

Does School District Consolidation Cut Costs?

Abstract

Consolidation has dramatically reduced the number of school districts in the United States. Using data from rural school districts in New York, this paper provides the first direct estimation of consolidation's cost impacts. We find economies of size in operating and capital spending: doubling enrollment cuts total costs per pupil by 28 percent for a 300-pupil district and by 9 percent for a 1,500-pupil district. Adjustment costs in capital spending lower these enrollment-based cost savings by about 5 percentage points. Overall, consolidation makes fiscal sense, particularly for very small districts, but states should avoid subsidizing unwarranted capital projects.

The results of this paper should be of interest to state and local elected officials, to people in state education departments, and to public school administrators.

Introduction

School district consolidation represents one of the most dramatic changes in education governance and management in the United States in the twentieth century. Over 100,000 school districts have been eliminated through consolidation since 1938, a drop of almost 90 percent (National Center for Education Statistics, 2003, Table 87). This trend continues throughout the country, largely because consolidation is widely regarded as a way for school districts to cut costs. This paper provides a new look at the potential cost consequences of consolidation. Using a unique panel data set for rural school districts in New York State, we ask whether consolidation leads to significant cost savings, controlling for student performance. This paper therefore complements recent research on the causes of consolidation (Brasington, 1999, 2003).

Although the pace of school district consolidation has slowed since the 1970s, some states still provide incentives to consolidate. New York and at least seven other states have aid programs designed to encourage district “reorganization,” typically in the form of consolidation (Gold et al., 1995). Some other states encourage consolidation through their building or transportation aid formulas (Haller and Monk, 1988). In contrast, about one-third of states use operating aid formulas that compensate school districts for sparsity or small scale (Huang, 2004) and thereby discourage consolidation. Although scholars do not agree on the cost impacts of consolidation, it is likely to remain on the education policy agenda in many states, particularly when school districts are under pressure to cut costs and raise student performance. As Haller and Monk (1988, p. 479) put it, “the modern reform movement is likely to prompt additional school district reorganization efforts, despite its virtual silence on the question of size.”

This paper begins with a discussion of the concept of economies of size and its link to school district consolidation. The second section provides a review of the cost function literature and evaluates existing evidence on economies of size in education. In the third section, we present the first formal evaluation of school district consolidation, including the first look at economies of size in capital spending. This evaluation is based on a panel of rural school districts in New York, some of which consolidated during the sample period, 1985 to 1997.

Economies of Size and the Effects of Consolidation

By altering the size of participating districts, consolidation could raise or lower per-pupil costs. In this section we define economies of size and discuss the possible linkages between education costs and consolidation.

Defining Economies of Size

Economies of scale are said to arise when the cost per unit declines as the number of units goes up. In some contexts, the notion of a unit is straightforward; one could ask, for example, how the cost per widget changes with the number of widgets produced. In education, however, several different “units” could be defined, including the number of students, the quality of the services (as measured, say, by student performance), or the scope of educational services.¹

As in most empirical research on economies of scale in education, the focus here is on economies of size, which refer to the relationship between per-pupil expenditure and total enrollment, all else equal. This relationship can be estimated from an education cost function, which controls for output (that is, student performance), input prices, and other variables. Economies (diseconomies) of size exist if the estimated elasticity of education costs per pupil with respect to enrollment is less than (greater than) zero.

Potential Sources of Economics of Size

Tholkes (1991) and Pratten (1991) identify five potential sources of long-run economies of size that seem especially pertinent to education.

Indivisibilities. Economies of size may exist because the services provided to each student by certain education professionals do not diminish in quality as the number of students increases, at least over some range. For example, the central administration of a district, as represented by the superintendent and school board, has to exist whether the district has 100 or 5,000 students. Although additional administrators need to be added at some enrollment level, the same central administration may be able to serve a significant range of enrollment. Moreover, teachers may provide a public good over some range of enrollment because they may be able to teach up to, say, 15 students without a significant drop in the quality of education they provide.²

Increased Dimension. The traditional long-run concept of economies of scale focuses on efficiencies associated with larger units of capital. Larger plants may be able to produce output at a lower average cost, because they can employ more efficient equipment, for example. In education, the logical plant is the school, and equipment includes the heating plant, communications system, and specialized facilities, such as science or computer labs.

Specialization. Economies of size might arise if larger schools are able to employ more specialized labor, such as science or math teachers. This possibility may provide a particularly compelling justification for consolidation in an era of rising standards, with its call for more demanding and specialized classes at the high school level (Haller and Monk 1988).

Price Benefits of Scale. Large districts may be able to take advantage of the price benefits of scale by negotiating bulk purchases of supplies and equipment or by using their monopsony power to impose lower wages on their employees (Wasylenko 1977).

Learning and Innovation. If the cost of implementing innovations in curriculum or management declines with experience, a larger district may be able to implement such innovations at lower cost. In addition, teachers may be more productive in a large school because they can draw on the experience of many colleagues.

Potential Sources of Diseconomies of Size

The existence of economies of size in education has been challenged by recent studies on the effects of large schools on student performance (Fowler and Walberg 1991; Friedkin and Necochea 1988; Haller 1992; Lee and Smith 1997). This research focuses on schools rather than districts, and on production rather than cost functions. The distinction between school and district size is important in urban districts, but in rural areas the sizes of the district and the high school are highly correlated. These studies claim that the potential cost savings from consolidation are seldom realized, and that larger schools have a learning environment that hurts student performance. The research on effective schools provides additional evidence that moderate-sized schools are more successful than large schools at retaining students through high school (Figilio and Stone 1999; Purkey and Smith 1983; Witte 1996).

Five potential sources of diseconomies of scale have been cited in this literature (Guthrie 1979; Howley 1996; Lee and Smith 1997).

Higher Transportation Costs. One potential source of higher costs for larger districts is in transportation. To the extent that consolidating districts make use of larger schools, average transportation distance must increase, as must travel time for students (Kenny 1982).

Labor Relations Effects. According to Tholkes (1991), “the labor relations scale effect, caused by seniority hiring within certification areas and by change in comparison groups for collective negotiations, could be a major source of diseconomies of scale” (p. 510). The potential monopsony power of large districts may be counteracted by the increased likelihood of an active teacher’s union because larger districts are easier to organize. Stronger unions may also prevent staff layoffs and thereby eliminate a major source of cost savings from consolidation.

Lower Staff Motivation and Effort. Administrators and teachers may have a more positive attitude toward work in smaller schools, which tend to involve less formalization of rules and procedures, that is, more flexibility (Cotton 1996). Smaller organizations are also “flatter” organizations with fewer layers of middle management between the teacher or principal and the superintendent, encouraging more input from all school personnel.

Lower Student Motivation and Effort. Students in smaller schools may be more apt to participate in extracurricular school activities (Cotton 1996). Moreover, the employees in smaller schools are more likely to know students by name and to identify and assist students at risk of dropping out. Thus, students in smaller schools may have a greater sense of belonging to the school community, a more positive attitude toward school, and a higher motivation to learn (Cotton 1996; Barker and Gump 1964).

Lower Parental Involvement. Parental contributions to educational production may be facilitated by parental participation in school activities and contacts with teachers and administrators. The role of parents is linked to economies of size whenever parents find participation less rewarding or personal contacts more difficult in larger districts.

Research on Economies of Size and Consolidation

The vast majority of evidence on economies of size, and, by inference, on consolidation, has come from the estimation of education cost functions using data on operating or total spending. The recent literature on this topic has been largely directed toward the methodological concerns raised in a comprehensive literature review by Fox (1981).

Average test scores particularly in math and reading, are the most common measures of student performance, although a few studies use graduation rates. Many studies include factor prices, particularly teacher salaries,³ and five studies, all since 1990, have made teacher quality adjustments.⁴ Several recent studies model costs as part of a behavioral system involving the demand for education, and either estimate a reduced-form expenditure function (Ratcliffe, Riddle, and Yinger 1990; Downes and Pogue 1994) or treat student performance as endogenous (Downes and Pogue 1994; Duncombe, Ruggiero, and Yinger 1996; Duncombe and Yinger 1997, 1998, 2000; Reschovsky and Imazeki 1997, 2001; Imazeki and Reschovsky 2004). Several recent studies also attempt to control for unobserved factors, such as efficiency.⁵ Despite the variety of measures used and geographic areas examined in these studies, a surprising consensus emerges: almost all studies find economies of size over some range of enrollment.

While cross-sectional spending regressions can provide evidence of potential cost savings from consolidation, a more direct and compelling approach is to apply longitudinal methods to a sample of school districts in which some consolidation occurred. No formal evaluations of this type have been conducted, however. Existing case studies (Weast 1997; Hall 1993; Benton 1992; Piercey 1996) focus only on one school district, have no control group or do not use statistical controls, and have limited pre- and post-consolidation data. A case study by Streifel, Foldes,

and Holman (1991) compares pre- and post-consolidation finance data in a national sample of 19 school districts but does not include controls for student achievement, teacher salaries, or changing student composition. Thus, as emphasized by Howley (1996, p. 25): “The lack of pre- and post-consolidation studies means that we have no solid information about the accrual of benefits alleged to depend on school closures and consolidation.”

Evaluation of School District Consolidation in New York

New York State provides an excellent setting for an evaluation of consolidation. First, New York actively promotes the consolidation of small districts by providing “reorganization aid.” Specifically, New York contributes an additional 40 percent in formula operating aid (“Incentive Operating Aid”) to consolidated districts for five years. This aid is then phased out slowly over another nine years. “Incentive Building Aid” provides an additional 30 percent in building aid for capital projects that are committed within ten years of reorganization (New York State Education Department 1999). Reorganization aid totaled close to \$40 million in 1999.⁶ Second, consolidation continues to take place in the state. We examine 12 pairs of districts that consolidated from 1987 to 1995.⁷ See Table 1.

Evaluation Design

To estimate the impact of consolidation on education costs, we begin with the standard formulation of an education cost function (Downes and Pogue 1994; Duncombe and Yinger 1997, 2000; Reschovsky and Imazeki 1997, 2001; Imazeki and Reschovsky 2004):

$$E = f(S, P, N, M, Z) , \tag{1}$$

where E is school spending per pupil; S is school performance; P is input prices; N is enrollment;⁸ M is environmental cost factors, which are fixed inputs outside the control of school officials; and Z is a set of factors that influence school district inefficiency. This cost function can be applied to total spending or to functional subcategories of spending, such as administration, instruction, or transportation. This approach can account for more than measure of school performance, and it can shed light on the costs associated with all of a district's activities, including counseling, health, transportation, and administration.

In our data set, which is described below, S is measured using student test scores and the drop-out rate, P is measured with teacher salaries, and M is measured by the characteristics of the students in a school district, such as the share who live in poverty. Because we observe spending, not cost, our analysis also must consider school district inefficiency, defined as spending any more than necessary, given input and environmental costs, to provide a given level of performance. Thus, inefficiency is tied to the performance variables included in the regression. A school district is inefficient if it provides activities that do not boost performance as measured by the variables in S , even if those activities are worthwhile in some other sense, or if it pays overly generous wages, hires too many administrators, or uses outmoded teaching methods. We cannot observe inefficiency directly; instead, we control for inefficiency by including a set of school district characteristics, Z , that influence the extent to which the behavior of teachers and school administrators is monitored by parents and voters.⁹

To estimate this model, we assemble pre- and post-consolidation data for all consolidating districts and for a comparison group in the years 1985 to 1997. Because all the consolidating districts are rural, the comparison group consists of all rural districts that did not consolidate during this time period.¹⁰ All of these districts, consolidating and comparison, are

“upstate,” that is, not in the New York City region. Following most of the studies in the literature, we specify equation (1) in log-linear form.¹¹

We face four major methodological challenges in estimating this model. First, we need to specify the cost impact of consolidation. A key component of this impact is picked up, of course, by the population variable, N .¹² Consolidated districts are, by definition, larger than the separate districts that consolidate, and the cost impact of the resulting increase in enrollment can be determined from the coefficients of the enrollment variables. We use a quadratic specification for $\ln(N)$; that is, we include $\ln(N)$ and $[\ln(N)]^2$. This specification makes it possible to determine if cost per pupil reaches a maximum or minimum at some enrollment level. In addition, consolidation might result in adjustment costs that fade out over time or some other type of shift in the education cost function. To account for this possibility, we include a post-consolidation dummy variable and time trend for each consolidating district pair.¹³

Second, unobserved factors influencing the consolidation decision might also influence spending per pupil, so our estimated coefficients could be biased if these unobserved factors are not taken into account. This is a form of endogeneity bias in which both the dependent variable and variables associated with consolidation, such as N , are influenced by variables that cannot be observed. To address this problem, we estimate our model with district-specific fixed effects and time trends. This well-known approach accounts for all unobservable factors that vary linearly over time and therefore eliminates the possibility of bias from these factors, even if they are correlated with consolidation.¹⁴

To preserve all pre-consolidation information, we retain each district as a separate observation even after it consolidates. Once a district has consolidated, however, we assign it the characteristics of the combined district as a whole. This approach requires an adjustment in the

district fixed-effects variables to account for consolidation. Because the post-consolidation dependent variable combines spending per pupil for the two districts, the fixed effect for each original district (a) is diluted and (b) has an impact on the dependent variable for post-consolidation observations of its partner district. After consolidation, therefore, each district's fixed effect is weighted by that district's share of total enrollment in the combined district just before the consolidation, and is switched on for each consolidating district and its partner.¹⁵ These two steps are also applied to the district-specific time trend.

District fixed effects and time trends provide extensive protection against endogeneity bias, but cannot protect against factors that influence both spending and the decision to consolidate and that vary in a nonlinear way over time. In the case of New York State, such factors are unlikely to play a major role. According to the rules in New York, consolidation is a process, not an event (The University of the State of New York, undated). Districts considering consolidation must first ask the New York State Department of Education for an analysis of this step, along with a recommendation. Once this analysis and recommendation have been prepared, the districts must negotiate with the state about the aid involved and decide whether to proceed. If they do, they must present the consolidation option to the voters and cannot consolidate without the approval of a majority of the voters in each participating district. As a result, consolidation generally cannot be completed for two or three years from the time the State is brought into the discussion. In this setting, short-term fluctuations in the relative social or economic position of a district are unlikely to have an impact on the consolidation decision, and controlling for district fixed effects and time trends provides adequate insurance against selection bias. One possible exception involves a change in district leadership. A new superintendent might push for consolidation even when his or her predecessor did not think it was a good idea.

We account for this possibility by including a variable identifying districts that changed superintendents within the last two years.

Despite the institutional constraints on consolidation and the steps we have taken to address endogeneity, we cannot formally rule out the possibility that our estimates are biased by unobservable factors that influence both consolidation and non-linear variation in spending. Moreover, we know of no instrumental-variables (IV) procedure for panel data like ours in which some observations combine and then remain in the sample.¹⁶ Fortunately, however, we can implement two additional procedures that shed light on the possibility of endogeneity bias in our estimates.

The first procedure is to estimate our model without the consolidating districts. With this procedure, the coefficients of the enrollment variables obviously are not influenced at all by changes in enrollment associated with consolidation. If the endogeneity of consolidation is a source of bias, then this alternative procedure should yield substantially different estimates of economies of size.

The second procedure is to use a propensity-score method to determine whether non-consolidating rural districts are an appropriate comparison group for studying consolidation as a form of “treatment.” As shown in the program evaluation literature (Heckman, LaLonde, and Smith, 1999; Cobb-Clark and Crossley, 2003), a comparison of treated observations with observations that are unlikely ever to be treated can lead to inaccurate estimates of the treatment effect. One method for addressing this problem is to estimate a model of the propensity to receive treatment and then to include a propensity score based on this model in the regression that determines the impact of treatment on the outcome of interest. We use an alternative procedure suggested by Cobb-Clark and Crossley, namely, to trim the comparison-group sample

so that both the treatment and comparison groups have a similar propensity to consolidate.¹⁷ The details of our procedure are discussed below.

The third methodological challenge is that S , P , and state aid, one of the variables in Z , are influenced by the actions of school officials and are therefore endogenous. The endogeneity of S and P is explored in previous cost studies. The total aid variable is endogenous in this study because at least one important aid program, building aid, uses a matching formula.¹⁸ In addition, building aid in New York is project-based, which means that a district must submit a capital project to the state for approval and funding. Aid will be endogenous in the capital spending regression if post-consolidation capital spending plans result in post-consolidation increases in state building aid.

We address this challenge using two-stage least squares. Following well-known rules, we select instruments for this procedure that (1) make conceptual sense, (2) help to explain the endogenous explanatory variables, and (3) do not have a significant direct impact on the dependent variable. To meet the first two rules, we considered determinants of the demand for education as instruments for S , determinants of local labor market conditions as instruments for P , and factors influencing state aid as instruments for the aid variable.¹⁹ The instruments we selected are described below. In addition, we conducted an over-identification test (Wooldridge 2003) to determine whether our final set of instruments was exogenous, and we implemented the Bound, Jaeger, and Baker (1995) procedure to check for weak instruments.²⁰

The final methodological challenge is that capital spending is lumpy, so capital spending in a given year is not a good indication of a district's long-term expected annual capital spending. Indeed, capital spending often exhibits a large "spike" somewhere between two and eight years after the consolidation took place. Consolidation is not the whole story, however,

because capital spending takes the form of a spike in non-consolidating districts, too. To focus on expected long-term spending, we define the dependent variable in our capital cost regression as a district's nine-year average capital spending. We can calculate this variable for every year in our panel, because our data on spending, unlike our data for most explanatory variables, goes back until 1977. The dependent variable for a 1985 observation (the first year in our panel) therefore is based on the nine-year average from 1977 through 1985. To reflect the fact that capital deteriorates over time, we used a 2 percent annual depreciation rate to adjust capital spending.²¹

The use of nine-year average capital spending necessitates three other changes in our cost model for capital spending. First, we also average the enrollment variable over the same nine-year period. This step makes it possible to interpret the coefficient of the enrollment variables as describing the impact of a long-run shift in enrollment on a district's expected long-run annual capital spending. Second, state aid for capital spending in New York State is largely project-based aid, so the time series for state aid is almost as lumpy as the series for capital spending. To smooth out the state-aid data, that is, to translate it into a long-run measure, we also use a district's nine-year average state aid as an (endogenous) explanatory variable.²² Because we average both capital spending and state aid over a nine-year period and treat aid as endogenous, we are able to look at the impact of a district's state aid pattern, including any increases in aid that occur after consolidation, on a district's capital costs. Thus, the coefficient of the aid variable indicates the impact of higher average aid on the average capital spending needed to deliver a given current student performance. Third, the use of a nine-year average for capital spending requires a change in the district-specific fixed effects and time trends.²³

Data and Measures

Our panel data set covers a subset of school districts in New York State for the years 1985 to 1997. To ensure at least two years of data before or after each consolidation, we focus on the 12 consolidations that occurred from 1987 to 1995. Approximately 190 other rural districts serve as a comparison group.²⁴ Our main data sources are the New York State Education Department and the New York State Comptroller.²⁵

Our approach requires data on student performance. Previous research on New York has identified three performance measures that are correlated with voter preferences: (1) the percent of students unable to reach minimum competency on elementary school math and reading tests (PEP tests), (2) the dropout rate among high school students, and (3) the percent of students receiving a Regents diploma, which requires passing a set of demanding exams in high school (Duncombe, Ruggiero, and Yinger 1996; Duncombe and Yinger 1997, 1998, 2000). Accounting for the third variable is particularly important New York, where one argument for consolidation is that it facilitates the offering of special classes to support the Regents exams.

As noted earlier, instruments for S are drawn from an analysis of the demand for education. The best known demand variables are district income, state aid, and tax price. However, income, tax base (the denominator of tax price), and state aid are also determinants of school district efficiency (Duncombe and Yinger, 1997, 1998, 2000). Higher values for these variables lower the incentives of parents to monitor a school district's performance on specific outcome variables, as identified by S . As a result, these variables violate the third rule and are not legitimate instruments. To identify alternative instruments, we hypothesize that voters' desired level of student performance depends in part on what adjacent districts are able to accomplish. This hypothesis leads to a potential list of instruments that consists of minimum,

maximum, and mean values of our three performance variables in adjacent districts. The role of these variables may be tempered by other comparisons across districts, in salaries, incomes, property values, and so on. As a result, our potential set of instruments also includes the average, maximum, and minimum value of income, property value, and percentage of students receiving a subsidized lunch in adjacent school districts. This initial list of instruments (along with the two discussed below) was pared down using the rules outlined earlier. These steps were taken separately for the operating and capital cost regressions, so that the instruments in these two regressions are not the same. The instruments are also allowed to vary for all the sub-categories of operating spending.

To capture efficiency in the cost model, we include income per pupil, tax base per pupil, and state aid per pupil divided by total income, which we call the state aid ratio.²⁶ Our variable indicating a change in superintendent might also capture efficiency effects. We hypothesize that state legislators are wary of large differences in aid per pupil between similar districts, because of the implications for fairness and for political standing. As a result, our list of potential instruments for the state aid variable consists of minimum, maximum, and mean aid in adjacent districts; average aid in districts in the same county; average aid in districts in the same enrollment category; the interaction between the previous two variables; and the “temporing” variables identified above.

For our price variable, we use information on the average salary for teachers with one through five years of experience, which is a better indicator of the cost of attracting teachers than a measure of salaries for more experienced teachers. To control for teacher quality differences, we regressed actual salaries on teacher education and experience, and then constructed a predicted wage for teachers with average experience and education. Research on public labor

markets has found significant spillovers across adjacent governments, particularly when active unions are present, as in New York (Freeman 1986). As a result, our list of instruments for the wage variable includes the minimum, maximum, and mean teacher wage in adjacent counties.

Environmental variables identified in past research include child poverty, the incidence of single-parent families, the proportion of students with limited English proficiency or special needs, and the share of secondary students in a district. Because Census data are not available for each year, our environmental cost variables are limited to the percentage of students receiving a subsidized lunch, a well-known proxy for poverty, and the percentage of students in secondary grades. However, district fixed-effects and trend variables control for unobserved student and family characteristics, at least to the extent that they follow a linear time trend.

Empirical Results

Descriptive Analysis. The first two columns of Tables 2 and 3 compare the characteristics of consolidating districts in 1985, before they consolidated, with the characteristics of non-consolidating rural districts in the same year.²⁷ On the financial side (Table 2), consolidating districts spent less in every category except for central administration, for which the difference is not statistically significant. They also have less local revenue and more state aid than non-consolidating districts and pay somewhat lower salaries. In contrast, consolidating districts spend more than other districts in 1997. The shift in capital spending is particularly striking; consolidating districts were spending three times as much per pupil in 1997, despite considerably lower spending per pupil in 1985. Hence, the cost advantages of consolidation, if any, are not visible in the aggregate figures.

Turning to Table 3, we find that in 1985 consolidating districts had fewer pupils per administrator, lower property wealth, smaller total enrollment, smaller schools, fewer schools,

and a lower percentage of students going to college. For example, fifty percent of consolidating districts had only one school before consolidation, but no one-school districts remained after consolidation. One key question is whether consolidation has positive effects on student performance. Table 3 suggests that the effects are modest, at best. Differences between consolidating and non-consolidating districts were not significant in 1985, except for a lower college-going rate in consolidating districts. The pattern in 1997 is similar, with only one significant difference, namely a smaller failure rate on the math PEP tests for consolidating districts. These differences are consistent with the view that consolidation boosts performance, but the differences are small in magnitude. Table 3 also does not support the view that consolidation increases the number of more demanding Regents courses and hence the number of students receiving Regents diplomas.

Table 4 shows that, almost across the board, inflation-adjusted expenditure per pupil, revenue per pupil, and average teacher salaries are higher after consolidation than before. The only exception is expenditure for central administration. However, a similar pattern emerges in non-consolidating districts.²⁸ Table 4 also shows that capital expenditure and operating and maintenance expenditure rise more rapidly after consolidation than before, but spending for teaching and central administration grow more slowly or even decline.

Cost Regression Results. The cost models were estimated using 2SLS, with student outcomes, teacher salaries, and the state aid ratio treated as endogenous. Separate regressions were estimated for operating expenditure, capital expenditure, and selected functional subcategories of expenditure that do not involve substantial capital spending. Table 5 presents detailed results for operating and capital spending per pupil. Regressions for functional spending subcategories include the same explanatory variables.²⁹

Because the regressions include district-specific fixed effects and time trends, the estimated coefficients are identified only by nonlinear variation in the explanatory variables. Thanks to the large non-linear changes in enrollment caused by consolidation, this methodology is ideal for estimating the enrollment coefficients, but it also implies that the coefficients of other variables do not provide general tests of the impact of these variables on education costs. In other words, the district-specific fixed effects and time trends undermine our ability to estimate precisely the impact of the non-enrollment variables in order to minimize the possibility of bias in the coefficients of the enrollment variables.

In the operating cost regression, the first column of Table 5, the estimated coefficients of the outcome variables are all statistically significant. With the district fixed effects and trends in the model, however, these coefficients all have signs opposite to those in a standard specification. One of the cost variables, percent receiving a subsidized lunch, also has the opposite sign from a standard specification and is statistically significant. Because these variables all have the expected sign in other analyses using New York data in this period (Duncombe and Yinger, 1997, 1998, 2000), we interpret these results as an indication that it is difficult to identify the effects of these variables in a model that contains fixed effects and time trends.³⁰ The core efficiency variables, state aid, property values, and median income, have the standard positive signs and are statistically significant (state aid at only the 10 percent level).³¹ A change in superintendent also boosts costs, but is not significant. In addition, the efficiency variables measuring aid received by similar districts have non-standard positive coefficients and are significant.

In the case of capital costs, the high-school student-performance variable has the standard positive coefficient and is statistically significant. See the second column of Table 5. The

coefficient of one efficiency variable, property value, has the standard positive coefficient and is significant, and a recent superintendent change leads to significantly lower costs, perhaps because new superintendents take some time to work out their capital spending plans.

Estimated Economies of Size. The coefficients of the enrollment variables in Table 5 are highly significant statistically and indicate large economies of size in both operating and capital spending. Table 6 presents the coefficients of the enrollment variables for various categories of spending. The first enrollment variable is negative and significant in every regression. We dropped the second enrollment variable, the square of log enrollment, if it had a t-statistic below 1.0. This rule led us to drop the second enrollment variable for capital spending, administrative services, and transportation. For these categories, economies of size exist throughout the size distribution. In the other cases, the estimated relationship between per-pupil spending and enrollment is U-shaped and both enrollment variables are statistically significant.

The estimated economies of size are illustrated in the last three columns of Table 6 and in Figure 1. These columns in Table 6 indicate the economies of size associated with three hypothetical consolidations, corresponding roughly to the types of consolidations in our data (see Table 1). The panels of Figure 1 plot cost per pupil as a function of district enrollment, compared to a district with 300 pupils that does not consolidate. For now, we want to focus on the thickest lines in these panels, which are labeled “baseline.”

Panel A of Figure 1 and Table 6 reveal that operating cost per pupil has a U-shaped relationship with enrollment, with a minimum at 3,532 pupils. Because this minimum is near the maximum enrollment observed in our data, economies of size in operating spending arise with most patterns of consolidation but are larger when the consolidating districts are relatively small.

As shown in Table 6, operating cost per pupil declines by 28.8 percent when two 300-pupil districts merge, but the cost savings drop to 7.0 percent when two 1,500-pupil districts merge.

Results for the functional spending categories support elements of the traditional view of economies of size. As shown in Table 6 and in panel C of Figure 1, spending for instructional purposes and for teaching alone exhibit the expected U-shape, with minimum per-pupil costs in a district with 2,463 pupils and 3,529 pupils, respectively. These results imply that the hypothetical consolidations in Table 6 result in substantial savings in both instructional and teaching costs per pupil, particularly when two small districts are combined. The cost savings in the fourth column of Table 6 are 24.8 percent for instruction and 27.9 percent for teaching. These results support the view that, up to a point, there is publicness in the provision of classroom instruction.

The results concerning spending for central administration also confirm the traditional view. As shown in panel D of Figure 1, the per-pupil cost for these services declines steadily as enrollment increases. In fact, as indicated in the last three columns of Table 6, doubling district enrollment cuts administrative costs per pupil by more than two fifths—a sign of extensive publicness in administrative services.

The results for transportation services contradict the traditional view, because they reveal that per-pupil transportation costs decline steadily as enrollment increases, even at high enrollment levels. See panel E of Figure 1. As shown in Table 6, doubling district enrollment cuts transportation costs per pupil by over one quarter. Thus, we find clear evidence of economies of size—not diseconomies of size—in the provision of transportation services. Consolidation-induced increases in travel distance, if any, are clearly offset by cost savings in bus route planning, maintenance, or some other aspect of transportation services.

Finally, the results for capital spending also indicate substantial economies of size. As shown in Table 6 and Figure 1, doubling enrollment cuts capital spending by almost one quarter.

Alternative Procedures. As explained earlier, the steps we have taken may not completely eliminate endogeneity bias in the estimated coefficients of variables associated with consolidation, including enrollment. Table 7 presents the results of two alternative procedures designed to shed light on this possibility.

The first procedure is to estimate economies of size using only the districts that do not consolidate. The results of this procedure are presented in the second column of Table 7. We find that the magnitude and statistical significance of estimates obtained through this procedure are remarkably similar to the estimates from our original procedure, which are repeated in the first column. In the case of operating spending, for example, the two estimated coefficients change by less than 5 percent. These results support the conclusion that endogeneity bias is not a serious problem in our original method.

The second procedure is to trim the comparison-group sample using propensity scores. We begin by using our entire sample to estimate a proportional hazard model of the probability that a district will consolidate.³² This model expresses the probability of consolidation in year t , given that it has not happened before t , as a function of variables drawn from recent studies of the consolidation decision (Brasington, 1999, 2003). These variables are the number of years since the previous consolidation in a district's county, educational performance in surrounding districts (our measures of drop-outs and regents diplomas), and characteristics of the district (share of students eligible for a free lunch and our state aid variable). Each of these variables is statistically significant at the 5 percent level. A district's propensity score in each year is then the predicted value of the dependent variable from this regression.

The next step in this procedure is to determine the lowest propensity score for a consolidating district in the year before consolidation—and to eliminate non-consolidating districts with lower propensity scores. We found that none of the non-consolidating districts had a propensity score below this cut-off in every year, which suggests that the treatment and comparison districts are not fundamentally different. Nevertheless, we replicated our analysis with a trimmed sample that drops all non-consolidating districts with propensity scores below this cut-off in three or more years. The results for the enrollment variable are in the third column of Table 7. In every case, the results are very similar in magnitude and statistical significance to those with the full sample.³³ Based on these results, we conclude that our results are not driven by a lack of comparability between our treatment and comparison districts.

Estimated Non-Enrollment Cost Impacts of Consolidation. The district-specific fixed effects and time trends for each consolidating pair measure the cost impacts of consolidation that are not associated with enrollment. The mean values of these coefficients (along with associated t-statistics) are presented in Table 8. Both operating spending and the functional spending subcategories exhibit the same significant pattern: a positive upward shift in per-pupil costs at the time of consolidation followed by a gradual decline in per-pupil costs in the years after consolidation has taken place.³⁴ Moreover, as shown in Table 8 and in panels A and C through E of Figure 1, the gradual decline more than offsets the initial upward shift somewhere between the third and eighth year after consolidation, depending on the category. In other words, consolidation results in short-run adjustment costs in operating spending, but these adjustment costs phase out quickly over time.³⁵ As shown in Table 8, these adjustment costs are equivalent to a 1.3 percent increase in annual operating spending over a 30-year period.

The time pattern of the results for capital spending is different because both the initial shift and the time trend after consolidation are positive; that is, consolidation results in an upward shift in the cost of capital projects and this upward shift grows over time. See Table 8. The estimated net impact is large, but coefficients are not statistically significant. With a 30-year time horizon and a 5 percent discount rate, these point estimates imply adjustment costs equivalent to a 37.4 percent annual increase in capital costs over a 30-year period.

Because consolidation results in a large increase in state building aid, it is also appropriate to ask whether higher state aid results in higher capital costs. As shown in Table 5, we find that the impact of aid on capital costs is large, although it is statistically significant at only the 10 percent level (with a two-tailed test). We use a simple regression to estimate the average change in state aid for each year after consolidation and then multiply this aid change by the coefficient of the aid variable.³⁶ By adding up these impacts over time (with discounting), we find that the impact of increased state aid on capital costs is equivalent to a 13.0 percent increase in annual capital costs with a 30-year horizon (see Table 8). This effect could arise because increased state aid reduces the incentives of voters to monitor public officials and therefore boosts district inefficiency.³⁷

The capital-cost regression in Table 5 holds current student performance constant but it does not control for future student performance. Consequently, the short-run non-enrollment cost increases we estimate could result in long-run performance increases. One possibility is that consolidation and the aid that accompanies it encourage districts to speed up the capital projects that they would have taken anyway. Indeed, districts have a strong incentive to speed up their capital projects: in order to receive consolidation-based building aid, districts must have capital projects approved within ten years after consolidation. To the extent that the non-enrollment

capital cost increases we estimate reflect this type of speeding up, they will be partially offset by capital cost decreases in the future. From society's point of view, the present value of future cost savings will not fully offset short-run cost increases, but they will offset them to some degree.

A related possibility is that districts take advantage of the opportunities for capital planning and state aid increases that accompany consolidation to undertake capital projects that boost student performance in the long run. Our regressions are not designed to study this type of effect. These regressions estimate the relationship between capital spending over a nine-year period and student performance at the end of that period, but, as explained earlier, they include district-specific fixed effects and time trends, so the performance coefficients are based only on nonlinear variation in the performance variables. Despite this limitation, we find that an increase in the percentage of graduates receiving Regents diplomas is significantly related to capital spending over the preceding nine-years. Because a production function is just the inverse of the cost function we estimate, this result implies that short-run increases in capital spending will result in student performance increases over the subsequent nine years. Indeed, the estimated coefficient in Table 5 implies that this effect is large: a 1.0 percent increase in average capital costs over the preceding nine years results in a $0.01/0.00754 = 1.33$ percentage point increase in the percentage of graduates receiving Regents diplomas.

These long-run changes in student performance require an adjustment in long-run capital cost curves analogous to those in Figure 1. To hold long-run student performance constant, these cost curves must be shifted downward. Table 8 presents one rough version of this type of correction. We use the coefficient of the Regents-diploma variable to estimate the performance impacts that occur in the future because of the short-run capital cost increases associated with consolidation and the associated state aid increases. The implied performance increases are

illogically high in some cases, so we impose the constraint that the share of Regents diplomas cannot exceed 95 percent. We then shift the capital cost curve downward in the future so that it holds long-run student performance constant, and calculate the present value of the downward shifts over 30 years. As shown in Table 8, this type of performance feedback brings the capital cost curves down by 14.5 percent annually. Overall, our rough calculations suggest that consolidation boosts 30-year capital costs not related to enrollment by $(37.4 + 13.0 - 14.5) = 35.9$ percent per year.

Table 9 combines the enrollment effects in Table 6 and the non-enrollment effects in Table 8. Capital costs are weighted at their share of spending in non-consolidating districts in 1997, namely 9.2 percent. Economies of size result in annual cost savings of 28.4 percent, 15 percent, and 8.7 percent for the three hypothetical consolidations in Table 9. The non-enrollment cost effects, which include adjustment costs, aid-induced cost effects, and (for capital spending only) long-run performance feedback, imply a net annual cost increase of 4.7 percent. This cost increase is not large enough to eliminate the cost savings due to economies of size. Specifically, the overall net annual cost savings from our hypothetical consolidations range from 3.9 percent for two 1,500-pupil districts to 23.7 percent for two 300-pupil districts.

Conclusions and Policy Implications

Because of significant economies of size, consolidation clearly cuts operating costs for small, rural school districts in New York. Although consolidation results in adjustment costs in operating spending, these adjustment costs are small and phase out quickly. The operating cost savings are largest when consolidation combines two very small districts, but even two 1,500-pupil districts can cut their operating costs about 6 percent by consolidating.

Our results for capital spending are more difficult to interpret. We also find large, significant economies of size in capital spending. Doubling district size cuts capital spending by 24 percent. We also find, however, that consolidation and the increases in state aid accompanying it may result in large short-run increases in capital costs. Although our estimates of these effects are not statistically significant at conventional levels, they suggest that the cost savings associated with economies of size may be offset, at least in the short run, by consolidation-linked decisions to provide more capital than is needed to improve current student performance. This added capital may enhance student performance in the long run, but our rough estimates suggest that this type of performance effect only partially offsets the short-run cost increases associated with consolidation. Despite large economies of size in capital spending, therefore, districts that consolidate should not expect their capital costs to decline. Given the approximate nature of these results, further work on capital costs would be fruitful.

These results do not provide a complete benefit-cost analysis of consolidation because they do not consider changes in consumer surplus associated with consolidation-induced changes in school performance, changes in transportation costs for students and their parents, or changes in dimensions of school performance other than test scores and graduation rates. Because consolidation in New York requires the consent of voters in each consolidating district, we presume that the benefits to voters are perceived to outweigh the costs in every district that consolidates with another.³⁸ In other words, costs we do not measure, if they exist, must not be large enough to overcome cost savings, at least in the perceptions of voters.

Nevertheless, a policy discussion is incomplete without some information on the extent to which other factors offset the cost savings from consolidation. A complete analysis of these factors is beyond the scope of this paper, but we can shed some light on the potential losses of

consumer surplus. Changes in consumer surplus can arise because the school performance choices of a consolidated district may diverge from the desired performance levels of the voters in the districts that consolidate. As explained by Bradford and Oates (1974), combining jurisdictions with different preferences into a single jurisdiction generally results in a loss of consumer surplus. People in the jurisdiction with relatively low demand for public services are forced to consume units of public service for which their marginal benefit is less than their marginal cost and people in the jurisdiction with relatively high demand for public services are forced to forgo units for which their marginal benefit exceeds their marginal cost.

In the current context, this theorem must be modified to account for changes in tax share, which equals a voter's house value divided by property value per pupil in the district. The consolidation of two districts lowers the tax share (and hence tax price) for voters in the district with the lower tax base and raises the tax share for voters in the district with the higher tax base. If the demand for education is positively correlated with tax base, these changes in tax share offset, to some degree, the consumer surplus losses associated with heterogeneous demand for school performance. Consider the case of a performance level in the consolidated district above the desired level for the median voter in one of the consolidating districts. The offset occurs because an increase in tax base (and hence a decline in tax share) raises this median voter's desired level of school performance and thereby moves this level toward the level in the consolidated district. If the correlation between demand and tax base is negative, however, the tax-share effect supplements the demand effect, that is, it leads to an even greater loss of consumer surplus.

To explore this issue, we applied the demand function estimated in Duncombe and Yinger (2001) to 12 districts that consolidated after 1990.³⁹ For each of these districts, we first

define the change in a demand variable as the difference between the value of that variable in 1990 in a given district and the average value of that variable in 1990 for that district and the district with which it eventually consolidated. Second, we multiplied the change in each demand variable by its coefficient in this demand regression. Third, we added these products across demand variables to obtain the change in demand associated with consolidation. Finally, we also calculated the consolidation-induced change in tax price in each of these districts.

We find that in every one of these consolidating pairs, the district with the lowest demand for services also has the lowest tax base. As a result, the consumer surplus loss associated with a consolidation-induced change in the actual level of school services, which is fairly small to begin with, is offset to some degree by the consolidation-induced change in tax price. Moreover, by integrating under the (inverted) demand function between the desired and actual level of services for each of these twelve districts, we can calculate the magnitude of these losses. To make these results comparable to our results for cost savings, we express the consumer surplus loss as a percentage of operating spending. The maximum loss among these twelve districts is 0.002 percent, and the average loss, weighted by households, is 0.0001 percent. Overall, therefore, losses of consumer surplus do not appear to be an important consideration in a benefit-cost analysis of consolidation in New York.

The key policy question, of course, is whether states should encourage school district consolidation. After all, state education departments have played a central role in encouraging and sometimes financially supporting school district consolidation (Haller and Monk 1988). New York backs up its commitment to consolidation with a sizable long-term subsidy to consolidating governments, on the order of \$40 million per year. Our results indicate that some

financial support for consolidation is, indeed, warranted, at least for small districts, because a state can lower the overall cost of education by taking advantage of economies of size.

We also find, however, that capital costs shift upward substantially after consolidation, in part because of increased state aid. The key lesson for state policy makers, we believe, is that they should carefully monitor post-consolidation capital spending. They need to make certain that consolidation and the state aid given to support it do not result in capital projects that are not cost-effective. This is a difficult task, to say the least, because the relationship between capital spending and long-run student performance is poorly understood, but state officials should monitor capital projects based on the best available knowledge about this relationship.

The consolidation of school districts remains an important issue in state education policy. This paper shows how the cost impacts of consolidation can be evaluated and finds strong evidence that consolidation invokes economies of size for rural school districts. Consolidation and the state aid that accompanies it also appear to boost school districts' capital costs, however, thereby offsetting a portion of the enrollment-based cost savings. Overall, consolidation appears to be a valuable a cost-reduction strategy for rural school districts, but states need to carefully monitor capital spending by consolidating districts and to make certain that their aid programs do not encourage wasteful capital projects.

Table 1
New York School Districts Consolidating Between 1987 and 1995

District Pair	Year of Consolidation	Enrollment^a	District Pair	Year of Consolidation	Enrollment^a
Bolivar	1995	690	Dannemora	1989	250
Richburg		380	Saranac		1360
Bolivar-Richburg		1070	Saranac		1610
Cobleskill	1994	1860	Broadalbin	1988	970
Richmondville		390	Perth		620
Cobleskill-Richmondville		2250	Broadalbin-Perth		1590
Cohocton	1994	250	Cherry Valley	1988	480
Wayland		1640	Springfield		250
Wayland-Cohocton		1890	Cherry Valley-Springfield		730
Savona	1993	420	Jasper	1988	490
Campbell		710	Troupsburg		250
Campbell-Savona		1130	Jasper-Troupsburg		740
Cuba	1992	1010	Draper	1987	1990
Rushford		310	Mohonasen		920
Cuba-Rushford		1320	Mohonasen		2910
Mount Upton	1991	270	Edwards	1987	290
Gilbertsville		260	Knox Memorial		420
Gilbertsville- Mount Upton		530	Edwards-Knox		710

^a Enrollment in the year before consolidation.

Table 2
Comparison of Per-Pupil Spending and Revenue for Consolidating
and Non-consolidating School Districts in New York in 1985 and 1997^a

Expenditure Category (Inflation-adjusted dollars) ^b	1985			1997	
	Districts That Have Consolidated	Rural Districts Not Consolidating		Districts That Have Consolidated	Rural Districts Not Consolidating
Aggregate spending:					
Total	\$6,516	\$7,236	*	\$11,935	\$9,934
Total without capital (with debt service)	\$6,251	\$6,828	*	\$9,128	\$9,016
Operating (all but capital and debt)	\$5,979	\$6,485	*	\$8,255	\$8,435
Capital spending	\$265	\$407	**	\$2,807	\$918
Spending by function:					
Instructional	\$4,001	\$4,330	*	\$5,920	\$5,973
Teaching	\$3,680	\$3,952	*	\$5,346	\$5,437
Non-instructional	\$2,243	\$2,562	*	\$5,141	\$3,380
Operating and maintenance	\$708	\$882	*	\$3,257	\$1,382
Central administration	\$467	\$459		\$528	\$593
Transportation	\$474	\$588	*	\$637	\$644
Total revenue per pupil					
Local	\$2,143	\$2,986	*	\$2,370	\$3,990
Federal	\$302	\$320		\$454	\$402
State	\$4,261	\$3,891	**	\$6,596	\$4,918
Operating aid	\$2,606	\$2,710		\$2,030	\$2,664
Reorganization aid	\$0	\$9		\$274	\$9
Building aid	\$132	\$171		\$202	\$361
Transportation aid	\$297	\$408	*	\$325	\$413
Average teacher salaries:					
1-5 years of experience	\$22,074	\$23,557	*	\$28,685	\$29,181
11-15 years of experience	\$31,045	\$34,529	*	\$36,103	\$37,023
21-25 years of experience	\$39,079	\$40,845		\$48,449	\$50,163

* Means for consolidating and non-consolidating districts are statistically different at 5 percent significance level.

** Means for consolidating and non-consolidating districts are statistically different at 10 percent significance level.

^aTwelve pairs of districts consolidated between 1987 and 1995, and are used in the calculation. Rural districts not consolidating from 1985 to 1997 are used as comparison. Sample size is 2,747.

^bAdjusted using the fixed weighted GNP price deflator for state and local government purchases published by the U.S. Bureau of Economic Analysis.

Table 3
Class Sizes, Fiscal Capacity, Student Characteristics and Outcomes
for Consolidating and Non-consolidating School Districts in New York in 1985 and 1997^a

District Characteristics	1985			1997	
	Districts That Have Consolidated	Rural Districts Not Consolidating		Districts That Have Consolidated	Rural Districts Not Consolidating
Staffing Ratios					
Pupils per teacher	15.1	15.5		15.3	14.0
Pupils per school administrator	358.0	425.4	**	472.9	442.0
Fiscal capacity (adjusted for inflation):^b					
Property wealth per pupil (thousands)	\$114	\$167	*	\$155	\$253
Income per pupil	\$32,334	\$34,318		\$38,144	\$42,002
School Size and Number:					
Median elementary school enrollment ^c	407.0	450.3		431.7	462.4
Median high school enrollment ^c	427.3	539.1	*	541.0	515.5
Number of schools	1.7	2.3	*	3.2	2.4
Percent of districts with one school	50%	31%	**	0%	31%
Student Characteristics:					
Enrollment	703	1076	*	1469	1117
Subsidized lunch (percent)	32.2	30.0		24.4	25.4
Percent secondary students	48.5	49.1		46.4	46.3
Student Outcomes:					
Percent of students below minimum competency on PEP tests (3rd and 6th grades)					
Math	11.0	11.4		0.2	0.6
Reading	10.9	10.0		5.3	5.9
Dropout rate (percent)	3.7	3.7		2.3	2.3
College going rate (percent)	17.2	21.8	*	31.1	35.2
Percent receiving Regents Diploma	44.6	48.4		43.6	45.6

* Means for consolidating and non-consolidating districts are statistically different at 5 percent significance level.

** Means for consolidating and non-consolidating districts are statistically different at 10 percent significance level.

^a Twelve pairs of districts consolidated between 1987 and 1995, and are used in the calculation. Rural districts not consolidating from 1985 to 1997 are used as comparison. Sample size is 2,747.

^b Adjusted using the implicit GNP deflator for state and local government purchases published by the U.S. Bureau of Economic Analysis.

^c For those districts with only school, the one school was counted as both a high school and elementary school in calculating school size.

Table 4
Comparison of Per-Pupil Spending and Revenue
for Consolidating Districts in New York Before and After Consolidation^a

Expenditure Categories	Inflation-adjusted Dollars^b		Annual Percent Change (inflation-adjusted)^b			
	Districts Before Consolidation	Districts After Consolidation	Districts Before Consolidation	Districts After Consolidation		
Aggregate spending:						
Total	\$8,129	\$11,977	*	7.2%	14.0%	
Total without capital (with debt service)	\$7,524	\$9,809	*	6.0%	7.8%	
Operating (all but capital and debt)	\$7,066	\$8,002	*	5.6%	1.5%	*
Capital spending	\$604	\$2,168	*	64.5%	171.2%	*
Spending by function:						
Instructional	\$4,844	\$5,715		6.4%	2.0%	*
Teaching	\$4,465	\$5,137		6.2%	1.9%	*
Non-instructional	\$2,826	\$4,455	*	8.9%	22.5%	**
Operating and maintenance	\$985	\$2,575	*	23.6%	69.1%	*
Central administration	\$610	\$549	*	9.9%	-0.7%	*
Transportation	\$566	\$616	*	7.6%	5.0%	
Total revenue per pupil						
Local	\$2,202	\$2,300		4.3%	0.4%	*
Federal	\$329	\$391	*	3.4%	6.6%	*
State	\$5,316	\$7,476	*	6.1%	11.6%	
Operating aid	\$3,115	\$1,829	*	-3.2%	-19.7%	*
Reorganization aid	\$0	\$284	*	na	na	
Building aid	\$210	\$150	**	14.8%	7.9%	
Transportation aid	\$337	\$250	*	-4.9%	-10.0%	
Average teacher salaries:						
1-5 years of experience	\$24,762	\$29,091	*	5.0%	1.6%	*
11-15 years of experience	\$33,678	\$37,506	*	4.2%	0.9%	*
21-25 years of experience	\$42,229	\$48,606	*	4.3%	1.7%	

* Means for consolidating and non-consolidating districts are statistically different at 5 percent significance level.

** Means for consolidating and non-consolidating districts are statistically different at 10 percent significance level.

¹Twelve pairs of districts consolidated between 1987 and 1995, and are used in the calculation.

²Adjusted using the fixed weighted GNP price deflator for state and local government purchases published by the U.S. Bureau of Economic Analysis.

Table 5
Cost Regression Results
for Rural School Districts in New York, 1985 to 1997^a

Variable	Operating		Capital	
	Coefficient	t-statistic	Coefficient	t-statistic
Intercept	11.87621	18.06	-24.56679	-2.21
Log of enrollment^b	-1.61191	-11.58	-0.39554	-5.08
Square of log of enrollment^b	0.09865	8.29		
Consolidation intercept change (average)	0.20071	7.58	0.19758	0.26
Consolidation time trend change (average)	-0.04099	-4.14	0.14168	1.26
Outcomes				
Percent of students below minimum competency on PEP tests (3rd and 6th grades)	-0.00219	-2.33	-0.01774	-1.64
Dropout rate (percent)	0.01494	1.98	0.03311	1.54
Percent of graduates receiving Regents Diploma	-0.00272	-3.80	0.00754	2.22
Log of teacher salaries (1-5 years)	-0.00359	-0.13	-0.04441	-0.11
Other cost factors				
Percent secondary students	0.00056	0.62	-0.00476	-0.81
Percent receiving subsidized lunch	-0.00095	-4.16	-0.00365	-1.64
Efficiency factors				
Total state aid ratio ^c	0.23476	1.83	16.57100	1.86
Log of property values ^d	0.10143	9.65	3.180E-07	3.06
Log of average income	0.16495	4.86	1.62855	1.7
Superintendent change in last 2 years (1=yes)	0.00392	1.35	-0.03410	-2.2
State aid reference group:				
Districts with similar enrollment	0.75995	6.29		
Districts in same county	1.85505	6.98		
SSE		6.35		196.94
Adjusted R ²		0.9228		0.8217
Sample size		2703		2689

^a Estimated using linear 2SLS regression with district fixed effects and trend variables. Student outcomes, state aid, and teacher salaries are treated as endogenous. The dependent variable for operating costs is the log of per pupil spending. The dependent variable for capital cost model is the 9-year average of the log of per pupil capital spending adjusted for depreciation using a 2 percent annual rate. Instruments in the operating model are the average adjacent-district value for enrollment and Regents Diplomas; the minimum adjacent-district value for the PEP and Regents variables, salaries, and state aid; and the maximum adjacent district value for pupils and the dropout rate. Instruments in the capital regression are the average adjacent district value for income and property value; the minimum adjacent district value for income, pupils, the PEP variable, the dropout rate, property values, and state aid; and the maximum adjacent district value for the PEP variable.

^b The enrollment measure for the operating cost model is average daily membership (ADM). For the capital cost model, enrollment is the 9-year average of ADM.

^c State Aid for the operating cost model is per pupil total state aid divided by average income. For the capital cost model, the 9-year average of state aid per pupil is divided by average income in that year.

^d In the capital model, this variable is log of the 9-year average of property values.

Table 6
Coefficients of Enrollment Variables and Estimates of Economies of Size,
New York Rural School Districts, 1985 to 1997

Expenditure Category (Inflation-adjusted dollars) ^b	Regression Coefficients		Minimum	Economies of Size ^a		
	Enrollment		Cost	From 300 Pupils	From 900 Pupils	From 1500 Pupils
	Enrollment	Squared	Enrollment	to 600 Pupils	to 1800 Pupils	to 3000 Pupils
Spending by object:						
Operating (all but capital) (t-statistic)	-1.612 (-11.58)	0.099 (8.29)	3,532	-28.8%	-13.9%	-7.0%
Capital spending (t-statistic)	-0.396 (-5.080)		na	-24.0%	-24.0%	-24.0%
Spending by function:						
Instructional (t-statistic)	-1.596 (-11.51)	0.102 (8.66)	2,463	-24.8%	-9.3%	-2.1%
Teaching (t-statistic)	-1.565 (-10.85)	0.096 (7.78)	3,529	-27.9%	-13.5%	-6.8%
Non-instructional						
Central administration (t-statistic)	-0.803 (-14.44)		na	-42.7%	-42.7%	-42.7%
Transportation (t-statistic)	-0.440 (-11.65)		na	-26.3%	-26.3%	-26.3%

^aCalculation of percent change is based on enrollment coefficients for log of enrollment (labeled "Enrollment") and square of log of enrollment (labeled "Enrollment Squared"). Estimated cost change from consolidation is divided by pre-consolidation cost.

^bAdjusted using the fixed weighted GNP price deflator for state and local government purchases published by the U.S. Bureau of Economic Analysis.

Table 7
Estimated Economies of Size Using Alternative Procedures

	Sample		
	Full Sample ^a	Sample of Non-Consolidating Districts ^b	Trimmed Sample Based on Propensity Scores ^c
Spending by object:			
Operating			
Enrollment	-1.612 (-11.58)	-1.663 (-12.15)	-1.662 (-10.99)
Enrollment Squared	0.099 (8.29)	0.103 (8.90)	0.103 (7.96)
Capital spending			
Enrollment	-0.396 (-5.08)	-0.430 (-5.54)	-0.386 (-4.92)
Spending by function:			
Instructional			
Enrollment	-1.596 (-11.51)	-1.675 (-11.25)	-1.636 (-11.50)
Enrollment Squared	0.102 (8.66)	.110 (8.68)	0.105 (8.71)
Teaching			
Enrollment	-1.565 (-10.85)	-1.632 (-10.72)	-1.607 (-10.76)
Enrollment Squared	0.096 (7.78)	0.102 (7.89)	0.099 (7.77)
Non-instructional			
Central administration			
Enrollment	-0.803 (-14.44)	-0.774 (-8.54)	-0.825 (-13.66)
Transportation			
Enrollment	-0.440 (-11.65)	-0.399 (-9.42)	-0.438 (-10.31)

^a These estimates are taken from Table 6.

^b These estimates exclude the 24 districts that consolidated during the sample period.

^c These estimates exclude 15 non-consolidating districts with a propensity score in three or more years that falls below the minimum observed for consolidating districts in the year before consolidation.

Table 8
Effects of Consolidation Not Associated with Enrollment,
New York Rural School Districts

	Total	Operating				Capital
	All	Instructional	Teaching	Administration	Transportation	
Regression coefficients						
Average intercept (t-statistic)	na (7.58)	0.201 (7.51)	0.185 (7.85)	0.200 (2.40)	0.250 (6.00)	0.271 (0.26)
Average time trend (t-statistic)	na (4.14)	-0.041 (-4.15)	-0.037 (-3.34)	-0.031 (-2.90)	-0.063 (-1.38)	-0.035 (1.26)
Years Until Adjustment	na	5.0	5.0	6.6	4.0	7.9
Costs Phase Out^a						
Adjustment Costs	4.6%	1.3%	1.2%	1.8%	1.2%	2.8%
Over 30 Years^b						37.4%
Aid-induced Cost Effects	1.4%	0.2%				13.0%
Over 30 Years^c						
Performance Feedback	-1.3%	0.0%				-14.5%
Over 30 Years^d						

^a The adjustment costs equal the intercept (a) plus "time since consolidation" multiplied by the trend coefficient (b). This sum equals zero when time since consolidation equals -a/b, which is the ratio reported in this row.

^b This row presents the average percentage increase in costs over a 30 year period in present value terms based on the coefficients at the top of the table. Operating and capital spending are combined using their average shares in non-consolidating districts in 1997. Adjustment costs are set at zero once they reach the phase-out point in the previous row. Capital costs are phased out starting in year 9 after consolidation (at the rate indicated by the time trend but with the opposite sign). Capital adjustment costs are spread out over time as indicated by the specification of the post-consolidation intercepts and time trends in the appendix. The discount rate is set at 5%.

^c This row presents the estimated impact of state aid on costs based on the average increase in aid each year after consolidation multiplied by the aid coefficient in the relevant regression. Aid effects for capital are spread out over time as indicated by the specification of the aid variable. As in the previous row, results are expressed in present-value terms with a 5% discount rate.

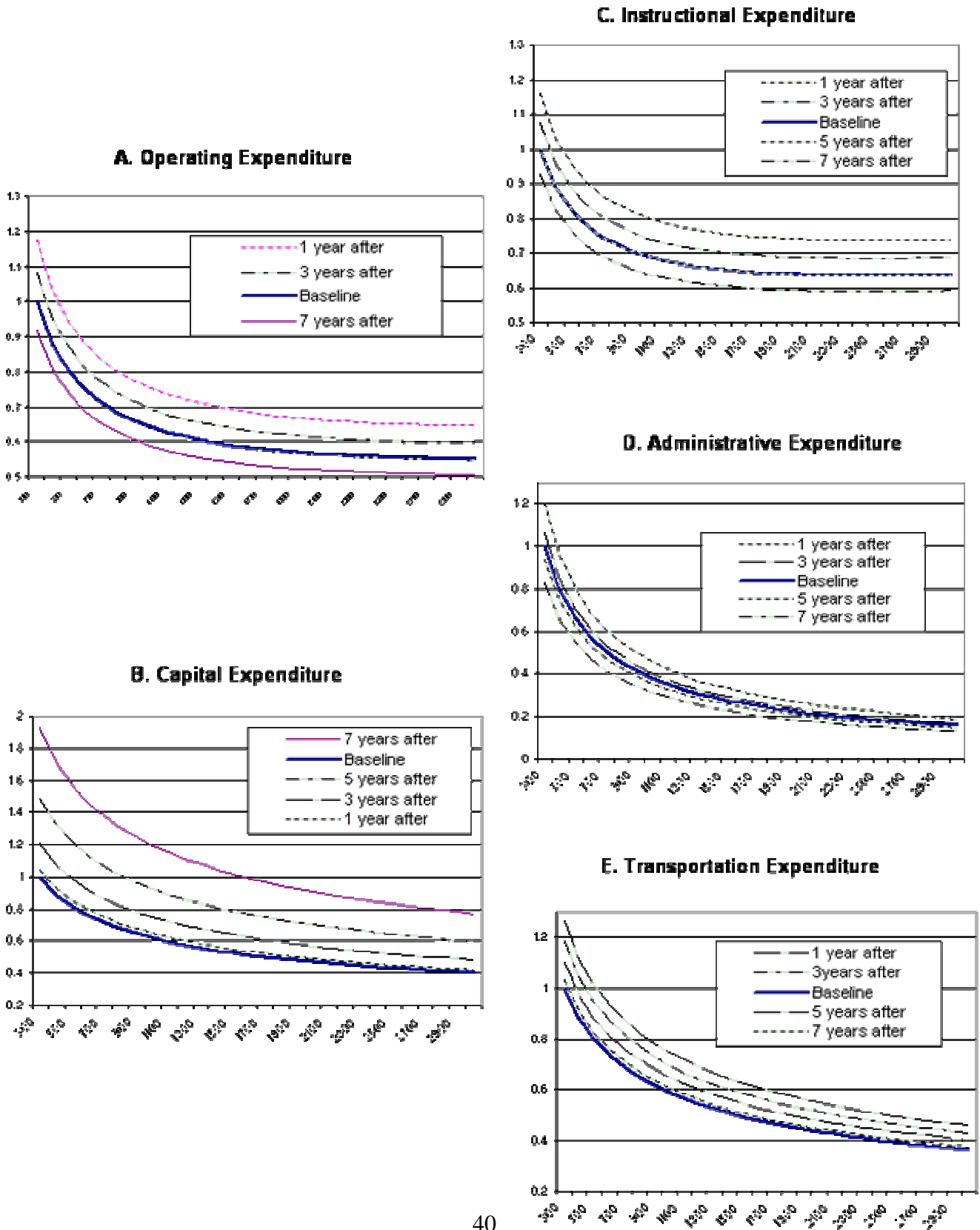
^d The performance feedback for capital is based on the coefficient for the Regents Diploma variable in Table 5. Following the specification of the dependent variable, capital spending this year is translated into performance impacts over the next 9 years. These performance impacts are constrained so that the Regents Diploma variable never exceeds 100 percent. The cost function is then shifted downward to remove these performance impacts in the years in which they appear.

Table 9
Estimated Annual Net Cost Changes from Consolidation,
New York School Rural Districts

	Type of Consolidation		
	From 300 Pupils to 600 Pupils	From 900 Pupils to 1800 Pupils	From 1500 Pupils to 3000 Pupils
Enrollment Effects Only	-28.3%	-14.9%	-8.5%
Non-Enrollment Cost Effects of Consolidation (over 30 years)			
Adjustment Costs	4.6%	4.6%	4.6%
Aid-Induced Cost Effects	1.4%	1.4%	1.4%
Performance Feedback	-1.3%	-1.3%	-1.3%
Total Cost Impacts of Consolidation (over 30 years)	-23.7%	-10.2%	-3.9%

Source: Tables 6 and 7.

Figure 1: Predicted Expenditures Per-Pupil at Different Enrollment Levels and Different Times After Consolidation (Compared to a Non-consolidating District with 300 Pupils)



Technical Appendix

I. District-Specific Variables in a Model of Consolidation

1. Definitions

Let superscripts define variables and subscripts define observations. Also, let E = spending per pupil; X = explanatory variables; D^i = dummy for district i ; i^* = consolidation partner for district i ; C = consolidation dummy (= 1 for district i in year t if district i is consolidated with another district in year t and = 0 otherwise); w = district weight (= district's share of total enrollment in its consolidated district in the year **before** consolidation and = 0 for districts that do not consolidate); t = time (1985=1); t^* = value of t in the year before consolidation (= 0 in districts that do not consolidate); N = number of districts; and M = number of districts that consolidate. Note that if district i consolidates, $w_i + w_{i^*} = 1$.

2. District-Specific Fixed Effects and Time Trends

Define

$$\begin{aligned} F1^i &= \text{district fixed effect} \\ &= D^i(1-C) + w_i C(D^i + D^{i^*}); \quad i = 1, N, \end{aligned}$$

and
$$T1^i = \text{district time trend} \\ = (F1^i)(t); \quad i = 1, N.$$

3. Post-Consolidation Fixed Effects and Time Trends

Let $j = j^{\text{th}}$ consolidating pair; j_1 = value of i for 1st district in pair j ; and j_2 = value of i for 2nd district in pair j . Now define

$$F2^j = C(D_1^j + D_2^j); \quad j = 1, M / 2$$

and

$$T2^j = C(D_1^j + D_2^j)(t - t^*); \quad j = 1, M / 2.$$

To estimate the average value of the pair effects, define $F2^* = C$ and $T2^* = C(t - t^*)$.

Now drop one post-consolidation district-pair effect and time trend and replace them with

$F2^*$ and $T2^*$. Then redefine the district-pair effects and trends as follows:

$$F2^{*j} = F2^j - \frac{2C}{M}; \quad j = 1, M / 2$$

$$T2^{*j} = T2^j - \frac{2C(t - t^*)}{M}; \quad j = 1, M / 2$$

With these variables, the coefficients of $F2^*$ and $T2^*$ are average effects, and their standard errors are the appropriate standard errors for the average effects.

4. District-Specific Variables for Capital Spending

The use of average, or long-term capital spending fundamentally alters the district-specific variables, which now pick up the role of unobserved factors in different time periods.

To account for this, we express the district fixed effects and time trends as averages, too.

Specifically,

$$F1^i = \frac{1}{9} \sum_{t'=t-8}^t \left[D^i (1 - C_{it'}) + w_i C_{it'} (D^i + D^{i*}) \right] \frac{1}{(1+d)^{t-t'}}; \quad i = 1, N, ,$$

where d is a depreciation rate for capital, which is also used to define average spending. Also,

$$T1^i = \frac{1}{9} \sum_{t'=t-8}^t \left[D^i (1 - C_{it'}) + u_i C_{it'} (D^i + D^{i'}) \right] \frac{t'}{(1+d)^{t-t'}}; \quad i=1, N.$$

The post-consolidation variables, as before, apply to pairs of consolidating districts:

$$F2^j = \frac{1}{9} \sum_{t'=t-8}^t \left[C_{it'} (D^{j1} + D^{j2}) \right] \frac{1}{(1+d)^{t-t'}}; \quad j=1, M/2$$

$$T2^j = \frac{1}{9} \sum_{t'=t-8}^t \left[C_{it'} (D^{j1} + D^{j2}) \right] \frac{t' - t^*}{(1+d)^{t-t'}}; \quad j=1, M/2.$$

In addition,

$$F2^* = \frac{1}{9} \sum_{t'=t-8}^t \frac{C_{it'}}{(1+d)^{t-t'}}$$

$$T2^* = \frac{1}{9} \sum_{t'=t-8}^t \frac{C_{it'} (t' - t^*)}{(1+d)^{t-t'}}$$

and

$$F2^{*j} = F2^j - \frac{2}{9M} \sum_{t'=t-8}^t \frac{C_{it'}}{(1+d)^{t-t'}}; \quad j=2, M/2$$

$$T2^{*j} = T2^j - \frac{2}{9M} \sum_{t'=t-8}^t C_{it'} \left(\frac{t' - t^*}{(1+d)^{t-t'}} \right); \quad j=2, M/2.$$

When the sets $F2^{*j}$ and $T2^{*j}$ are included, the coefficients of $F2^*$ and $T2^*$ yield the average post-consolidation shift and time-trend, respectively, with the appropriate standard errors.

II. Measuring Consumer Surplus

A calculation of net consumer surplus losses requires a demand function for education along with information concerning the impact of consolidation on all the variables that influence demand, including the property tax base. Duncombe and Yinger (2001) conduct a regression

analysis of the demand for education performance in New York State in 1990 using a dependent variable that is an index based on two of the three performance variables used in this paper. This analysis identified four factors other than state aid and tax price with a significant impact on demand: median income, the percentage of workers classified as managers, the percentage of housing that is owner-occupied, and the percentage of housing built before 1940.

The demand function for education in Duncombe and Yinger (2001) is $S = Q + \mu \ln(T)$, where S is an index of educational performance, Q is a function of demand factors, μ is an estimated coefficient, and T is tax price. To obtain desired school performance in unconsolidated district i , we substitute into this formula the value of Q in district i before consolidation and the value of T in district i before consolidation adjusted for the change in tax-base per pupil that is caused by consolidation. To obtain actual performance in the consolidated district that included district i , we substitute into this formula the values of Q and T in this consolidated district (weighted averages for district i and its new partner). To find the lost consumer surplus, we then invert the demand function used to obtain desired performance in district i , integrate it between the desired and actual performance, and subtract this integral from the relevant rectangle (the value of T in district i before consolidation adjusted for the consolidation-induced change in tax-base per pupil and multiplied by the difference between actual and desired performance). This demand curve applies to the median voter, so it is multiplied by the number of households in district i to obtain the total loss of consumer surplus in the district. Finally, this loss is expressed as a percentage of total operating spending.

Endnotes

¹ See Duncombe and Yinger (1993) for a discussion of the relationship among these three definitions of scale.

² Estimates of the effect of class size below 15 students are hard to come by because classes of that size are not usually observed. For example, Krueger (1999) finds that a shift from 22 to 15 students has significant performance benefits, but does not observe the impact of even smaller class sizes. See also, Ferguson and Ladd (1996).

³ Riew (1986) includes teacher quality measures rather than salaries in the regression.

⁴ Since teacher salaries are set by the school board, often through contract negotiations with the union, they are in fact determined simultaneously with budgets and outcomes. Downes and Pogue (1994), Duncombe, Ruggiero, and Yinger (1996), Duncombe and Yinger (1997, 1998, 2000), Reschovsky and Imazeki (1997, 2001), and Imazeki and Reschovsky (2004) treat teacher salaries as endogenous.

⁵ Two studies have employed stochastic frontier regression methods (Deller and Rudnicki, 1992; Duncombe, Miner, and Ruggiero, 1995) to account for efficiency in a cost function. They generally did not find large differences between the results of the frontier and OLS regressions with regard to the enrollment variables. Downes and Pogue (1994) employ panel methods to control for district-specific effects and find a statistically significant relationship between enrollment and expenditures. Duncombe, Ruggiero, and Yinger (1996) include in their cost model an efficiency index produced using a linear programming approach, called DEA. This approach is also used by Duncombe and Yinger (1997, 1998, 2000), Reschovsky and Imazeki (2001) and Imazeki and Reschovsky (2004).

⁶ The “Incentive Operating Aid” subsidy of 40 percent is for consolidations after July 1993. From 1983 until 1993 the incentive aid was 20 percent of operating aid. Capital projects may be reimbursed under “Incentive Building Aid” past ten years, so long as the project is approved within ten years of consolidation. A number of consolidating districts are still receiving building subsidies 20 years after consolidation. A number of districts that were receiving reorganization building aid in 1981 were still receiving this aid in 1997. Most of these districts consolidated before 1979.

⁷ During this period, three elementary school districts, each with fewer than 100 students, merged with much larger K-12 districts. We do not consider these districts in our analysis. Eight consolidations have occurred in New York since the latest one in our data. Although a few of these consolidations took place in our sample period, we do not include them (and we drop the districts involved) because we want to observe at least two years of data after consolidation.

⁸ Total enrollment is our measure of district size; in our sample of districts, this variable is highly correlated with an alternative measure, average daily membership.

⁹ For analysis of the determinants of school district efficiency, see Duncombe, Miner and Ruggiero (1997) and Duncombe and Yinger (1997, 1998, 2000). Unobserved determinants of school district efficiency are an unlikely source of bias in our regressions because they are largely captured by district fixed effects and time trends, which are discussed below.

¹⁰ Two consolidating districts, Draper and Mohonasen, were classified as “upstate suburban districts” by the New York State Department of Education. However, these districts lie on the edge of a small urban area, Schenectady, and are quite rural in character.

¹¹ Some studies, such as Gyimah-Brempong and Gyapong (1991), use a general trans-log specification, but this approach is not practical with the sample size available for this study.

¹² For any given district size, the size of schools may affect student performance and costs. It is not appropriate to include school size in a cost model, however, because it reflects administrative decisions that influence efficiency, not exogenous factors that influence costs.

¹³ We specify these variables so that one coefficient provides the state average post-consolidation shift and one provides the state average post-consolidation trend. See the technical appendix.

¹⁴ The role of fixed-effects is discussed in Heckman, LaLonde, and Smith (1999); Bloom (1984) provides an example involving both fixed effects and time trends.

¹⁵ Formal definitions of district fixed effects and trends are in the technical appendix.

¹⁶ The fundamental problem is that it is impossible to predict the probability of consolidation in a district after it has consolidated. Thus, for example, we cannot use the Ziliak and Kneisner (1998) IV method for panel data, because it applies to observations that drop out of the sample at a point in time (due, in their case, to unemployment).

¹⁷ With our data, standard propensity-score methods encounter the same problem as IV methods: they do not apply when some observations combine and remain in the sample.

¹⁸ We cannot use the matching rate as a variable because we do not observe it. Moreover, building aid in New York uses a closed-ended matching formula, and we cannot identify districts at the maximum spending level, at which point the matching rate no longer applies.

¹⁹ To implement rules (2) and (3), we rejected potential instruments that did not have a t-statistic of at least 1.5 in a regression to explain at least one of the performance indicators or that had a t-statistic of 1.3 or higher when included in the cost regression itself.

²⁰ The Bound, Jaeger, and Baker (1995) procedure is not formally specified for a model

with as many endogenous variables as ours. Neither is the alternative procedure developed by Stock and Yogo (2003). Thus, we examined various combinations of the instruments and used the set that produced the highest F-test for most endogenous variables. The F-statistics for our final runs indicate that we have strong instruments in most cases. As discussed later in the text we have five endogenous variables and six regressions. All five of the F-statistics (one for each endogenous variable) in the capital regression and all six of the F-statistics for the PEP and salary variables (one for each regression) are above 18.0. The drop-out variable has an F-statistic above 33.0 in every regression except the one for operating spending, for which its F-statistic is 3.14. The regents-exams variable usually has an F-statistic close to 7.0, except in the capital regression (43.2). Finally, the aid variable has an F-statistic above 24.0 in every regression except administration (1.51) and transportation (1.03).

²¹ The 2 percent rate is based on the assumption of a 50-year useful life and linear depreciation. Our results are similar with 5 percent depreciation and with no depreciation.

²² The property-value variable is also defined as a nine-year average. The two aid variables based on comparisons with other districts, which are not averages over time, are dropped from the capital regression (but are insignificant if included). The averaging procedure for the dependent variable does not require any change in other explanatory variables because trends in these variables are accounted for by the district-specific time trends.

²³ Details are presented in the technical appendix.

²⁴ New York State classifies school districts into different region and district types, such as “downstate suburb” or “upstate rural.” The upstate rural designation applies generally to non-city districts in a county that is not part of a metropolitan area. During our sample period, 216 non-consolidating districts were classified as rural; because of missing data, however, only

187 districts were used as the comparison group.

²⁵ Specifically, the data we used to construct the panel come from the *School District Fiscal Profile*, the *Comprehensive Assessment Report*, the *Personnel Master File* and the *Institutional Master File* published by the State Education Department. Spending, federal aid, income, and property value data are from *The Special Report on Municipal Affairs* from the New York State Comptroller.

²⁶ Standard theory calls for median income instead of income per pupil, but these two variables are highly correlated. Standard theory also indicates that the income term should be median income plus the product of state aid per pupil and tax price. Our specification for the aid variable is designed to approximate this additive income term. For more on these specification issues, see Ladd and Yinger (1991) and Duncombe and Yinger (1997, 1998, 2000).

²⁷ In a few districts we did not have data on every variable in every year. We filled in data when we had a plausible method for doing so. Thus, for example, we occasionally filled in poverty with the average values for the previous and succeeding years and filled in teacher salaries with the average for similar districts in the same county. A few districts were dropped for lack of data, such as missing data on test scores.

²⁸ One explanation may be an inadequate inflation adjustment; we are using a national inflation rate that may not adequately capture price changes in New York. Real per pupil expenditure in New York rose significantly during this period due in part to rapid increases in special education spending (Lankford and Wyckoff, 1996).

²⁹ Regression results for the subcategories are available from the authors upon request.

³⁰ One possibility is that these coefficients reflect the impact of temporary deviations from performance or subsidized-lunch trends, which are likely to have different impacts on costs

than the trends themselves. An unusually talented fourth-grade cohort, for example, might produce higher PEP test scores while saving money on remedial programs. An alternative specification provides some evidence to support this interpretation. We replaced the three performance variables and the poverty variable (and the two efficiency variables discussed at the end of this paragraph) with the deviations in these variables from their linear time trends. For every variable except the dropout rate, which is not significant, the sign and significance of these deviations variables are the same as reported in Table 5.

³¹ As a robustness check, we also included all failed instruments as additional efficiency variables. These variables involve comparisons between a district and either its neighbors or other comparison districts. As a result, they all have a clear conceptual link to efficiency because they could reflect comparisons that induce parents to alter their monitoring activities and school administrators to alter their management activities. Although some of these variables were statistically significant, their inclusion had a minor impact on our estimates of economies of size and of the non-enrollment cost impacts of consolidation.

³² Our model is estimated with the Cox proportional hazard option in Stata (Stata Corporation, 2003). We cannot reject the proportional-hazard assumption for any variable or for the equation as a whole. Detailed results are available from the authors upon request.

³³ This conclusion also holds for post-consolidation shift and trend variables, which are discussed in the next section.

³⁴ Only one result, the average trend for transportation, is not statistically significant.

³⁵ Taken literally, these results also indicate that after about 7 years there are cost savings from consolidation that are not related to enrollment. We know of no reason to expect that two otherwise similar 600-pupil districts, one of which was created from two 300-pupil districts and

the other of which was not, will have systematically different operating costs in the long run. To avoid exaggerating the operating cost savings from consolidation, therefore, we do not include these cost savings in our calculations.

³⁶ More formally, we regress our state aid variable on a fixed effect and time trend for each district and a set of time dummies for each year after consolidation. The coefficient of the time dummy for the t -th year after consolidation is our measure of the aid increase in that year.

³⁷ We also calculate an aid effect for operating spending, but as shown in Table 8, this effect is very small. Consolidation also might alter district efficiency by altering district incomes or property values. In fact, however, the same procedures we used for aid effects indicate that consolidation-induced changes in these variables have only a tiny impact on capital and operating spending.

³⁸ This logic is also central to Brasington's (1999, 2003) analysis of the fiscal incentives that lead to consolidation and to Fisher and Wassmer's (1998) analysis of the number of school districts in a metropolitan area.

³⁹ Four of these districts consolidated after 1995 and are therefore not in our sample. We cannot apply this analysis to districts that consolidated before 1990 because we cannot observe the census-based demand variables for these districts individually. Our procedures are described in the technical appendix.

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