994 and 2001, Groups 1 and 5

Note: For 2001, the foundation allowances for all districts in Group 1 were at $6000.



____ Lowest spending group …… Highest spending group

Figure 2. Distribution of Test Scores in 1995 and 2001, Groups 1 and 5

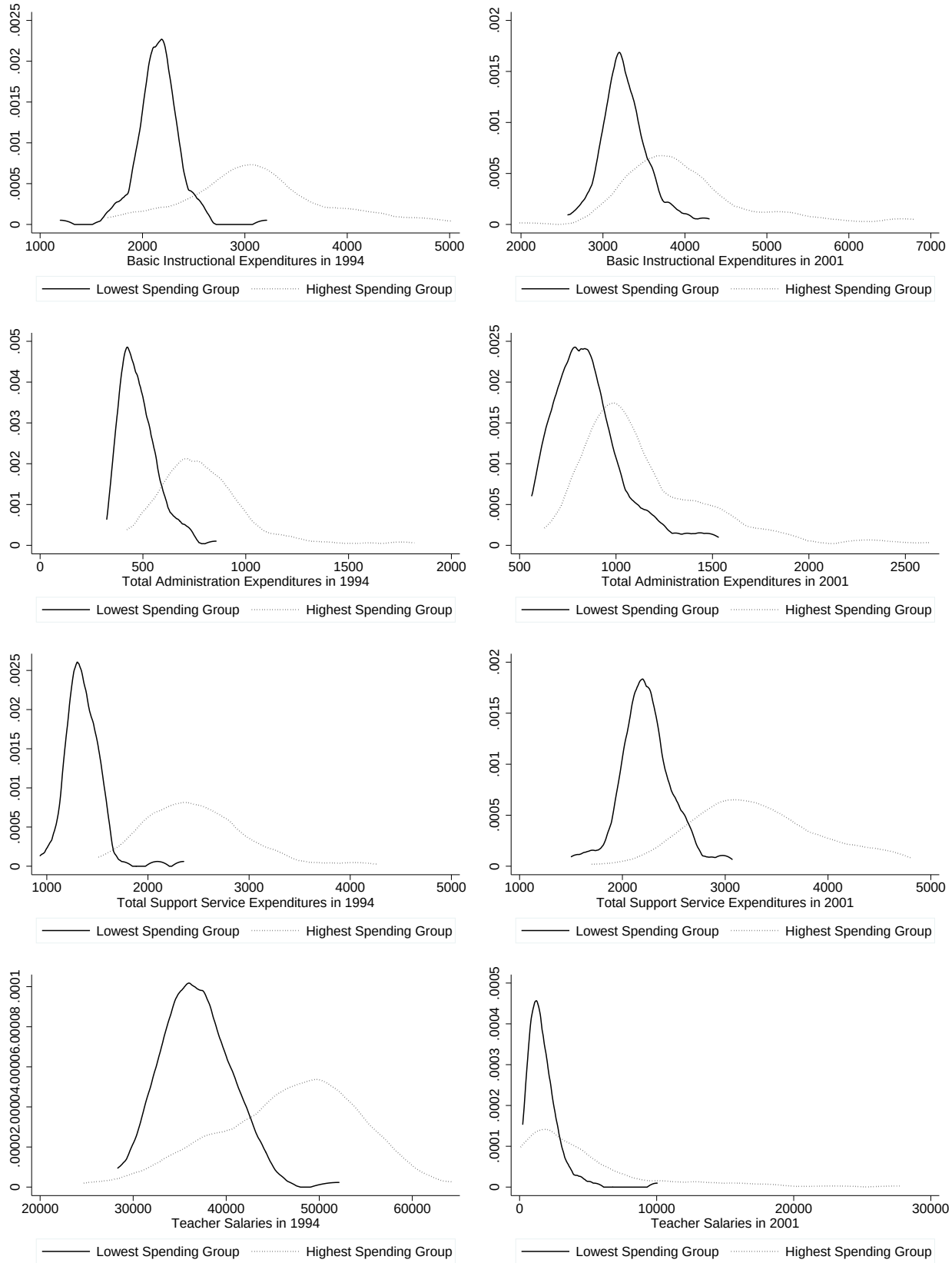


Figure 3. Resource Allocation: Distributions in 1994 & 2001, Groups 1 & 5

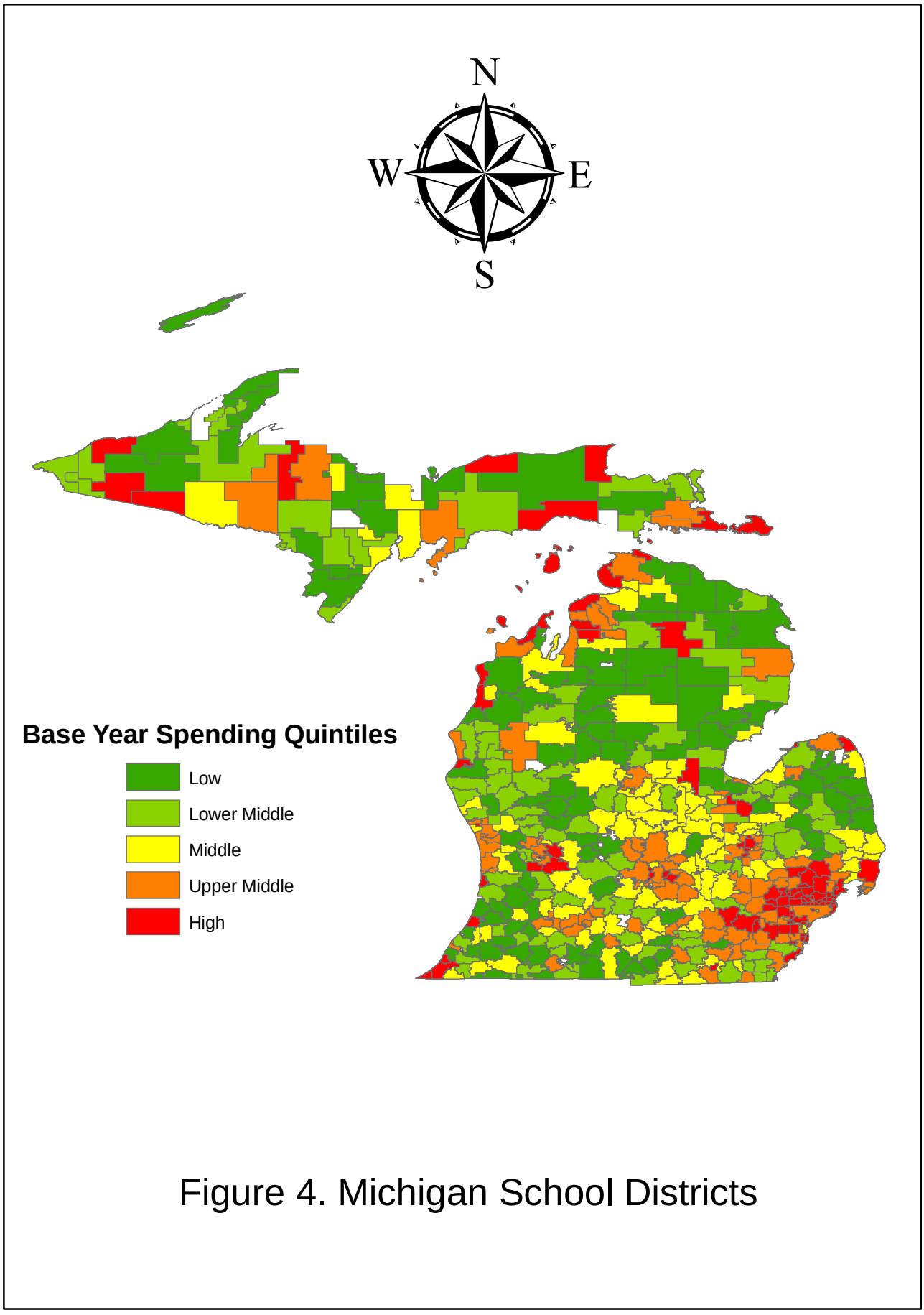


Figure 4. Michigan School Districts

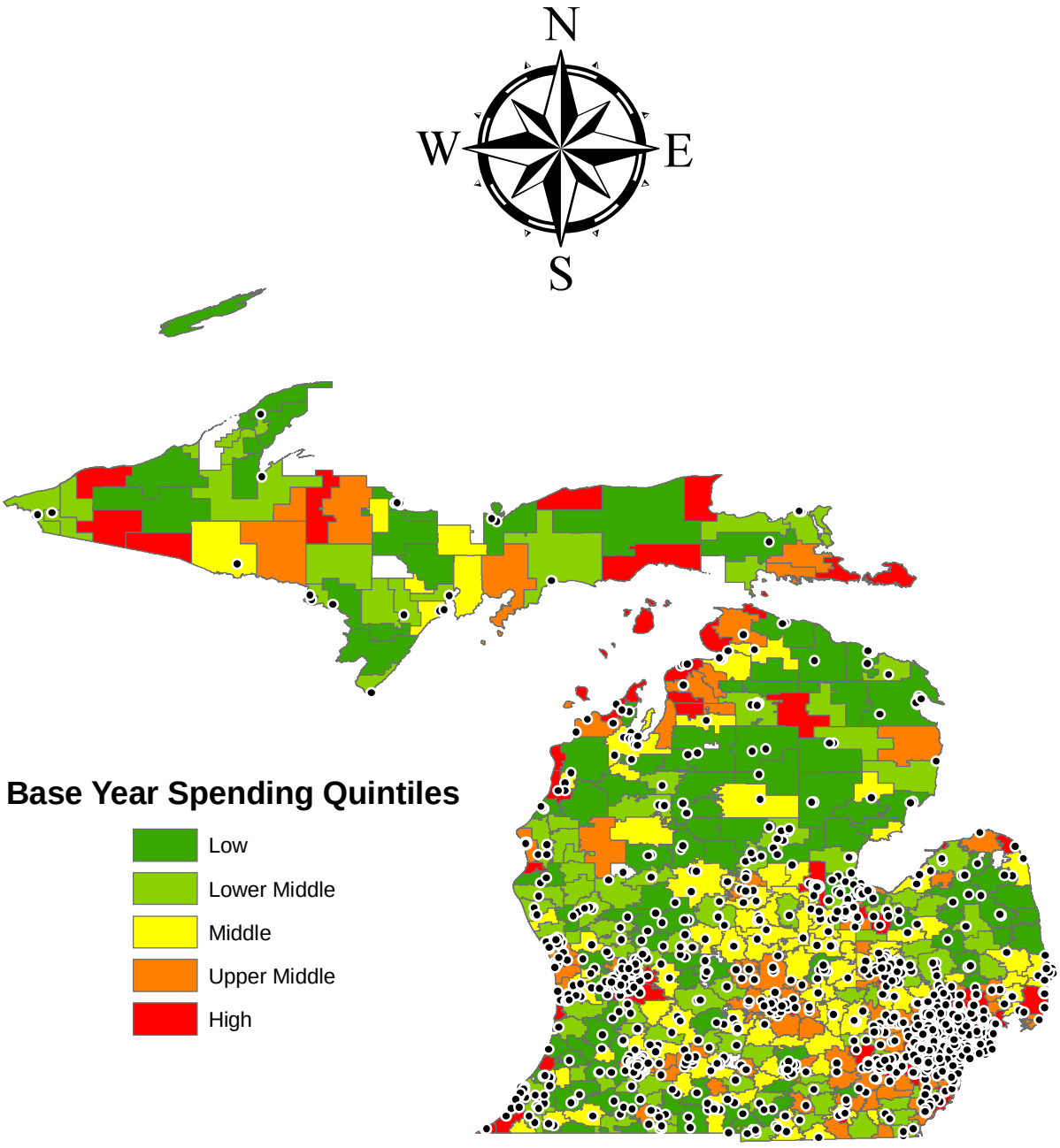


Figure 5. Michigan Private Schools by District