Final Report: Michigan Applied Public Policy Research Project on

K-12 School Finance

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March 10, 2000

Executive Summary

This study uses data from 1992 through 1997 on Michigan schools to determine the effects of spending on student performance. The years in the data set straddle 1994, when Proposal A was passed by the Michigan legislature. Proposal A dramatically changed the way that K-12 schools are funded, and has resulted in more equal spending across schools. I use the exogenous variation in spending resulting from the passage of Proposal A to more precisely estimate the effects of spending on student outcomes as measured by the standardized test scores of the Michigan Educational Assessment Program (MEAP) exams.

MEAP Pass Rates and Spending Statistics in Brief

The percentage of 4th graders performing satisfactorily on the math test increased every year with the exception of the last year of data, 1996/7. The average pass rate rose from about 37 percent in 1991/2 to over 60 percent in 1996/7. For the seventh grade math test, the average pass rate rose from almost 33 percent to over 50 percent over this same time period. For two science tests taken in the fifth and eighth grades, the average pass rate rose each year (from 69 and 54 percent, respectively) until the test was re-scaled in 1995/96.

Average real expenditures per pupil have risen every year and in each percentile for all schools combined. The lower percentiles increased the most in percentage terms. For example, in the 10^{th} percentile, expenditures rose from $2,484 (1997 dollars) in 1992/3 to $3,421 in 1996/7, a 38 percent increase. In the 50^{th} percentile, per pupil expenditures rose from $3,103 to $4,124 in 1996/7, a 33 percent increase. In the 90^{th} percentile, per pupil expenditures rose from $4,211 in 1992/3 to $5,198 in 1996/7, a 23 percent increase.

Average real expenditures per pupil rose from $3,259 in 1992/3 to $4,250 in 1996/7. Average real teacher salaries rose from $40,995 in 1992/3 to $46,891 in 1996/7. The pupil/teacher ratio is available for 1994/5-1996/7 (the pupil/staff ratio is reported for earlier years). The average pupil/teacher ratio fell slightly from 24.0 in 1994/5 to 23.5 in 1996/7.

Key Econometric Findings

Because I use data on the same schools over several years – a data structure typically called panel data or longitudinal data – I can explicitly control for unobserved school factors that might confound the effects of education inputs.

For some of the school performance measures, the findings are remarkably robust across different models and econometric specifications, while for others, the estimates are less stable across different econometric methods. The key findings of the econometric analysis of the effect of total spending on MEAP pass rates follows.

• Fourth Grade Math Test: A 10% increase in spending increases the pass rate by about .45 percentage points (from 60.00 to 60.45, for example), and this estimate is robust across various specifications. For schools that were initially weak performers (less than a 50% pass rate in 1993), the effect is about half a point.

• Fourth Grade Reading Test: The estimated effect of a 10% increase in spending ranges from just above zero and statistically insignificant to about .5 percentage points and statistically significant. One possible reason for the nonrobustness of the results is that the composition of the reading test changed during this period, and an overall reading pass rate had to be constructed from different components.

• Fifth Grade Science Test: The largest estimated effect for all outcome variables is for the 5th grade science test for schools that initially performed poorly. For schools with a pass rate below the median in 1993, a 10% increase in spending is estimated to increase the test pass rate by about one percentage point, and the estimate is very statistically significant. The estimated effect for all schools combined is smaller – about .46 points, so roughly the same as for the 4th grade math test – but it is still statistically significant. The larger effect for initially below-median schools is offset by essentially no effect for schools with initially high pass rates. The 5th grade math test also underwent a re-scaling in 1995-96 school year, but this is picked up by an aggregate time shift.

• For the middle school math and reading pass rates, there are no consistently positive effects of spending. While there appears to be a relationship when no other factors are controlled for, the effects disappear when both observed and unobserved factors are included. On the other hand, the estimated effect for the 8th grade science test is relatively large: current and lagged spending affect the pass rate, and the long run effect of a 10% increase in spending is an increased pass rate of about 1.37 percentage points.

- For the high-performing groups, spending has no estimated effect on any of the pass rates. This is very interesting because we find, at least for math and especially science, there are nontrivial effects for the low-performing group. This lends support to policies that increase spending at poor-performing schools relative to high-performing schools.

- My preferred estimate of the elasticity of average teacher salary with respect to spending is about .19, that is, a 10 percent increase in spending results in a 1.9 percent increase in teacher salaries. The pupil/teacher ratio is also affected by spending increases. I estimate that a 10% increase in spending implies a reduction in the pupil/teacher ratio of about .51, or about half a student per teacher. This is not a trivial effect. The estimates show that the student-teacher ratio increases, at an increasing rate, with the percent of the student body eligible for a free lunch.

- I also estimate the relationship between student performance and specific inputs into the teaching process, namely, teacher salaries and student-teacher ratios, using exactly the same econometric methods as for total spending. Interestingly, increasing spending on teacher salaries or reducing student-teacher ratios have no systematic effect on student performance.

An introduction to the report follows. Section 2 provides a brief history of the Michigan school finance reform. Section 3 explains the econometric methodology. Section 4 contains the data description and summary statistics. Section 5 presents the econometric analysis. Section 6 contains additional analysis that serves as a robustness check for the findings in section 5. Section 7 discusses possible shortcomings of the analysis.

1. Introduction

Much research has been done attempting to determine the link between education inputs and student outcomes. Generally, the goal is to estimate education production functions, which relate various inputs to measurable outputs. Having precise estimates of the effects of spending or other resources on student performance is very important from a policy perspective.

Most attempts to estimate the causal effect of spending on student performance can be expected to suffer from confounding factors. Generally, the problem is that variations in education inputs might be correlated with unobserved factors that affect student outcomes, such as family income. Demographic and economic variables are known to affect student outcomes. Failure to account for such variables can lead to spurious relationships between spending and performance.

Many attempts to estimate education production functions rely on cross-sectional data. While student demographic information can sometimes be controlled for, there is always the possibility that some unobserved factors that affect spending are correlated with student outcomes. For example, parent support, while perhaps partly captured by family income, cannot easily be measured. If schools that have large parent support also have higher per pupil spending, the effect of parent support will be wrongly attributed to spending.

Studies that use aggregate time series data require other factors that affect student performance over time to be uncorrelated with spending. This can rarely be assured. Hanushek (1986) contains detailed discussions of the problems inherent in inferring causality when relating student outcomes to spending and specific education inputs.

In this study, I exploit panel, or longitudinal, data on Michigan elementary and middle schools for the years 1992 to 1997. The data come from annual Michigan School Reports (MSRs). For each school, student pass rates on various MEAP exams are available, along with per student spending, school enrollment, average teacher salaries, and pupil-to-teacher ratios. The percent of students eligible for the school lunch program is also available, and this serves as a proxy variable for economic well-being of the students at a school.

Using econometric methods designed for panel data models when unobservables might be correlated with the observed explanatory variables, I can obtain a better estimate of the causal effect of school funding on student performance. The econometric methods are described more fully in Section 3. It turns out that allowing for arbitrary correlation between time-constant, unobserved school factors and observed inputs is effective only if the observed inputs contain sufficient variation over time. Fortunately, the years in my data set straddle 1994, when a dramatic change occurred in the way Michigan funds K-12 schools. In brief, the passage of Proposal A resulted in notable changes in the distribution of funding across schools. This exogenous change in funding acts as a natural experiment, and can allow more precise estimation of the effects of school inputs.

2. Background

Since 1974, Michigan had used a power-equalizing/guaranteed tax base (GTB) plan that was intended to provide an equal, basic per-pupil property tax base to each district, rather than a basic per-pupil minimum level of expenditure. In effect, the marginal cost of education spending was reduced because the GTB plan involved matching grants. No limits were placed on school spending. It was anticipated that the matching grants, by lowering the price of education, would increase education spending in low-spending districts. In fact, spending differences increased because residents of low-spending districts did not respond to the reduction in price of the GTB plan, while higher-spending districts continued to approve local tax increases to increase spending. Further, state categorical aid at this time was not equalizing.

As a consequence of growing spending inequalities across districts, in 1994 Michigan changed its system of school finance entirely (see Fisher and Wassmer, 1995 for a detailed discussion). The hallmark of the new system is a district foundation guarantee equal to per-student spending in the 1993-1994 school year plus annual increases. Districts above the state's basic foundation receive annual lump-sum per-student increases equal to the percentage growth of per-student state school aid revenue times the basic foundation. Districts spending less than the basic foundation receive up to double those annual per-student amounts. Thus, spending differences between districts will be reduced as low-spending districts are gradually raised to the basic foundation and as the growth is limited in high-spending districts.

Implicit in this finance change is the assumption that requiring increased spending of formerly low-spending schools will improve student performance. But the empirical evidence on this matter is mixed (Hanushek, 1986). Several recent studies have evaluated other states' attempts to equalize spending across districts. Downes (1992) analyzes California's Proposition 13, adopted in 1978, and finds reductions in differences between districts in total expenditures per student, but no corresponding equalization of student achievement as measured by test scores.

Downes, Dye, and McGuire (forthcoming) find that the recent imposition of property tax limits in the Chicago metropolitan area do not appear to affect student performance. This is only indirect evidence on these issues, however, since the districts could reshuffle their budgets to accommodate the tax limits. Indeed, the authors speculate that districts subject to the limitation measure appear to have protected instructional spending at the expense of other, potentially less productive, spending. With the Michigan data, I examine the changes in input mix directly on student performance.

Michigan's Proposal A created an excellent opportunity to examine this issue since the dramatic change in school funding produced a natural experiment (exogenous shift in the data) that allows for better estimates of the effect of spending. And, by using data from before and after the re-financing initiative, I can use econometric techniques that control for unobservables (time-constant characteristics of the student population, for example) as well as key covariates that are in the data.

The goal of this study is to use longitudinal MEAP data to determine the effects of the Michigan K-12 funding change on (1) student performance on various MEAP exams and (2) measures of educational inputs, namely, district pupil-teacher ratios and school-level teacher salaries.

3. Econometric Methodology

There are several different models and estimation methods available for linear models with panel data. One possibility is to essentially ignore the repeatability over time, and to simply estimate standard regression models by pooled ordinary least squares (OLS), where the student performance is related to education inputs and whatever other observable controls are available. At a minimum, one would include aggregate time intercepts to allow for secular changes in student performance and spending over time (including, for example, changes in definitions). Such an equation can be written as

$$Y_{it} = X_{it}\beta + I_{it}\gamma + T_t\theta + v_{it}, \tag{1}$$

where Y_{it} is the output of interest – such as percent of students passing the MEAP math test – X_{it} is a vector of student or school characteristics, such as enrollment and percent of students eligible for the school lunch program, I_{it} contains the education inputs, T_t denotes a vector of time dummies (to allow for aggregate effects), and v_{it} is the unobserved disturbance. If we have a very rich set of controls in X_{it}, we may be able to isolate the causal effect of the inputs on the output. The Michigan School Report data sets do not contain very rich controls in X_{it}, although the free-lunch variable essentially measures the poverty rate.

The primary problem with (1) is that it assumes that all school-level unobservables affecting student performance are uncorrelated with the inputs in I_{it}. Even if this is the case, the disturbances v_{it} are likely to contain substantial serial correlation, which invalidates the usual OLS inference procedures.

There are two common methods for exploiting the repeatability in panel data. One is to decompose the disturbance in (1) into a part that is constant over time – a so-called "school fixed effect" – and an idiosyncratic error that changes over time. This leads to

$$Y_{it} = X_{it}\beta + I_{it}\gamma + T_t\theta + \alpha_i + u_{it}, \qquad (2)$$

where α_i is the unobserved school effect. Provided the variables of interest in I_{it} – such as spending -- change over time, we can estimate the elements of γ while allowing for arbitrary correlation between α_i and I_{it}. Practically, this means that schools with historically high levels of student achievement, as captured in α_i, are allowed to have higher levels of spending.

The standard method for allowing correlation between the unobserved fixed effect and the observable explanatory variables is to remove α_i by subtracting off time averages. This leads to the fixed effects estimator. The fixed effects estimator is the pooled OLS estimator applied to the equation where time averages have been removed.

As a practical matter, we often need substantial time-variation in the explanatory variables in order to obtain precise estimates of γ (and β). Fortunately, the passage of Proposal A results in significantly more variation in spending than there would have been in the absence of the change in school funding.

A second possibility – one that is described in Hanushek (1986) when two time periods are available – is to add a lagged dependent variable to the equation. Instead of (1) or (2), we have

$$Y_{it} = X_{it}\beta + I_{it}\gamma + T_t\theta + \rho Y_{i,t-1} + v_{it}. \tag{3}$$

This specification allows for inertia in student performance by adding the lagged performance variable (rather than the school fixed effect in (2)). By controlling for the lag, we explicitly allow for spending to be correlated with student performance in the previous year. Neither (2) nor (3) is a special case of the other. I will estimate both kinds of models to obtain results as robust as possible.

4. Data Description and Summary Statistics

In this section, I use frequency distributions and summary statistics to describe the MEAP scores and education inputs over time. Tables 1-12 contain frequency distributions, averages, and standard deviations of test scores in elementary and junior high schools.

These data were obtained from the Michigan Department of Education web site www.mde.state.mi.us.

4.1. Test Pass Rates

The percentage of 4th graders performing satisfactorily on the math test, called *math4,* increased every year with the exception of the last year of data, 1996/7. This improvement is evident in the 10th, 25th, 50th, 75th, and 90th percentiles. (See Table 1.) For example, in the 1991/2 school year, 13.3 percent of the lowest 10th percentile of 4th graders performed satisfactorily. By 1996/7, this percent had risen to 31.3 percent of students. For students in the 50th percentile, the percentage of students passing rose from 35.7 to 62.2, and for the top 90th percentile, the percentage passing rose from 60.3 to 85 percent. I find a similar pattern for the 7th grade math test, but the pass rates for 7th graders are lower than for 4th graders. (See Table 2.)

The average annual pass rates for these two math tests are given in Table 11. For *math4* the average pass rate rose from about 37 percent in 1991/2 to over 60 percent in 1996/7. For *math7,* the average pass rate rose from almost 33 percent to over 50 percent over this same time period. For the two science tests, *sci5* and *sci8,* the average pass rate rose each year (from 69 and 54 percent, respectively) until the test was re-scaled in 1995/96.

Tables 3 and 4 report comparable results for the 5th and 8th grade science tests. The pattern appears to be similar to the math test, but an overhauling of the test for the 1995-96 school (to make the test much harder to pass) makes comparisons of the last two years with the first five years impossible. Fortunately, in our regression analysis we can handle this aggregate shift in the science pass rates by allowing for aggregate time shifts.

I do not present simple summary statistics for the reading test because a definitional change in the test midway in the time period makes the interpretation of statistics problematic. For the first three years of data, two reading test scores are reported for fourth and seventh graders (referred to as *story* and *info*). So, for the first three years of data, I construct a single pass rate equal to the average of the *story* and *info* pass rates. Beginning in the 1994/5 school year, one test score is reported, referred to as *read*. Consequently, the reading scores are not comparable across the entire period. Again, we can accommodate this at least to some extent in our econometric analysis.

4.2. Per-Pupil Spending and Components of Spending

Table 5 provides percentile breakdowns for real annual per pupil expenditures. Average real expenditures per pupil have risen every year and in each percentile for all schools combined. The lower percentiles increased the most in percentage terms. For example, in the 10th percentile, expenditures rose from $2,484 (1997 dollars) in 1992/3 to $3,421 in 1996/7, a 38 percent increase. In the 50th percentile, per pupil expenditures rose from $3,103 to $4,124 in 1996/7, a 33 percent increase. In the 90th percentile, per pupil expenditures rose from $4,211 in 1992/3 to $5,198 in 1996/7, a 23 percent increase.

Table 6 breaks out expenditures for elementary schools only. Elementary schools experienced a similar real increase in per pupil expenditures over this period, but the percentage increase from 1992/3 to 1996/7 does not fall uniformly with percentile (29 percent, 38 percent, 37 percent, 34 percent and 28 percent in the 10th, 25th, 50th, 75th, and 90th percentiles, respectively). Intermediate school percentage increases did decrease uniformly with percentile (31 percent, 27 percent, 26 percent, 21 percent, and 13 percent). (See Table 7.)

Average real teacher salaries rose from 1992/3 to 1996/7, although they fell between 1995/6 and 1996/7. (See Tables 8-10). For all schools combined, salaries in the 10th percentile rose over 17 percent over this period, in the 50th percentile the increase was 14 percent, and 12.7 percent in the 90th percentile. In the 1996/7 school year, teacher salaries averaged $36,583 in the 10th percentile to $57,876 in the 90th percentile.

The averages of per pupil spending, teacher salaries, and the pupil teacher ratio are given in Table 12 for school years 1992/3 – 1996/7 (these data are unavailable for 1991/2). Average real expenditures per pupil rose from $3,259 in 1992/3 to $4,250 in 1996/7. The coefficient of variation in expenditures, which measures average variation relative to the mean, fell uniformly from .257 to .198 over this period. Average real teacher salaries rose from $40,995 in 1992/3 to $46,891 in 1996/7. The coefficient of variation of teacher salaries fell from .192 in 1992/3 to .175 in 1995/6, but increased to .189 in 1996/7. The pupil/teacher ratio is available for 1994/5-1996/7 (the pupil/staff ratio is reported for earlier years). The average pupil/teacher ratio fell slightly from 24.0 in 1994/5 to 23.5 in 1996/7.

5. Econometric Findings

5.1. Effects of Spending on MEAP Pass Rates

5.1.A. Elementary Schools

I begin by estimating equations relating pass rates on the MEAP exams to spending and other controls. Table 13 contains the results for the 4th grade math score (*math4*). As a basis for comparison, I estimate equations that do not allow for a lagged dependent variable or an unobserved effect. The first column in Table 13 looks at a simple relationship between the math test pass rate, measured as a percent, and real per student spending. Spending appears in logarithmic form. Therefore, to obtain the effect of a 10% increase in spending on the pass rate, the coefficient on the spending variable is divided by 10. Allowing only for aggregate time effects, a 10% increase in spending is associated with about a .76 percentage point increase in the pass rate, or roughly three-quarters of a percentage point. Column (2) allows the effect of spending to act with a one-year lag. Interestingly, the lagged effect is much larger than the contemporaneous effect, and the contemporaneous effect is not statistically different from zero. The effect of lagged spending is similar to the effect estimated in column (1).

Column (3) adds the percent of students eligible for the school lunch program and school enrollment (the latter in logarithmic form). Both variables are allowed to have a diminishing effect – this is why they appear as quadratics. Including these controls lowers the estimated effect of spending, although the long run effect – obtained by summing the coefficients on the current and lagged spending variables – implies an increase in the *math4* pass rate of about .7 when spending increases by 10%. The smaller estimated effect of spending when *lunch* is added to the regression is consistent with the idea that schools with more children in poverty tend to spend less (that is, there is a negative correlation between poverty rates and spending). The *lunch* coefficient indicates that students living in poverty perform less well on standardized tests.

Columns (4) and (5) in Table 13 add last year's pass rate as an additional control. This allows us to do the following thought experiment: If two schools have the same enrollment, same percent of students eligible for the school lunch program, and had the same *math4* pass rate the previous year, what is the estimated difference in performance this year due to 10% more spending? Because the current spending variable is insignificant in column (4), and the long run effect in columns (4) and (5) are similar, I focus on column (5). Not surprisingly, when we control for inertia in performance, which allows past performance and past spending to be correlated, we find a smaller estimated effect. If spending is 10% higher in the previous year, *math4* is estimated to be about .43 points higher. This is not a large effect of spending, but it is statistically significant with a t-statistic above four.

Table 14 contains the results where the pass rate on the fourth grade reading test is the dependent variable (*read4*). The pattern of coefficients is remarkably similar to those for the *math4* outcome. The estimate in column (5) implies that 10% more spending in the previous year increases the pass rate on the reading test by about .40 points. Again, the effect is statistically significant.

The results for the fifth grade science test (*sci5*) are given in Table 15. The effects of spending are uniformly smaller for science than for math or reading. In addition, current spending seems to be more important than past spending (compare columns (4) and (5)). Overall, the pooled OLS results for *sci5* results suggest an effect about half as large as the effect for *math4* or *read5*.

Tables 16 and 17 contain the results of estimating equation (2) by fixed effects. Recall that this technique controls for an unobserved school effect – characteristics of the school that do not change over this time period – that may be correlated with spending and influence pass rates. Controlling for *lunch* and *enroll*, the fixed effects estimate implies that a 10% increase in spending last year increases *math4* by about .45 points, which is remarkably similar to the .43 obtained from column (5) of Table 13. (The total effect estimated in column (2) of Table 16 is about .67, but the current spending variable is insignificant.) Interestingly, once the unobserved school effect is controlled for, *lunch*, *enroll*, and their squares are insignificant. In fact, the joint F test for joint significance of these four variables yields a p-value of about .50, which is very large. This is not too surprising, as poverty rates and enrollments are generally slow to change over time, and a time-constant school effect is likely to capture across-school differences fairly well over short time horizons.

By contrast to the estimated effects for *math4*, the fixed effects estimates for *read4* differ significantly from the pooled OLS estimates with a lagged dependent variable. In columns (4) through (6) of Table 16, none of the spending variables is statistically significant, and each is small in magnitude.

Table 17 present the fixed effects estimates for the fifth grade science test. Column (3) shows that the fixed effects estimate for a 10% increase in spending on *sci5* is about .46, which is now essentially the same as the estimated effect for the math score.

To summarize, my estimates suggest a positive effect of spending on math and science test outcomes, with both methods of allowing for unobserved effects leading to very similar estimated effects. There is reason to believe a priori that the math results are the most reliable. The reading test changed its composition in 1994-95, and so a new reading variable had to be constructed. Unfortunately, this change coincides with the first year of Proposal A, so that the aggregate year effect captures both the new test structure as well as the shift in financing. Similarly, the fifth grade science test underwent a new scaling in 1995-96. Nevertheless, controlling for unobserved school heterogeneity via equation (2) is likely to be better than including a lagged dependent variable when the dependent variable is rescaled. Therefore, I conclude, somewhat cautiously, that the effects of spending on science and math are similar.

5.1.B. Middle Schools

The effects of changes in spending are likely to be larger for younger students since the fraction of time spent in school with higher spending levels is greater for younger children. For example, a fourth grader that has two years of additional spending is likely to be affected more than a seventh grader with two years of additional spending. Nevertheless, it is of some interest to see if we can detect the effects of more spending on middle school children. I analyze the seventh grade math and reading tests, and the eighth grade science test.

Table A.1 in the Appendix contains pooled OLS results, with and without the lagged pass rate, for the 7th grade math test. The last two columns show that, once the lagged pass rate is included in the model, spending has no measurable effect. However, when the model with current and lagged spending is estimated by fixed effects (Table A4), *math7* is predicted to increase by about .5 for a 10% change in spending. The current spending variable is more significant than the lagged spending.

The seventh grade reading score variable also shows a similar effect, at least in the first model without other controls. (See Tables A2 and A4.) The results in columns (5) and (6) of Table A.4 are a bit difficult to explain. None of the controls is significant, yet including them changes the coefficients on spending in important ways.

Tables A3 and A5 display the pooled OLS and fixed effects estimates for the eighth grade science test. The fixed effect estimates for the 8th grade science test are the largest of all effects. The current and lagged spending variables are both statistically significant, and the long run effect of a 10% increase in spending is estimated to be about 1.37 percentage points.

5.2. Effects of Spending on Teacher Salaries and Pupil-Teacher Ratios

In addition to studying the effects of school spending on MEAP test pass rates, it is also of interest to examine how the components of spending change when total spending change. Two components of spending are provided in the annual Michigan School Reports. The first is school average teacher salary, and the second is pupil to teacher ratio (available at the district, not the school, level). While the definition of the teacher salary variable has been the same since the beginning of the sample, the pupil-teacher ratio has not. Up through 1994, the MSR included information sufficient to compute the pupil-to-staff ratio. After 1994, the MSR reports the pupil-to-teacher ratio. Thus, the measure is not entirely comparable across years. (By definition, the pupil-to-staff ratio is smaller than the pupil-to-teacher ratio, and this is born out in the averages for each year.) Nevertheless, for estimating the relationship between the pupil-to-teacher ratio and spending, the change in definition may only be a minor problem. All regressions contain year intercepts, which can capture an aggregate shift. What it cannot capture is changing composition between teachers and staff across different schools.

Table 18 estimates regression models of the form of equation (1), where Y_{it} is either the log of real, average teacher salary (at the school level) or the pupil-teacher ratio (at the district level). I still include enrollment and the percent of students eligible for the school lunch program in X_{it}. Now, I_{it} is the log of real per-student spending. I pool the data for elementary schools and middle schools, as there is no reason to think separate equations are needed. (And, the pupil-teacher ratio is measured only at the district level, anyway.)

Without controlling for a school fixed effect, the estimated elasticity of average teacher salary with respect to total spending is about .37, and the estimate is very statistically significant. (It turns out that if one lag of spending is added to the regression, its coefficient is also statistically significant, but much smaller: about .086. The long run effect is about the same, so for brevity I only report the results from a static regression.) For the pupil-teacher ratio, a 10% increase in spending implies a drop in *ptratio* of about .83, or almost one student per teacher (or staff). Again, the effect is very statistically significant. Both regressions show that as spending increases, resources are put into both higher teacher salaries and smaller class sizes.

To control for unobserved school effects – so as to better estimate the change in salaries and pupil-teacher ratios when a school is exogenously given more money – I also estimate fixed effects models as in equation (2). These are also given in Table 18. The fixed effects estimate of the elasticity of average teacher salary with respect to spending falls to about .19, but still has a very large t statistic (17.8). Similarly, the relationship between *ptratio* and spending becomes weaker: a 10% increase in spending implies a reduction in *ptratio* of about .51, or about half a student. Still, this is not a trivial effect. (For *ptratio*, only current spending matters; lagged spending has a small and very insignificant effect.)

An interesting point is that both the pooled OLS and fixed effects estimates show that *ptratio* increases at an increasing rate once the percent of the student body eligible for free lunches reaches about 20 (the function turns up at 21.35 percent for OLS and 18.83 percent for fixed effects).

5.3. Effects of Teacher Salaries and Pupil-Teacher Ratios on Student Performance

Because we have at least two measures of school inputs other than total spending, we can study how these affect student performance directly. Tables 19 through 22 contain regressions of the form (1) and (3), where the inputs are teacher salaries and pupil-teacher ratios, both current and lagged one year. The pattern of the results is remarkably similar across all three pass rates. Without other controls, higher teacher salaries and lower pupil-teacher ratios are associated with higher test scores, and the effects are practically large. However, once enrollment and eligibility for the school lunch program are controlled for, the effects become small and insignificant, and also of the counterintuitive sign. When the lagged pass rate is added as a further control, the effects of higher teacher salaries and lower pupil-teacher ratios essentially disappear. The results of fixed effects estimation (see equation (2)) are consistent with the results that include a lagged dependent variable: if anything, the estimates in Table 22 show that higher teacher salaries lead to lower MEAP pass rates, and lower pupil-teacher ratios lead to lower pass rates. These results are somewhat puzzling. When we couple them with the findings from Section 5.1, we must conclude that spending generally – or at least spending that is not associated with higher teacher salaries or smaller class sizes – has a positive effect on MEAP pass rates, but spending to increase teacher salaries or to reduce class size essentially has no effect.

6. Robustness Checks

The econometric results reported in Section 5.1 are broadly consistent with the notion that increased spending can improve student performance, although the effects are fairly modest. One potential limitation of the models estimated in Section 5.1 is that they pool schools that begin with fairly low performance with those that are always high performers. To see why this pooling might be undesirable, consider an elementary school that has an 80 percent pass rate on the *math4* exam in the first school year for which we have full information on spending and at the beginning of the sample period, 1992-93. One argument is that, for these schools, it is very difficult to increase the pass rate. Conversely, for a school with a 30 percent pass rate in 1992-93, it should be easier to increase its pass rate. This criticism is partly handled by the fixed effects estimation, because each school has its school-specific unobserved effect that includes historical factors that cause some schools to be better than others. However, because the pass rates are necessarily capped at 100, the linear models may not adequately capture the effect of spending on pass rates throughout a wide range of pass rates. How this affects the estimates of the effect of spending on, say, the average or median school, is not clear. However, if spending grew at a faster rate at poorer performing schools, and such schools have scores that naturally grow at a faster rate, then the models from Section 5.1 might overestimate the effects of spending.

I use two approaches to examine the sensitivity of the estimates I reported in Section 5.1. The first is based on equation (3), which explicitly controls for the lagged pass rate when estimating the effect of spending on pass rates. If the effect of spending depends on the initial condition – as measured by last year's test score – then an interaction of the spending variable (say, lagged one year) with the score lagged one year should be statistically significant and practically large. In other words, $I_{i,t-1}Y_{i,t-1}$ should appear as a significant explanatory variable in (3). The hypothesis that spending has a smaller effect for higher performing schools means that the coefficient on the interaction term should be negative.

For brevity, I only discuss results for elementary schools; the findings for middle schools are qualitatively similar. Table 23 reports the coefficients on the lagged spending variable (which is still in logarithmic form) and the interaction term. For *math4* and *read4* the coefficients on the interaction term are small and statistically insignificant, while the level effect of the spending variable is very significant and roughly of the same magnitude as the models without the interaction. For the science pass rate, *sci5*, the coefficient on the interaction term is negative and marginally statistically significant. As mentioned above, this implies that spending has a smaller effect at higher performing schools, although the difference is not huge (.006 points for a 10 percent increase).

A second approach for studying differences between low- and high-performing schools is to split the sample based on the 1992-93 pass rates, and then estimate the fixed effects models in equation (2). For the math test, the median pass rate in 1992-93 was roughly 50 percent. Therefore, I reestimate the models reported in Section 5.1, but on two different samples: those with *math4* below 50 in 1992-93, and those with *math4* above 50 in 1992-93. The results for the spending coefficient are given in Table 24 for elementary schools. (This sample splitting approach is less successful for middle schools, as the sample size is already much smaller than for elementary schools).

For the low-performing group, the estimated effect of spending on the 4^{th} grade math pass rate is slightly larger than that obtained for the entire sample: a 10% increase in spending increases the predicted pass rate by about .52, so about half a point (compare Tables 16 and 24). For *read4* the effect is notably larger than that obtained in Table 16, although the estimate is still statistically insignificant. For *sci5*, the fixed effect estimate of spending on the pass rate, for initially low performing schools, is more than twice as large as that obtained on the entire sample (compare Tables 17 and 24). A 10% increase in spending is estimated to increase the science pass rate by about one point.

For the high-performing groups, spending has no estimated effect on any of the pass rates. This is very interesting because we find, at least for math and especially science, there are nontrivial effects for the low-performing group. This lends support to policies that increase spending at poor-performing schools relative to high-performing schools.

7. Caveats

One potential limitation of this study is that it may be too early to pick up the full effects of the funding change. In fact, in most of the specifications a change in spending one year ago has a larger effect on MEAP pass rates than a change in current spending. Given that we have only five years of data with full spending and MEAP information, we cannot hope to estimate effects at longer lags with any precision. One might view the estimates in this paper as a lower bound, as they capture only relatively short-term effects.

Second, the data are at the school level so I am able to control for school-level characteristics. However, the student body changes every year. So, I am not able to fully control for unobserved differences in the students across years. The fraction of students eligible for the free-lunch program does reflect one characteristic of the students each year.

Third, participation in the MEAP exams is optional. Discussions with school officials indicate that each school decides whether or not to emphasize school-wide participation (some require it). This may introduce a sample selection problem if schools that expect high pass rates, for example, require that the students take the tests and poor performing schools do not. Self-selection may take place among the students as well. Suppose the school encourages participation but does not require it. Less-skilled students may prefer not to take the test. Newspaper accounts of participation suggest that the bias may go the other way as well, as better students do not want to risk a possible black mark on their record. Since there is no a priori indication of a systematic bias in test-taking, the possible direction of the bias can not be signed. Data on participation rates by school would be useful in addressing this issue.

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