The Relationship between School Funding and Student Achievement in Kansas Public Schools

Florence Neymotin

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ABSTRACT

Recent changes in public school educational finance in the state of Kansas are shown to have had little positive effect on student educational achievement. A differences structure is used to determine the effect of changes in revenue per student at the district level on changes in measures of student achievement. Measures of achievement employed in the analysis are student test scores in math and reading, as well as various measures of student persistence in schooling.

INTRODUCTION

During the time period of 1997–2006, the state of Kansas witnessed drastic changes in its financial approach to educational reform, as documented in the School District Finance and Quality Performance Act.[1] These changes affected how the state distributes per student financial support to school districts in Kansas. In particular, the state of Kansas has progressively moved towards a redistributive system of financing education at the school district level. One example of this sort of change is increasing school funding based on the number of at-risk youth.

1. Kansas Legislative Research Department, 2006. Amendments to the 1992 School District Finance and Quality Performance Act and the 1992 School District Capital Improvements State Aid Law (Finance Formula Components).

Florence Neymotin is an Assistant Professor of Economics at Kansas State University.

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The current analysis of the amended Act finds different conclusions from those in an earlier study, which analyzed the Act before its recent amendments. John Deke examined the effect of the School District Finance and Quality Performance Act from the 1989 to the 1995 school years on the student dropout rate.[2] Deke's 2003 study focused on the immediate impact of the Act and found that, during the early 1990s in Kansas, a 20% increase in spending had the effect of increasing a student's probability of going on to college by 5%. The present analysis uses more current data than Deke's study and is, therefore, unique in its ability to analyze the effects of the most recent amendments to the School District Finance and Quality Performance Act on student outcomes.

In contrast to Deke's results, the current analysis finds only weak evidence that recent changes to school funding in Kansas had any role in increasing graduations rates. There is also little evidence of the effect of changes in school funding on improving student test scores.

The current analysis employs a differencing approach using district-level data for the years before and after 2005. A differencing approach for this particular time period is justified due to the large number of amendments to the School District Finance and Quality Performance Act which occurred in the year 2005.

BACKGROUND AND MOTIVATIONAL ELEMENTS

The history of education in the U.S. is one with varied systems of finance and educational goals of both the educators and the governing legislative bodies.[3] Until recently, education in urban schools was primarily seen as achieving the goal of assimilation and indoctrination of immigrants and other non-traditional groups—such as American Indians—with the values of "Americans."[4] Education today, however, is recognized as a force that can yield many other benefits to the individuals accruing the education, their peers, and to the society they live in as a whole. In addition to increasing an individual's earnings and longevity, increased education is also found to foster increases in civic participation, decreases in criminal activity, and a general heightening of the productive capacities of society.[5]

2. J. Deke. 2003. A study of the impact of public school spending on postsecondary educational attainment using statewide school district refinancing in Kansas. *Economics of Education Review*. 22: 275-284.

3. C. Goldin. 1999. A Brief History of Education in the United States. *NBER Working Paper Historical Paper 119*. 1-76.

4. R.J. Murname. 2008. Educating Urban Children. *NBER Working Paper no. 13791*. 1-45.

5. D. Card and A. Krueger. 1996. Labor Market Effects of School Quality: Theory and Evidence. In W. Burtless (Ed.), *Does money matter? The effect of school resources on student achievement and adult success*. 97-140. Washington D.C.: Brookings Institute Press. See: Jamison, E.A. et al. 2007. The Effects of Education Quality on Income Growth and Mortality Decline. *Economics of Education Review,* 26(6):

There is now, and has been for some years, debate regarding the appropriate measure of educational attainment.[6] Two routes have generally been taken in the economics literature in answering this question. The first route measures educational attainment with years of completed schooling, and the second route measures educational attainment in a broader sense via the test scores of students.[7]

There are benefits and drawbacks to both of these methods of measuring educational achievement. One of the clear benefits of using years of schooling as an outcome measure is that it is more intuitive, easily defined, and the input is clear—time in school. However, years of schooling as an outcome may not actually be capturing what it should. It is not clear that actual physical presence in a classroom is equivalent to "learning" and similarly it is unclear whether students whose test scores are higher needed to physically be present in school to achieve success.

A student's test scores, on the other hand, by measuring not just his or her physical presence in a classroom but also what has been absorbed, are a more precise measure of what the student is actually learning. However, test scores must take factors of the educational process into account which are not solely school-based inputs. Test scores may reflect inherent abilities of the child, the time the child puts into studying at home, or parental inputs into education accrued in the home. For this reason, test scores may be a better measure of achievement, but for these same reasons will be more difficult to manipulate.[8]

The current analysis takes the following approach: Test scores are used in addition to measures of years of schooling "attained"—alternately termed "persistence" in this article—as the outcomes of interest. In this way, it is possible to determine how both of the classical measures of educational achievement are

771-788. See: H.M. Levin et. al. 2007. The Public Returns to Public Educational Investments in African-American Males. *Economics of Education Review.* 26(6): 699-708; L. Lochner, L. and E. Moretti. 2004. The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports. *The American Economic Review*. 94(1): 155-189; K. Milligan et. al. 2004. Does education improve citizenship? Evidence from the United States and the United Kingdom. *Journal of Public Economics.* 88: 1667-1695.

6. E.A. Hanushek. 1986. The Economics of Schooling: Production and Efficiency in Public Schools. *Journal of Economic Literature*. 24(3): 1141-1177.

7. D. Card and A. Krueger, 1996, *op. cit.* 2002. School Finance Reform, the Distribution of School Spending, and the Distribution of Student Test Scores. *Journal of Public Economics*. 83(1): 49-82. Card and Payne note that several appropriate measures of educational achievement to use are student test scores and measures of persistence such as the graduation rate or post-secondary attendance or college graduation rates. This article is also particularly appropriate to the current analysis as it looks at changes in district finances and how they affect student outcome measures. There are various papers using test scores as the relevant outcome measure in the literature dealing with the effects of school finances on educational achievement. For one example see: J. Guryan. 2001. Does Money Matter? Regression-Discontinuity Estimates from Education Finance Reform in Massachusetts. *NBER Working Paper 8269*. 1-54.

8. Test scores have two additional benefits. They are a factor that is more variable in what employers see, i.e. there are many individuals with the same level of schooling but different test scores. Test scores are also considered more integral to increasing levels of societal production.

affected by changes in per pupil revenues.

Just as there are two avenues to measuring educational achievement in schooling, there have been two prominent avenues for determining how educational outcomes can be manipulated, which are through (1) changes in total revenues per student and (2) changes in class size. The current analysis focuses on the first avenue—namely it determines the effect of per pupil revenues on measures of student achievement.[9] Part of the reason for the popularity of this method is because of data availability on revenues per student through state departments of education and the department of the census.

The second possible way to measure school resources is through measuring class size. This approach has encountered some obstacles in the literature due to inadequate methods for ensuring that there is exogenous variation in class size.[10] The main result in these studies has shown that class size is indeed an effective method for increasing student educational achievement.[11] Reduced class size is particularly effective at helping students who are either "at risk"—experiencing some type of behavioral problems—or in younger grades and hence, easier to influence. Although this article does not employ class size data in the analysis, this is a possible avenue for additional or future research.

In terms of policy, the first step to determining whether total revenues per student affect student achievement is to document a relationship between policy related to school funding and actual changes in the amount of funding schools receive.[12] This issue is complicated by the fact that individuals often sort themselves into neighborhoods as a result of changes in school funding and possibly either counteract or exacerbate the intended effects of changes in

9. One might be tempted to use formula grants or a more specific breakdown of student funding in schooling when looking at the effect of finances on achievement. There are several problems with using this approach. The first problem is that there is often a complicated relationship between the various subtypes of funding. The second related problem is that funding is often allocated based on formula grants and when the funding runs out, then the actual allocation may be somewhat haphazard. For this reason, using total revenues or expenditures per student does not get into the minutiae and so avoids these particular pitfalls.

10. C. Hoxby. 2000. The Effects of Class Size on Student Achievement: New Evidence from Population Variation. *Quarterly Journal of Economics*. 115(4): 1239-1285

11. E. Lazear. 2001. Educational Production. *Quarterly Journal of Economics*. 116(3): 777-803.

12. One of the earliest studies in this genre was an analysis of the effects of educational finance reform in California on actual school finances. The major changes affecting California educational finance at the time of the study were the advent of Proposition 113 and the *Serrano* ruling. R. Fernandez and R. Rogerson. 1999. Education Finance Reform and Investment in Human Capital: Lessons from California. *Journal of Public Economics*. 74(3): 327-350. See also: S.E. Murray et al. 1998. Education-Finance Reform and the Distribution of Education Resources. *The American Economic Review*. 88(4): 789-812. It is also possible to answer a similar question using methods of calibration rather than employing actual documented policy changes. The authors of the study who do this find that switching from a system of state finance of education to one of purely local educational finance would increase school district educational spending. R. Fernandez and R. Rogerson. 1998. Public Education and Income Distribution: A Dynamic Quantitative Evaluation of Education-Finance Reform. *The American Economic Review*. 88(4): 813-833.

funding.[13] After showing that there is an effect of policy on changes in revenues per student, some studies went further and looked at the effect of these funding changes on student outcomes—with the aforementioned methodology.[14] In the case of Kansas, the time period of interest (1997–2006) did not witness the enactment of any new major legislation affecting its school funding practices. It did, however, witness a large number of amendments to its School District Finance and Quality Performance Act. This is one of the major education acts in Kansas, whose goal is the redistribution of finances to school districts to equalize educational resources.[15] Of the many amendments that were enacted to the School District Finance and Quality Performance Act, those that went into effect targeting at risk students were perhaps the most important amendments for the purposes of the current study.[16]

Deke's 2003 study is the only one to analyze the effects of the School District Finance and Quality Performance Act. Deke's analysis (2003) focuses on the years from 1989–1995 and the initial impact of the act. This does not account for the many amendments over the last 13 years. The current analysis instead seeks to determine how recent changes in per pupil revenues have caused changes in measures of student achievement, that is, how recent changes in education finance have affected the educational achievement of Kansans.[17] The data

13. C. Hoxby. 2001. "All School Finance Equalizations Are Not Created Equal." *Quarterly Journal of Economics.* 1189-1231. The issue of student sorting by location is not directly considered; however, it should be noted that the period considered represents one where only amendments were made to the main legislation on school financing, but no new major legislation enacted. It is less likely that individuals will sort as a direct result of application of the amendment rather than application of a larger piece of legislation. In terms of the concern that individuals are sorting on school quality irrespective of knowing about the enactment of legislation, is not formally treated in the analysis, however an argument is made in footnote 28 that addresses potential biases which will result from the estimation. The sign of the bias is discussed. It would represent an interesting extension of the current analysis to additionally include a methodology incorporating a sorting on school quality or an analysis specifically targeted to urban versus rural populations where the issues of sorting would vary—with the urban population being presumably more stable.

14. See J. Guryan. 2001. Guryan exemplifies the logic of examining school finance reform. In his work on Massachusetts schools, he first calculates the fraction of funding passed through to schools as a result of a particular Massachusetts policy change and then looks at the effect of this change in funding on student test scores. The current analysis, because of the more gradual and cumulative nature of policy changes, is not the right venue to employ a technique of regression discontinuity design in which Guryan employs a simple pre-and post-reform structure to test for the effect of policy changes on changes in school funding. In the current analysis, there was an initial change in funding followed by several later changes and amendments to the funding structure.

15. In September 2006, the Kansas Legislative Research Department published a document detailing in great specificity the particular changes made to the School District Finance and Quality Performance Act. As can be seen from this document, and as noted later on in this article, the majority of significant changes to the act were enacted for the 2005 school year. See: Kansas Legislative Research Department, 2006.

16. In conversations with Kansas legislators, it became apparent that the most important and recent changes to this act were in targeting special education students and at-risk student populations. Because of the nature of the current analysis, targeting at-risk students will clearly affect empirical results.

17. Notice that the effects of legislation on changes in per pupil revenues are not tested per se. Although trends in revenues per student (unadjusted for other factors in districts changing over time) can be seen,

employed in the current analysis comprise the longest time frame which can reasonably be used to capture the effects of the amendments to the act rather than the act itself.[18]

DATA

Information on school district level measures of student achievement including test scores, graduation rates, and dropout rates come from the Kansas State Department Board of Education (KSDE).[19] Subject test scores used are math, reading, science, and social studies. Specifically, the test scores provide information on the percentage proficient in each grade. Information on school district characteristics, revenues per student, as well as an alternative measure of student achievement—the diploma rate—come from the National Center for Education Statistics (NCES).[20] Multiple measures of student persistence are employed in the analysis because these measures were compiled by different agencies and will, therefore, serve as a robustness check in the analysis. The population for the analysis includes all school districts in the state of Kansas.

The general period covered in this analysis is the 1997–2006 time period. While data for student persistence—dropout rates, fraction receiving diplomas, and the graduation rate—are available for the full time period in question, data for reading and math test scores are only available for the years 2004–2006. Data for test scores in science and social science are available for the years 2003 and 2005, and for 2005 respectively. The reason for the piecemeal nature of the test-score data is that test scores were not uniformly administered each year, and only certain test scores were administered in each particular year for all school districts. It is also not possible to employ earlier test-score data due to a change in the nature of testing in Kansas and tests prior to 2004, which are

this is not a formal element of the analysis. The main purpose of discussing the nature of legislative processes over this time period is to motivate the empirical analysis of the effect of total revenues per student on student achievement, rather than constituting a separate part of the analysis interesting in its own right. In some sense, due to the gradual nature of the enactment of the amendments, it would be much more difficult to determine the effect of amendments on actual changes in revenues per student. This is in contrast to a more structural approach or a regression discontinuity approach as discussed in earlier papers.

18. In order to not capture the effects of the act, but rather only the effect of its amendments, the data was chosen to begin with the year 1997. The latest data currently available to allow for a consistent end date for all measures was the year 2006. For the test-score data, not all tests were given in each of the relevant years; however, the two main tests (reading and mathematics) were given in the years 2004, 2005, and 2006 so the differencing portion of the analysis uses the 2004 and 2006 years for the analysis.

19. To be precise, the fraction of students receiving diplomas is used as the outcome measure of persistence. This measure is constructed for school district k in year l as:

$$Fraction\_Diplomas_{k,l} = \frac{\#\ Diplomas\_Awarded_{k,l}}{Enrollment\_Grade\_12_{k,l}}$$

20. CPI estimates are used to correct total revenues per student for inflation. The year 1997 is arbitrarily chosen to have the basket price of 100.

not comparable in nature to tests in 2004 and afterwards. For this reason, while the test score portion of the analysis provides an interesting counterpoint to the persistence rate analysis, it is the persistence rates that represent the longer time frame upon which to base results of the analysis and are, therefore, the more interesting portion of the analysis.

METHODOLOGY

Estimation begins with a cross-sectional Ordinary Least Squares (OLS) regression analysis of the effect of total revenues per student on measures of persistence after including school district characteristics as control variables. The use of a regression analysis is employed because it allows for a determination of the independent effect of each of the right-hand side variables on the left-hand side variable. In this analysis, the left-hand side, or the "outcome" variable in each of the various cross-sectional regressions is represented by the various different measures of student achievement and persistence, while the right-hand side variables are total revenue per student and school-district characteristics. Specifically, the model employed is for school district *k in year l*:

$$Persistence_{k,l} = \beta_0 + \beta_1 TRS_{k,l} + \beta_2 DistSchls_{k,l} + \beta_3 DistPop_k + \varepsilon_{k,l} \quad (1)$$

Where *Persistence* is either the dropout rate, the fraction receiving diplomas, or the graduation rate; *TRS* is total revenues per student; *DistSchls* are the variables describing the school district, i.e. the pupil teacher ratio, the fraction on free lunch, the number of full-time equivalent teachers, and total enrollment; *DistPop* are the variables describing the population in the school district, i.e. the fraction of 5–17 year olds under the poverty line, median family income, the fraction of males and of females in the labor force, and fraction of individuals with varying levels of education.[21]

The standard assumption used throughout the analysis is that a school district's quality is proxied by its observable characteristics.[22] Since student achievement is affected by school district quality, and the measure of total revenues per student will also be related to school district characteristics, leaving school district characteristics out of the regression will cause a biased measure of the relationship between persistence and total revenues per student. It is for this reason that characteristics of school districts are essential to include in the

21. As seen in Table 2, the education variable is broken into the fraction of individuals who have (1) a college degree or higher, (2) associate degree, (3) high school diploma or GED, (4) between 9 and 12 years schooling but no diploma, (5) between 1 and 8 years of schooling, (6) no schooling—the omitted category.

22. The chosen set of school district characteristics are standard in their use in the literature and should, in all likelihood, capture the major characteristics of school districts that are relevant for inclusion in the present analysis.

analysis to determine the independent effect of total revenues per student on student achievement, represented by β_1 in the previous regression. In this and all later parts of the analysis heteroskedasticity-robust standard errors are used.

When using school district test scores—percentage proficient in the grade—as the outcome measure, the same previous general structure was employed. One change, however, is that regressions are run separately for each district *k*, year *i*, and grade *g*. Specifically,

$$TestScore_{k,i,g} = \beta_0 + \beta_1 TRS_{k,i} + \beta_2 DistSchls_{k,i} + \beta_3 DistPop_k + \varepsilon_{k,i,g} \quad (2)$$

Although the cross-sectional regressions are used to determine the initial relationship between measures of school funding and measures of student achievement and persistence, they will clearly produce biased regression coefficients, i.e. despite the fact that controls for school district quality have been included, the true independent effect of total revenues per student on test scores will not have been captured. The reason is that—among other problems of selection bias—parents will choose a location to live in based on their own socioeconomic status (SES) and desire for their children to do well in school. In order to alleviate this issue of selection bias, a differences structure is next employed to determine how changes in school funding are related to changes in student achievement. To explain the meaning of the differences regression in this context, it is useful to contrast the baseline OLS regression just employed to the differences OLS regression which is next employed. In the baseline regression, the left-hand side variable are used as the level of student achievement in the district, and on the right-hand side are the levels of total revenues per student and school district characteristics. The difference regression instead uses the change in student achievement between an initial and a final year—for persistence this is 1997–2006 changes and for test scores this is 2004–2006 changes—as the left-hand side variable and changes in total revenues per student, as well as changes in school district characteristics as the right-hand side variables. The initial level of school district characteristics—1997 levels for persistence, and 2004 levels for test scores—are also included to allow for a nonlinear relationship between student achievement and school quality. The described methodology is parallel in structure to that used in J. Deke (2003). The reasoning for using this type of analysis is as follows: Major changes to school funding which occurred during this time period were due to amendments to the School District Finance and Quality Performance Act. This act was redistributive in nature so an increase in school funding would have occurred for lower-performing schools. In this way, focusing on a "flow" analysis will alleviate the issue of selection bias since parents will not be selecting school locations conditional on the same characteristics

which are causing schools to see increases in total revenues per student.[23]

The full regression used to determine the effect of total revenues per student on persistence for school district k between years i and j is thus:

$$\Delta Persistence_k = \gamma_0 + \gamma_1 \Delta TRS_k + \gamma_2 \Delta DistSchls_k + \gamma_3 DistSchls_{k,i} + \gamma_4 DistPop_k + u_k \quad (3)$$

where

$$\Delta TRS_k = TRS_{k,j} - TRS_{k,i}$$
$$\Delta Persistence_k = Persistence_{k,j} - Persistence_{k,i}$$
$$\Delta DistSchls_k = DistSchls_{k,j} - DistSchls_{k,i}$$

In the regressions, the initial level of *DistSchls* is used to allow for the possibility of a nonlinear relationship between *DistSchls* and *Persistence,* i.e. initial levels of school district characteristics are employed in the analysis. Only the level—and not the differenced amount—of *DistPop* is used because it comes from Census 2000 data where only one year of data is available.

Once again, test-score information follows the same relationship for the outcomes of reading and math proficiency. Specifically, for grade g in school district k between years i and j:

$$\Delta TestScore_{g,k} = \gamma_0 + \gamma_1 \Delta TRS_k + \gamma_2 \Delta DistSchls_k + \gamma_3 DistSchls_{k,i} + \gamma_4 DistPop_k + u_{g,k} \quad (4)$$

where

$$\Delta TRS_k = TRS_{k,j} - TRS_{k,i}$$
$$\Delta TestScore_{g,k} = TestScore_{g,k,j} - TestScore_{g,k,i}$$
$$\Delta DistSchls_k = DistSchls_{k,j} - DistSchls_{k,i}$$

A limitation of the analysis is the short timeframe for which the differencing analysis is available for test-score data. The differencing regressions where test scores are an outcome, therefore, serve as an interesting additional result; however, the more consistent measures of student achievement in the differencing portion of the analysis are student persistence.[24]

Robustness Analysis

To allow for the possibility that results were affected by censoring of observations, all regressions were additionally run using a Tobit regression structure. The initial assumptions regarding variables to be included in the analysis are all the same as in the previous sections; however, now the additional condition of censoring is allowed for in the analysis. Tobit regression is allowable in cross-sectional

23. If anything, the effect of per pupil revenues on achievement will be underestimated since presumably higher achieving students are moving away from increasing school funding during this time period. This will be true if the only changes in funding were due to the amendments. This should be kept in mind when analyzing the final results of the analysis.

24. It is also possible that the act will go into effect with a time lag and, for this reason, the effect of the last year or two of amendments may not fully show up if they require more than a year or two to fully take effect. This is a possibility that should be noted in any estimation of results of an educational act and is again noted in the conclusion.

regressions as well as in difference regressions in keeping with the structure in the earlier section. A Tobit analysis was especially relevant in the cases where top censoring might be suspected. Generally, the Tobit regression structure assumes there is a latent outcome variable y^* such that the true regression should be: $y^* = \beta X + \varepsilon, \quad \varepsilon \mid X \sim N(0, \sigma^2)$. In reality, only y is observed where $y=max(0, y^*)$ in bottom-censored regressions and alternatively $y=min(1, y^*)$ in top-censored regressions such as those in the present analysis.

RESULTS AND FINDINGS

Summary Statistics

Table 1 displays trends over time in total revenues per student, student persistence, student test scores, and school district educational characteristics. Overall, Table 1 paints a picture of school districts increasing funding and resources for students and those experiencing higher rates of persistence and achievement on test scores. To conclude that these relationships were more than correlations, however, requires significant analysis beyond these simple summary statistics. It is for this reason that after describing the summary statistics in Table 1 and Table 2 in more detail, results from the regression analysis allowing for a more in-depth analysis will be discussed.

In Table 1, inflation-adjusted total revenues per student increased over this time period from approximately $7,500 per student in 1997 to $9,400 per student in 2006. The one clear exception to the upwards trend in per pupil revenues was a sharp decrease in the year 2004.

Measures of persistence have also been exhibiting a generally consistent trend of students improving over time, with the fraction receiving diplomas and the graduation rate increasing and the dropout rate decreasing. For instance, the graduation rate went from 89.7% in 1997 to 91.3% in 2006. Similarly, the fraction receiving diplomas went from 94% in 1997 up to 97% in 2006. Dropout rates also display a generally downward trend from the period 1997–2004 going from 1.7 dropouts per 100 students in the year 1997 to a low of 0.92 dropouts per 100 students in 2004; however, they did display a sharp increase in the 2005–2006 time period. In terms of test scores, the data for math and reading is somewhat more limited, containing information only for the 2004–2006 time period. For this three-year period of time, student test scores in both math and reading were generally increasing over time for all relevant grades.

During the 1997–2006 time period, school districts experienced a movement towards higher enrollment levels, more full-time equivalent teachers, and a lower

Table 1. KS School District Characteristics 1997–2006

Panel A: School District Information	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Revenues Per Student (in thousands)	7.5	7.7	7.8	7.9	7.8	8.3	8.7	8.4	8.7	9.4
Pupil Teacher Ratio	13.5	13.3	13.1	12.9	13.0	13.2	13.1	13.0	12.8	12.3
Full Time Equivalent Teachers	103.3	104.8	108.0	107.3	108.4	107.4	107.8	109.3	111.9	115.2
Total Enrollment	1536	1539	1538	1536	1543	1548	1556	1562	1564	1599
Fraction Free Lunch	0.33	0.33	0.21	0.22	0.22	0.24	0.25	0.26	0.26	0.26
Panel B: Persistence Information	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Dropout Rate	1.7	1.6	1.5	1.4	1.3	1.0	0.9	0.9	1.5	1.6
Graduation Rate	89.7	89.8	89.9	90.6	91.1	89.2	90.0	91.7	91.1	91.3
Fraction Diplomas	0.94	0.94	0.94	0.94	0.94	0.93	0.94	0.95	0.95	0.97
Panel C: Proficiency Rates	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Grade 4 Math	-	-	-	-	-	-	-	82.7	87.5	83.5
Grade 7 Math	-	-	-	-	-	-	-	68.0	72.1	72.5
Grade 10 Math	-	-	-	-	-	-	-	52.0	54.8	61.0
Grade 5 Reading	-	-	-	-	-	-	-	71.7	78.1	79.8
Grade 8 Reading	-	-	-	-	-	-	-	75.4	78.8	80.4
Grade 11 Reading	-	-	-	-	-	-	-	63.0	65.8	79.1

pupil-teacher ratio. The fraction on free lunch declined from the higher levels (33%) from the 1997–1998 period, but rose somewhat from the 1999–2001 lows (21–22%). The number of full-time equivalent teachers went from a low of 103.3 in 1997 to a high of 115.2 in 2006, while the average enrollment went from a low of 1,536 students in 1997 to a high of 1,599 students in 2009.

The remaining information on school districts, which was used in this analysis comes from Census 2000 data. Table 2 displays these characteristics of Kansas school districts. It is apparent that just over 10% of students in Kansas school districts were living below the poverty line with a low of approximately 0% and a high of almost 40% living in poverty. The median income in the typical Kansas school districts stood at approximately $44,000 with some districts having a median income as high as $100,000, and others as low as $30,000. It is also true that approximately 80% of Kansans had at least a high school diploma and 72% of men were participating in the labor force—as were 58% of women. These local characteristics of school districts are used as control characteristics in the following regressions.

Table 2. Kansas School District Characteristics (Census 2000)

	Mean	S.D.	Minimum	Maximum
Fraction of Children in Poverty	0.11	0.06	0.00	0.40
Median Family Income	44005	8724	31100	102987
Fraction Males in Labor Force	0.72	0.06	0.37	0.92
Fraction Females in Labor Force	0.58	0.06	0.44	0.73
Fraction with College Degree	0.24	0.09	0.07	0.79
Fraction with Associates Degree	0.08	0.03	0.02	0.16
Fraction Who are HS Grads	0.47	0.08	0.12	0.67
Fraction 9-12 Years School	0.12	0.04	0.02	0.24
Fraction 1-8 Years School	0.08	0.05	0.01	0.40

Regression Analysis

Table 3 displays cross-sectional regressions where each of the outcome variables chosen consists of various measures of persistence in panel A and various test scores by grade in panel B. The panel A regressions are run for each of the years 1997–2006 and the panel B regressions are run for selected years in the 2003–2006 range with the years chosen depending on availability of a particular test in a given year. The regressions progressively add in the *DistPopul* and *DistEduc* controls—labeled CENSUS INFO and SCHOOL INFO respectively. This means that only the last column of each persistence regression for each year corresponds to equation (1), and only the last column of each test-score regression for each grade-year combination corresponds to equation (2). Due to this structure, Panel A contains the results from 90 separate regressions, while panel B contains the results of 82 separate regressions for a sum total of 172 separate regressions in Table 3.

Panel A of Table 3 displays the effects of total revenues per student on the three measures of student persistence in the data—namely the fraction receiving diplomas, the dropout rate, and the graduation rate. These regressions show that an increase in total revenues per student serves to improve student persistence (i.e. lower dropouts and improved graduation rates and the fraction receiving diplomas) as evidenced by the generally positive coefficients on total revenues per student in the regressions for the fraction receiving diplomas and graduation rates, and the negative coefficients for the regression where dropout rate is the left-hand side variable. This provides some slight evidence for positive effects of revenues on persistence, but they do not appear to remain when including district controls. One anomaly is the negative and significant relationship

Table 3. Effect of Total Revenues Per Student on Persistence and Test Scores

Panel A: Persistence 1997–2006

	1997			1998			1999			2000			2001			2002			2003			2004			2005			2006		
CENSUS INFO	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES
SCHOOL INFO	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES
Graduation Rate		1997			1998			1999			2000			2001			2002			2003			2004			2005			2006	
Tot. Rev. Per Stu.	0.984	0.793	0.098	1.123	0.799	0.173	1.255	0.752	0.146	0.928	0.375	-0.144	1.018	0.478	-0.112	0.279	-0.643	-1.043	1.082	0.813	1.144	-0.621	-0.76	-0.235	-1.579	-1.841	-2.159	0.38	0.259	-0.25
	[2.85]**	[3.28]**	[0.39]	[3.05]**	[2.23]*	[0.46]	[2.77]**	[1.71]	[0.24]	[2.90]**	[1.39]	[0.40]	[2.27]*	[1.01]	[0.19]	[0.31]	[0.61]	[0.78]	[2.36]*	[1.86]	[2.30]*	[0.61]	[0.67]	[0.23]	[1.22]	[1.14]	[1.30]	[0.95]	[0.70]	[0.54]
Observations	280	280	280	279	279	277	280	280	280	280	280	280	279	279	279	283	283	283	283	283	283	283	283	283	284	284	283	275	275	275
Dropout Rate		1997			1998			1999			2000			2001			2002			2003			2004			2005			2006	
Tot. Rev. Per Stu.	-0.174	-0.127	-0.043	-0.172	-0.13	-0.014	-0.13	-0.073	-0.021	-0.045	0.002	0.003	-0.245	-0.202	-0.078	-0.177	-0.139	-0.112	-0.125	-0.131	-0.068	-0.132	-0.126	-0.066	0.226	0.335	0.984	-0.041	-0.028	0.139
	[2.52]*	[2.00]*	[0.65]	[2.86]**	[2.37]*	[0.22]	[1.86]	[1.10]	[0.23]	[0.78]	[0.04]	[0.04]	[3.81]**	[3.27]**	[0.93]	[3.06]**	[2.39]*	[1.81]	[3.53]**	[3.46]**	[1.52]	[3.39]**	[3.03]**	[1.59]	[0.75]	[0.78]	[1.08]	[2.18]*	[1.70]	[0.94]
Observations	283	283	283	283	283	281	283	283	283	283	283	283	282	282	282	282	282	282	282	282	282	281	281	281	281	281	281	282	282	281
Receiving Diplomas		1997			1998			1999			2000			2001			2002			2003			2004			2005			2006	
Tot. Rev. Per Stu.	0.004	-0.001	-0.006	0.009	0.006	0.003	0.004	0	-0.005	0.005	0	-0.004	0.011	0.01	0.009	0.016	0.011	0.01	0.017	0.012	0.012	0.009	0.009	0.019	0.024	0.036	0.069	0.018	0.023	0.037
	[1.15]	[0.36]	[2.39]*	[2.04]*	[1.61]	[0.57]	[1.32]	[0.14]	[1.04]	[1.98]*	[0.00]	[1.09]	[2.24]*	[1.73]	[1.51]	[4.33]**	[2.72]**	[1.80]	[2.92]**	[2.74]**	[2.05]*	[2.83]**	[2.35]*	[1.63]	[1.27]	[1.32]	[1.16]	[1.68]	[1.97]*	[1.96]
Observations	296	296	296	295	295	293	296	296	296	296	296	296	293	293	293	291	291	291	286	286	286	288	288	288	283	283	282	277	277	277

Panel B: Test Scores Selected Years

CENSUS INFO	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES			
SCHOOL INFO	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES			
	Mathematics—Grade 4									Reading—Grade 5									Science—Grade 4						Social Science—Grade 6					
Tot. Rev. Per Stu.		2004			2005			2006			2004			2005			2006			2003			2005			2003				
	1.163	1.018	1.159	0.654	1.085	1.239	0.01	0.262	0.004	-0.743	-0.646	-0.51	-0.063	0.13	0.136	-0.339	0.233	0.385	0.957	1.104	0.677	1.043	1.355	0.288	1.27	1.497	1.545			
Observations	[2.35]*	[1.80]	[1.83]	[1.04]	[1.61]	[1.50]	[0.01]	[0.38]	[0.01]	[1.23]	[1.01]	[0.72]	[0.11]	[0.22]	[0.20]	[0.51]	[0.38]	[0.57]	[1.00]	[1.08]	[0.62]	[1.11]	[1.53]	[0.29]	[1.50]	[1.65]	[1.62]			
	269	269	269	270	270	270	269	269	269	270	270	270	271	271	271	270	270	270	267	267	267	270	270	270	267	267	267			
	Mathematics—Grade 7									Reading—Grade 8									Science—Grade 7						Social Science—Grade 8					
Tot. Rev. Per Stu.		2004			2005			2006			2004			2005			2006			2003			2005			2003				
	1.212	1.222	0.865	-0.172	0.059	0.162	-0.285	-0.248	0.428	0.821	1.088	0.863	-0.132	0.711	0.675	-0.181	0.241	0.118	1.724	1.677	0.875	0.992	1.29	0.505	0.614	0.993	0.911			
Observations	[2.03]*	[1.79]	[1.13]	[0.21]	[0.06]	[0.14]	[0.49]	[0.39]	[0.56]	[1.55]	[2.52]*	[2.02]*	[0.21]	[1.07]	[0.81]	[0.26]	[0.28]	[0.11]	[2.28]*	[2.43]*	[1.18]	[1.33]	[1.53]	[0.61]	[0.65]	[1.23]	[1.10]			
	273	273	273	273	273	273	269	269	269	275	275	275	271	271	271	274	274	274	274	274	274	273	273	273	272	272	272			
	Mathematics—Grade 10									Reading—Grade 11									Science—Grade 10						Social Science—Grade 11					
Tot. Rev. Per Stu.		2004			2005			2006			2004			2005			2006			2003			2005			2003				
	0.471	-0.412	-1.083	-1.601	-1.832	-2.102	0.596	0.541	-0.533	1.181	1.022	0.602	0.153	0.227	-0.017	0.434	0.019	-0.616	2.08	1.793	1.106	-0.151	-0.135	-0.471	0.141	-0.149	-0.212			
Observations	[0.60]	[0.62]	[1.67]	[1.23]	[1.13]	[1.22]	[0.73]	[0.59]	[0.49]	[2.16]*	[1.90]	[1.09]	[0.30]	[0.43]	[0.03]	[0.69]	[0.03]	[0.57]	[2.88]**	[2.53]*	[1.55]	[0.15]	[0.12]	[0.37]	[0.16]	[0.17]	[0.22]			
	273	273	273	272	272	272	276	276	276	274	274	274	268	268	268	272	272	272	274	274	274	272	272	272	271	271	271			

Note: Test score regressions are run at the grade level for each school district while persistence regressions are run at the school district level. Robust standard errors are employed in all regressions. CENSUS INFO includes district level averages for the fraction of women and men in the labor force, Median family income, the fraction of children living in poverty and the fraction of individuals in each of five educational groups (1–8, 9–12, High School Degree, Associate Degree, College or more). SCHOOL INFO includes the pupil teacher ratio, the school enrollment, the number of full time equivalent teachers, and the fraction of students on free lunch. Absolute values of t-statistics in brackets. *Significant at 5% level. **Significant at 1% level.

between total revenues per student and the fraction receiving diplomas for the fully controlled regression in the year 1997. Since this pattern does not persist in the data and is not evident at all until including district controls, this particular anomaly does not appear to be a relevant concern.

Panel B of Table 3 shows that there was also little effect of total revenues per student on student test scores in any grade. Once again, only one of the regressions with both district controls exhibits a significant coefficient on total revenues per student. This is the case for reading in grade 8 in 2004. In all other cases, any initial positive effects—present mostly in 2004 and to some extent in 2003—disappear when adding district controls to the regression. Overall, at the cross-sectional level there does not appear to be a significant effect of total revenues per student on test scores or persistence after controlling for school district characteristics. It is also important to note that there is more evidence for the persistence than the test-scores regressions due to the longer timeframe for the persistence regressions.

Although the numerical coefficients on the school district control variables used in the Tables are not displayed due to space constraints, it is interesting to note the presence or absence of significant relationships between them and the outcome measures. In these cross-sectional regressions, pupil-teacher ratios, number of full-time equivalent teachers, enrollment, and fraction on free lunch all have significant relationships with the dropout and graduation rate. Of these school district controls, only the pupil-teacher ratio is significantly related to the fraction receiving diplomas in any instance. There are also some instances where there is a significant relationship between both (a) average level of schooling in the district and (b) the number of five- to seventeen-year olds below the poverty line with measures of persistence. The importance of these Census measures of district characteristics on persistence should not be exaggerated since they disappear when also controlling for the pupil-teacher ratio, full-time equivalent teachers, enrollment, and fraction on free lunch.

In terms of the cross-sectional test-score regressions, in almost no instances are any of the pupil-teacher ratio, full-time equivalent teachers, or enrollment significantly related to student test scores. The only variable that is significantly related to test scores in multiple regressions is the fraction on free lunch. In a similar vein, there is sometimes an effect of median family income and the number of five- to seventeen-year olds below the poverty line on test scores. These effects appear more prevalent for measures of science test scores than any of math, reading, or social science.

Taken together, there is some evidence of a relationship between the district controls and persistence in the cross section, particularly for district measures related to the schools themselves, that is, pupil-teacher ratio, enrollment, fraction

on free lunch, and number of full-time equivalent teachers. These relationships are weaker when examining the relationship on test scores and tend to focus slightly more on the income variables—median family income, fraction on free lunch, and number in poverty.

Table 4 displays results from the Tobit regression where the structure is exactly the same as in Table 3. As Panels A and B of Table 4 demonstrate, there is no substantive change in the patterns we see in Table 4 after using this robustness analysis. The one notable departure in Table 4 is the higher levels of significance for the Tobit cross-sectional persistence regressions including district level controls. This is particularly true for the fraction receiving diplomas and for the dropout rate in the last several years of the data. One anomaly of note is the positive effect of revenues on dropouts—the opposite direction expected—in 2005. The reasoning for this anomaly could be as follows: The majority of amendment changes went into effect during the 2005 school year, and there was a significant change in the amount of funding which was targeted towards at-risk students during the 2005 school year, so it is natural to observe that total revenues per student have a negative effect on dropout rate since funding was directed towards schools that were doing poorly at this point in time. It is also possible, as evidence from Table 4, that this increase in funding would not affect graduation rates or the fraction receiving diplomas in an adverse fashion during that year. The longitudinal regressions which follow, however, do help to explain in more detail how funding changes affected schools over the entire time period.

The other piece of information gleaned from these regressions is the decrease in the significance of the effect of total revenues per student on the test-score regressions. This provides even more evidence that revenues were not helping to improve test scores.

It would be unwise to finish the analysis at this point, for the reason mentioned earlier regarding possible selection bias and it is, therefore, necessary to obtain results from the differenced regression in Table 5, as well as the Tobit analysis difference regression in Table 6.

Table 5 displays results of the difference regressions and shows how changes in revenues per student affected changes in persistence (in panel A) and changes in math and reading scores (in panel B). Panel A displays results from all three types of persistence of interest, while Panel B results are broken out by grade of analysis. Once again, controls are added in progressively as well as including initial levels of the school district characteristics so that for panel A, only the fourth column of each set of persistence regressions corresponds to the regression specified in equation (3), and in panel B the fourth column of each grade-subject combination corresponds to the regression specified in equation (4). The structure of Table 5 means that there are 12 separate regressions

Table 4. Tobit Regressions Effect of Total Revenues Per Student on Persistence and Test Scores

Panel A: Persistence 1997–2006

CENSUS INFO	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES
SCHOOL INFO	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES
Graduation Rate	1997			1998			1999			2000			2001			2002			2003			2004			2005			2006		
Tot. Rev. Per Stu.	1.224	0.924	0.063	1.459	0.977	0.111	1.629	0.967	0.208	1.268	0.586	-0.203	1.509	0.787	-0.069	1.161	-0.009	-0.664	2.612	2.074	2.018	0.494	-0.045	0.317	-0.399	-1.038	-1.75	0.949	0.651	-0.152
	[3.77]**	[3.14]**	[0.19]	[3.87]**	[2.79]**	[0.26]	[3.78]**	[2.44]*	[0.42]	[3.16]**	[1.58]	[0.42]	[2.99]**	[1.55]	[0.11]	[1.45]	[0.01]	[0.69]	[3.28]**	[2.51]*	[2.19]*	[0.64]	[0.06]	[0.34]	[0.51]	[1.27]	[1.82]	[1.91]	[1.27]	[0.25]
Observations	280	280	280	279	279	277	280	280	280	280	280	280	279	279	279	283	283	283	283	283	283	283	283	283	284	284	283	275	275	275
Dropout Rate	1997			1998			1999			2000			2001			2002			2003			2004			2005			2006		
Tot. Rev. Per Stu.	-0.22	-0.153	-0.043	-0.236	-0.17	-0.011	-0.229	-0.136	-0.048	-0.097	-0.035	-0.008	-0.375	-0.302	-0.14	-0.303	-0.234	-0.184	-0.213	-0.215	-0.119	-0.278	-0.265	-0.162	-0.25	-0.051	0.773	-0.518	-0.337	0.121
	[4.03]**	[3.10]**	[0.77]	[3.85]**	[2.90]**	[0.15]	[2.88]**	[1.72]	[0.45]	[1.64]	[0.58]	[0.10]	[4.71]**	[3.74]**	[1.46]	[4.66]**	[3.42]**	[2.44]*	[3.77]**	[3.56]**	[1.97]*	[4.36]**	[3.88]**	[2.17]*	[1.02]	[0.19]	[2.78]**	[2.08]*	[1.27]	[0.91]
Observations	283	283	283	283	283	281	283	283	283	283	283	283	282	282	282	282	282	282	282	282	282	281	281	281	281	281	281	282	282	281
Receiving Diplomas	1997			1998			1999			2000			2001			2002			2003			2004			2005			2006		
Tot. Rev. Per Stu.	0.004	-0.001	-0.006	0.009	0.006	0.003	0.004	-0.001	-0.005	0.005	0	-0.004	0.011	0.01	0.009	0.016	0.011	0.01	0.017	0.013	0.012	0.009	0.009	0.019	0.024	0.036	0.069	0.018	0.024	0.037
	[1.45]	[0.29]	[1.66]	[2.27]*	[1.49]	[0.54]	[1.30]	[0.15]	[1.01]	[1.89]	[0.02]	[0.98]	[2.72]**	[2.37]*	[1.57]	[4.01]**	[2.52]*	[1.86]	[3.78]**	[2.74]**	[2.16]*	[1.89]	[1.88]	[3.20]**	[1.96]	[2.61]**	[4.59]**	[2.20]*	[2.61]**	[3.41]**
Observations	296	296	296	295	295	293	296	296	296	296	296	296	293	293	293	291	291	291	286	286	286	288	288	288	283	283	282	277	277	277

Panel B: Test Scores Selected Years

CENSUS INFO	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES			
SCHOOL INFO	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES			
	Mathematics—Grade 4									Reading—Grade 5									Science—Grade 4						Social Science—Grade 6					
Tot. Rev. Per Stu.	2004			2005			2006			2004			2005			2006			2003			2005			2003					
	1.182	1.036	1.154	0.691	1.125	1.257	0.009	0.242	-0.018	-0.731	-0.633	-0.496	0.015	0.184	0.153	-0.349	0.245	0.382	0.945	1.094	0.654	1.102	1.394	0.268	1.285	1.491	1.462			
Observations	[1.87]	[1.53]	[1.54]	[1.17]	[1.78]	[1.71]	[0.02]	[0.42]	[0.03]	[1.07]	[0.87]	[0.62]	[0.03]	[0.30]	[0.22]	[0.64]	[0.42]	[0.61]	[1.12]	[1.30]	[0.72]	[1.46]	[1.76]	[0.30]	[1.56]	[1.79]	[1.67]			
	269	269	269	270	270	270	269	269	269	270	270	270	271	271	271	270	270	270	267	267	267	270	270	270	267	267	267			
	Mathematics—Grade 7									Reading—Grade 8									Science—Grade 7						Social Science—Grade 8					
Tot. Rev. Per Stu.	2004			2005			2006			2004			2005			2006			2003			2005			2003					
	1.224	1.221	0.862	-0.128	0.071	0.168	-0.287	-0.238	0.417	0.811	1.074	0.852	-0.124	0.724	0.684	-0.057	0.347	0.211	1.724	1.666	0.744	1.041	1.314	0.496	0.58	0.958	0.877			
Observations	[1.73]	[1.75]	[1.13]	[0.20]	[0.11]	[0.22]	[0.41]	[0.33]	[0.50]	[1.46]	[2.02]*	[1.46]	[0.21]	[1.15]	[0.93]	[0.11]	[0.64]	[0.34]	[2.20]*	[2.25]*	[0.92]	[1.84]	[2.34]*	[0.81]	[0.68]	[1.12]	[0.93]			
	273	273	273	273	273	273	269	269	269	275	275	275	271	271	271	274	274	274	274	274	274	273	273	273	272	272	272			
	Mathematics—Grade 10									Reading—Grade 11									Science—Grade 10						Social Science—Grade 11					
Tot. Rev. Per Stu.	2004			2005			2006			2004			2005			2006			2003			2005			2003					
	0.449	-0.428	-1.098	0.547	0.108	-0.649	0.609	0.541	-0.593	1.191	1.034	0.608	0.163	0.229	-0.019	0.425	0.005	-0.648	2.096	1.79	0.94	-0.165	-0.146	-0.481	0.201	-0.09	-0.157			
Observations	[0.64]	[0.61]	[1.46]	[0.80]	[0.16]	[0.83]	[0.91]	[0.80]	[0.77]	[2.13]*	[1.82]	[0.97]	[0.31]	[0.43]	[0.03]	[0.74]	[0.01]	[0.98]	[2.83]**	[2.47]*	[1.19]	[0.24]	[0.21]	[0.64]	[0.25]	[0.11]	[0.17]			
	273	273	273	272	272	272	276	276	276	274	274	274	268	268	268	272	272	272	274	274	274	272	272	272	271	271	271			

Note: Test score regressions are run at the grade level for each school district while persistence regressions are run at the school district level. Robust standard errors are employed in all regressions. CENSUS INFO includes district level averages for the fraction of women and men in the labor force, Median family income, the fraction of children living in poverty and the fraction of individuals in each of five educational groups (1–8, 9–12, High School Degree, Associate Degree, College or more). SCHOOL INFO includes the pupil teacher ratio, the school enrollment, the number of full time equivalent teachers, and the fraction of students on free lunch. Absolute values of t-statistics in brackets. *Significant at 5% level. **Significant at 1% level.

Table 5. Effect of Changes in Revenue per Student on Student Outcomes

Panel A: Effect on Persistence (2006–1997)												
CENSUS INFO	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
SCHOOL INFO	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
INITIAL LEVELS	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
	Change in Dropout Rate				Change in Grad Rate				Change in Frac Diplomas			
Change Rev. per Stu.	-0.09	-0.106	0.12	0.196	-0.18	-0.222	0.084	-0.005	0.014	0.014	0.011	0.023
	[0.64]	[0.68]	[1.21]	[1.32]	[0.44]	[0.53]	[0.19]	[0.01]	[0.84]	[0.92]	[0.75]	[1.28]
Observations	281	281	281	281	280	280	280	280	292	292	292	292
Panel B: Effect on Test Scores (2006–2004)												
CENSUS INFO	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
SCHOOL INFO	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
INITIAL LEVELS	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
	Change in Math Scores											
	4th Grade				7th Grade				10th Grade			
Change Rev. Per Stu.	-1.05	-0.848	-0.514	-1	1.06	1.335	0.626	1.834	1.888	2.532	1.241	1.769
(2006–2004)	[0.77]	[0.62]	[0.37]	[0.78]	[0.86]	[0.94]	[0.42]	[1.21]	[1.35]	[1.71]	[0.74]	[0.96]
Observations	264	264	264	264	265	265	265	265	271	271	271	271
	Change in Reading Scores											
	5th Grade				8th Grade				11th Grade			
Change Rev. Per Stu.	-1.049	-1.285	-1.045	-0.64	-0.97	-1.391	-2.087	-1.812	0.372	0.329	-0.73	-1.12
(2006–2004)	[1.06]	[1.18]	[0.89]	[0.56]	[0.73]	[1.01]	[1.48]	[1.06]	[0.38]	[0.32]	[0.65]	[1.01]
Observations	265	265	265	265	270	270	270	270	267	267	267	267

NOTE: Test score regressions are run at the grade level for each school district while Persistence regressions are run at the school district level. Robust standard errors are employed in all regressions. CENSUS INFO includes district level averages for the fraction of women and men in the labor force, Median family income, the fraction of children living in poverty and the fraction of individuals in each of five educational groups (1–8, 9–12, High School Degree, Associate Degree, College or more). SCHOOL INFO includes changes between 1997 or 2004 (for persistence or test scores respectively) and 2006 which occurred in the following variables: the pupil teacher ratio, the school enrollment, the number of full time equivalent teachers, and the fraction of students on free lunch. For the test score regressions, INITIAL LEVELS includes information on the 2004 values of the pupil teacher ratio, the school enrollment, the number of full time equivalent teachers, and the fraction of students on free lunch while the persistence regressions INITIAL LEVELS includes the 1997 values for these same variables. Absolute values of t-statistics in brackets. *Significant at 5% level. **Significant at 1% level.

contained in panel A, and 24 separate regressions contained in panel B for a total of 36 separate regressions in Table 5.

Panel A of Table 5 shows no regressions where the effect of changes in total revenues per student on changes in persistence reaches conventional levels of statistical significance. Similarly, panel B of Table 5 shows no regressions where the effect of changes in total revenues per student on changes in test scores reaches conventional levels of statistical significance—either positive or negative. Both panels of Table 5 support the idea that there is little or no effect of total revenues per student over this time period on persistence or test scores. Once again, the results for test scores are measured over a three-year time period while persistence is measured over a 10-year time period and so are more trustworthy in nature.

Once again, it is useful to note which of the control variables were significantly related to measures of change in persistence and test scores. In panel A, there were almost no regressions where there was a significant relationship between the control variables and changes in persistence. The only variables which displayed at times significant coefficients were the initial level of full-time equivalent teachers or the average level of schooling attained in the school district in Census 2000 data. In panel B, only changes in the pupil-teacher ratio showed up as significantly related to changes in test scores. In a few cases, there was a significant relationship between the Census 2000 characteristics of average schooling and median family income in the district; however, these were more rare.

Table 6 replicates the exact same structure as Table 5 as well as displaying the same general pattern of results. The only difference is that Table 6 employs a Tobit regression for this robustness portion of the analysis. In all but one scenario, there is no statistically significant effect of changes in revenues on changes in either persistence or test scores. The one exception where the t-statistic on the coefficient of "Changes in Revenues per Student" is above the 5% statistical significance threshold conventionally employed is for the case where full controls are employed in the regression—specifically, census information, school information, and initial levels are included—and the outcome measure is changes in dropout rates between 1997 and 2006. In this particular case, the coefficient on changes in revenues is 0.195 with a t-statistic of 2.15, above the threshold to be statistically significantly different from zero at the 5% level. The reason that this coefficient may be positive could be due to the anomalous nature of what occurred in 2005 in particular—as seen in the regressions at the cross-sectional level in Table 4—which will also be evidenced in the differenced regressions. It is thus possible that there is some selection of funding affecting dropouts by targeting the at-risk population of students. It should be noted, however, that in no other instance is there a statistically significant effect at the 5% level of changes in revenues on either persistence or test-score changes. T-statistics on changes in revenue per student in all cases but the one mentioned range from a low close to 0 to a high—approaching, but not gaining conventional levels of significance—above 1.9 in absolute value. In the majority of cases, the t-statistics are below one in absolute value, similar to the results in Table 5, implying very little reason to trust that the associated coefficients are different from zero. In the subset of cases where the t-statistics are approaching statistical significance, the effects show an increase in revenues associated with an increase in student outcomes. Because these results are not statistically significant at conventional levels of significance, however, no clear statements should be made from that set of results.

Table 6. Tobit Regressions Effect of Changes in Revenue per Student on Student Outcomes

Panel A: Effect on Persistence (2006–1997)												
CENSUS INFO	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
SCHOOL INFO	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
INITIAL LEVELS	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
	Change in Dropout Rate				Change in Grad Rate				Change in Frac Diplomas			
Change Rev. per Student	-0.091	-0.107	0.12	0.195	-0.176	-0.215	0.085	-0.001	0.014	0.014	0.011	0.024
	[1.04]	[1.22]	[1.33]	[2.15]*	[0.34]	[0.42]	[0.15]	[0.00]	[1.56]	[1.58]	[1.11]	[1.95]
Observations	281	281	281	281	274	274	274	274	276	276	276	276
Panel B: Effect on Test Scores (2006–2004)												
CENSUS INFO	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
SCHOOL INFO	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
INITIAL LEVELS	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
	Change in Math Scores											
	4th Grade				7th Grade				10th Grade			
Change Rev. Per Stu. (2006–2004)	-1.074	-0.889	-0.544	-1.03	1.088	1.392	0.69	1.902	1.922	2.576	1.282	1.834
	[0.90]	[0.72]	[0.41]	[0.73]	[0.75]	[0.93]	[0.43]	[1.13]	[1.48]	[1.93]	[0.93]	[1.26]
Observations	264	264	264	264	265	265	265	265	271	271	271	271
	Change in Reading Scores											
	5th Grade				8th Grade				11th Grade			
Change Rev. Per Stu. (2006–2004)	-1.061	-1.309	-1.062	-0.65	-0.932	-1.344	-2.06	-1.774	0.371	0.332	-0.725	-1.107
	[0.94]	[1.13]	[0.86]	[0.49]	[0.88]	[1.26]	[1.80]	[1.45]	[0.32]	[0.28]	[0.59]	[0.81]
Observations	265	265	265	265	270	270	270	270	267	267	267	267

NOTE: Test score regressions are run at the grade level for each school district while Persistence regressions are run at the school district level. Robust standard errors are employed in all regressions. CENSUS INFO includes district level averages for the fraction of women and men in the labor force, Median family income, the fraction of children living in poverty and the fraction of individuals in each of five educational groups (1–0, 9–12, High School Degree, Associate Degree, College or more). SCHOOL INFO includes changes between 2004 and 2006 which occurred in the following variables: the pupil teacher ratio, the school enrollment, the number of full time equivalent teachers, and the fraction of students on free lunch. For the test score regressions, INITIAL LEVELS includes information on the 2004 values of the pupil teacher ratio, the school enrollment, the number of full time equivalent teachers, and the fraction of students on free lunch while the persistence regressions INITIAL LEVELS includes the 1997 values for these same variables. Absolute values of t-statistics in brackets. *Significant at 5% level. **Significant at 1% level.

CONCLUSIONS

Changes in the School District Finance and Quality Performance Act over 1997–2006 had little effect on student persistence or test scores. It should be kept in mind, however, that the current results may be an underestimate of the effect of school funding on student achievement due to persistent problems of selection into schooling, which were not possible to correct in the current analysis. It is also possible that some of the amendment effects show up with a lag and are thus not being picked up by the analysis that only covers the years through 2006.

It is also important to note that the availability and allocation of resources is not equivalent to the ability and means to use these resources effectively to help students. Teacher and administrator ability/training/salary, administrative structure, and parental involvement all play a role in how effectively resources are employed in helping students to succeed.[25] Besides these factors, at issue is whether schools that actually need and know how to use funds are the ones that receive them. It is alternatively possible that funds are allocated in a way so as to satisfy political exigencies rather than school district's direct concerns.

The diversity of student populations and the demographic makeup is also important for schools to consider in making their choices in how to create the best environment for students to succeed. In the cross-sectional regressions, there was some slight evidence that characteristics of school districts other than funding were in some instances significantly related to student outcomes.[26] The pupil-teacher ratio, fraction on free lunch, enrollment levels, and number of full-time equivalent teachers were, at times, significant predictors of persistence in the cross section if not in the difference analysis. Some of these, such as the pupil-teacher ratio and the number of full-time equivalent teachers are partially able to be manipulated by school districts. There was also some slight evidence that income and poverty levels of students and their school district areas were related to persistence and test-score achievement. This should be noted since it implies that administrators need to account for the underlying populations and neighborhoods and learn to use the correct strategies for areas with higher rates of poverty and lower-median family income levels.[27]

All of these various issues are key factors to consider in moving forward to decide on the best school policies related to levels of school funding, as well as the distribution and uses of those funds so as best to meet student need and foster student achievement.

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25. The literature on parental involvement consistently shows a positive effect of parental involvement on student achievement, particularly for minority and low-income students. A.J. Houtenville and K. Smith Conway, 2008. Parental Effort, School Resources, and Student Achievement. *Journal of Human Resources*. 43(2). 437-453. Evidence that allocating funding to raising teacher salaries—either to attract better teachers or to encourage current teachers to work harder—serves to increase test scores. L. Chaudhary, 2009. Education Inputs, Student Performance and School Finance Reform in Michigan. *Economics of Education Review*. 28(1): 90-98. It is possible to assess how teachers and administrators understood how to use an influx of funds and, therefore, whether such an influx of funds improves achievement. L.Goe, 2006. Evaluating a State-Sponsored School Improvement Program through an Improved School Finance Lens. *Journal of Education Finance*. 31(4): 395-419.

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