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Abstract

Over the last 50 years, consolidation has dramatically reduced the number of school districts in the United States, and state governments still recommend consolidation, especially in rural school districts, as a way to improve school district efficiency. However, state policies encouraging consolidation are often challenged on the grounds that they do not lead to cost savings and instead foster learning environments that harm student performance. Existing evidence on this topic comes largely from educational cost functions, which indicate that instructional and administrative costs are far lower in a district with 3,000 pupils than in a district with 100 pupils. However, research on the cost consequences of consolidation itself is virtually nonexistent. This paper fills this gap by evaluating the cost impacts of consolidation in rural school districts in New York over the 1985 to 1997 period. Holding student performance constant, we find evidence that school district consolidation substantially lowers operating costs, particularly when small districts are combined. The operating cost savings ranges from 22 percent for two 300-pupil districts to 8 percent for two 1,500-pupil districts. In contrast, consolidation lowers capital costs only for relatively small districts, and capital costs increase substantially when two 1,500-pupil districts come together. Overall, consolidation is likely to lower the costs of two 300-pupil districts by over 20 percent, to lower the costs of two 900-pupil districts by 7 to 9 percent, and to have little, if any, impact on the costs of two 1,500-pupil districts. State aid to cover the adjustment costs of consolidation appears to be warranted, but only in relatively small districts.

Introduction

School consolidation represents the most dramatic change 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 (NCES 1999, Table 90). This longstanding 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 efficiency consequences of consolidation. Using a unique panel data set for school districts in New York State, we ask whether consolidation leads to significant cost savings, after controlling for student performance. This paper therefore complements recent research on the causes of consolidation (Brasington 1999).

While the pace of school district consolidation has slowed since the early 1970s, some states still provide incentives to consolidate. For example, New York and at least seven other states have separate aid programs designed to encourage school district "reorganization," typically in the form of district consolidation (Gold et al. 1995). Some other states informally encourage consolidation through their distribution of building or transportation aid (Haller and Monk 1988). However, many state governments provide mixed incentives concerning consolidation. In particular, close to half the states use operating aid formulas that compensate school districts for sparsity or small scale (Gold et al. 1995, and Verstegen 1990).

The principal policy question raised by consolidation is that of school district efficiency: Can consolidation help districts lower the per-pupil cost of obtaining a given student performance? Although scholars do not agree on the answer to this question, consolidation is often seen as a way to lower costs. As a result, consolidation is likely to remain on the education policy agenda in many states, particularly when school districts are under heavy pressure to cut costs and raise student performance. As Haller and Monk (1988) put it, “the modern reform movement is likely to prompt additional school district reorganization efforts, despite its virtual silence on the question of size” (p. 479).

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 brief synthesis of the recent “cost function” literature and evaluates the existing evidence on economies of size in education. While the cost function literature is suggestive, it does not directly examine the impact of consolidation. Thus, in the third section, we present an evaluation of school district consolidation in New York from 1985 to 1997. This evaluation is based on a panel of rural school districts, some of which consolidated during the sample period. This data set makes it possible to determine the impact of consolidation on per-pupil costs, controlling for other factors.

Economies of Size and the Effects of Consolidation

By definition, school district consolidation alters the size of participating districts, and any analysis of consolidation must begin with a look at the impact of district size on educational costs. In this section we define economies of size and discuss the possible benefits and costs of consolidation.

Defining Economies of Size

The traditional concept of economies of scale refers to the relationship between average costs and output. In education, output is a difficult concept to define because educational services are multi-dimensional and involve the actions of many personnel. The most general formulation in the literature is to say that educational output is defined by student performance and that this output is produced by a combination of inputs supplied by a school, such as teachers, and fixed inputs, such as student characteristics. Even in this context, however, the notion of scale can be defined in several different ways.

The focus of this paper and of most empirical research on economies of scale in education is on economies of size, which refer to the relationship between per-pupil expenditure and enrollment, after accounting for other factors that might influence spending. This relationship can be estimated from an educational cost function, which controls for output (that is, student performance), input prices, and other factors. Economies (diseconomies) of size exist if the estimated elasticity of per-pupil educational costs with respect to enrollment is less than (greater than) zero. Economies of size must be distinguished from economies of quality scale or economies of scope. See Duncombe and Yinger (1993).

Benefits of Consolidation

The conventional wisdom is that consolidating small districts (in terms of enrollment), particularly those in rural areas, can result in significant cost savings. Tholkes (1991) and Patten (1991) have identified five sources of long-run economies of size that seem especially pertinent to education.

Indivisibilities. To some degree, education may have the properties of a public good. Specifically, 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. While it is likely that additional administrators need to be added at some enrollment level, the same central administration may be able to serve a significant range of enrollments. To a lesser extent, teachers may provide a public good over some range of enrollment because they may be able to teach up to, say, 20 students without a significant drop in the quality of education they provide.[1] School administrators and some support staff, such as librarians and curriculum development staff, may also exhibit some degree of publicness.

Increased Dimension. The traditional long-run concept of economies of scale focused on the 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 the education context, 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. Specialization is linked directly to the concept of publicness; a small school district could employ these specialized staff, but only if they taught very small classes or taught outside their area of expertise, thereby negating the benefits of specialization. The potential gains from specialization 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, assuming that the early implementers share their experience with others. In addition, teachers may be more productive in a large school if they are able to benefit from the experiences of their colleagues. A second-grade teacher in a school with five second-grade classrooms has more colleagues to ask for advice or assistance than a teacher in a school with only two classrooms.

Costs of Consolidation

The conventional view that economies of size exist in education has been challenged by a series of 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). Much of this research has focused on schools rather than districts, and production functions 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 often closely correlated. The decision to consolidate two rural districts may hinge crucially on a decision about sharing one high school. The argument put forth in these studies is that the potential cost savings from consolidation are seldom realized, and that larger schools lead to a learning environment that hurts student performance, particularly for low-income students. The research on effective schools, particularly private schools, has provided additional evidence that moderate-sized schools are more successful at retaining students through high school (Figilio and Stone 1997; Pukey and Smith 1983; Witte 1996).

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

Higher Transportation Costs. One obvious source of higher costs for larger districts is in transportation. To benefit from larger scale, districts generally need to consolidate students into larger schools. Assuming the consolidating districts are sparsely populated, consolidation is therefore likely to result in longer commuting times for at least one group of students.[2]

Labor Relations Effects. Tholkes (1991) has identified the potentially higher teacher costs associated with larger districts: "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). These costs can stem from several sources, including the "leveling up" of wages to those of the most generous district. 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, which eliminates one of the major sources of cost savings associated with consolidation.

Lower Staff Motivation and Effort. Administrators and teachers may have a more positive attitude toward work in small schools, because there is less formalization of rules and procedures; that is, it is easier to be flexible in a small school (Cotton 1996). Smaller organizations are "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 have a greater sense of belonging to the school community, in part, because they are more apt to participate in extracurricular school activities (Cotton 1996). Moreover, in a small school, the personnel are more apt to know students by name, and more importantly, to identify and assist students at risk of dropping out. Thus, students in large schools may have a less positive attitude toward school and a lower motivation to learn (Cotton 1996; Barker and Gump 1964).

Lower Parental Involvement. Parents make a contribution to educational production by reinforcing lessons their children learn in school and by helping to motivate their children. These contributions may be facilitated by parental participation in school activities and by 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 school districts.

Research on Economies of Size and Consolidation

Given these two sets of arguments, the impacts of actual school district consolidations on educational costs must be determined empirically. The vast majority of evidence on economies of size, and, by inference, on consolidation, has come from the estimation of education cost functions.

Fox (1981) provided a detailed review of the literature on economies of size before 1980. The early evidence suggested that significant economies of size did exist, and that high schools between 1,000 to 2,000 students and districts over 10,000 students were optimal.

Fox (1981) pointed out many deficiencies in this research, and studies since 1990 have addressed most of the methodological concerns he raised. Average test scores are the most common measure of student performance, particularly in math and reading, although a few studies use graduation measures. Many studies include factor prices, particularly teacher salaries,[3] and five studies have made teacher quality adjustments, all since 1990.[4] Several recent studies have modeled costs as part of a behavioral system involving the demand for education, and have either estimated a reduced-form expenditure function (Ratcliffe, Riddle, and Yinger 1990; Downes and Pogue 1994), or treated student performance as endogenous (Downes and Pogue 1994; Duncombe, Ruggiero, and Yinger 1996; Duncombe and Yinger 1997, 2000; Reschovsky and Imazeki 1997, 1999). Several recent studies have attempted to control for unobserved factors, such as efficiency.[5]

Despite the variety of measures used and geographic areas examined in these studies, a surprising level of consensus emerges. To be specific, almost all the studies find economies of size over some range of enrollment. Many studies have included a quadratic for enrollment in the cost model and have found a U-shaped cost curve for most types of expenditure (Duncombe, Miner, and Ruggiero 1995; Duncombe, Ruggiero, and Yinger 1996; Duncombe and Yinger 1997, 2000; Reschovsky and Imazeki 1997, 1999). The "optimal" (that is, lowest-cost) district enrollment is approximately 6,000 students for total costs, 1,500 to 3,500 students for operating or instructional costs, and just over 1,000 students for transportation costs. Even for total costs, Duncombe, Miner, and Ruggiero (1995) found that 90 percent of the cost savings are exhausted when a district reaches 1,500 pupils. For New York State, they found that one-half of the cost decrease was due to administrative costs, which dropped from $1,124 per pupil with 50 pupils to $193 per pupil with an enrollment of 1,500.

While cross-sectional spending regressions can provide evidence of potential cost savings from consolidation, a more direct and compelling approach is to evaluate consolidation using longitudinal methods applied to a sample of school districts in which some consolidation actually occurred. Unfortunately, however, no high-quality evaluations of this type have been conducted. Quantitative 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. The best case study is by Streifel, Foldesy, and Holman (1991), who compare pre- and post-consolidation finance data in a national sample of 19 school districts. However, they do not include any controls for student achievement, teacher salaries, or changing student composition. In short, despite widespread consolidations of school districts in the United States, there exists little direct evidence on how consolidation actually affects school districts in the medium or long run. This point is underscored by Howley (1996):

> In the 60 years between 1929 and 1989, consolidation reduced the number of school districts across the United States by 90 percent and the number of schools by 70 percent, yet during those years the number of students increased by 60 percent! 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. (p. 25)

Evaluation of School District Consolidation in New York

This paper attempts to fill the gap in evidence about school district consolidation through a detailed evaluation of this phenomenon in New York State. New York provides an excellent setting for this effort. First, New York actively promotes the consolidation of small districts by providing “reorganization aid” for capital construction and operations to consolidating districts. Specifically, New York State contributes an additional 40 percent in formula operating aid (“Incentive Operating

Aid") to consolidated districts for five years which 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. Second, consolidation also continues to take place in the state.[6] While more consolidations occurred in the 1960s and 1970s, 12 pairs of districts consolidated from 1987 to 1995.[7] These consolidations are described in Table 1.

Evaluation Design

Ideally, an evaluation of consolidation should be based on extensive pre- and post-consolidation data for both a control group and the consolidating districts, which should be randomly selected (Cook and Campbell 1979). Unfortunately, random selection is not feasible with a policy change, such as consolidation, that requires approval by local voters. Thus, systematic differences may exist between consolidating and non-consolidating districts, which could bias the evaluation results. These differences could involve observable factors, such as teacher salaries and student characteristics, or unobservable factors, such as school district management and staff motivation. Differences in the timing of consolidation across school districts also could lead to misleading results. Districts could face different external environments right before or after consolidation, such as differences in the business cycle, inflation, or the level of state aid.

To address these potential threats to internal validity, we have taken several steps. First, we employ a non-equivalent control group design (Cook and Campbell 1979). Pre- and post-consolidation data on variables in the cost model have been assembled for all consolidating districts and a control group, which permits assessment of change within consolidating districts across time and in reference to similar districts. Given that all of the consolidating districts are rural, the remaining 95 percent of rural districts that did not consolidate during this time period are used as the control group.[8]

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. 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. 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. The largest of these differences involve district and school size. Other differences are small and not statistically significant. Overall, the other rural districts are not a perfect match with consolidating districts, but they are similar enough to serve as a reasonable comparison group.

The second major step taken to remove bias in this evaluation is the use of a multivariate regression method that allows us to control for both observable and unobservable differences across school districts. We estimate cost regressions, described in detail below, with explanatory variables that include three different measures of student outcomes, a measure of teacher salaries, and several socio-economic variables. We also employ an interrupted time-series methodology to control for unobservable district effects. To be specific, we estimate a separate fixed effect and time trend for each district and we estimate the impact of consolidation on these coefficients for each consolidating district pair. This approach allows us to separate enrollment effects from the cost effects of consolidation that are not related to enrollment.

The estimation of fixed effects and time trends also deals with the possibility endogeneity of the consolidation variable. Specifically, unobserved factors that influence the consolidation decision might also influence spending per pupil, so estimated coefficients could be biased if these unobserved factors are not taken into account. This is an example of a selection bias, in which the unobserved characteristics of the districts that "select" to consolidate may differ from those of other districts. The estimation of fixed effects using panel data is one way to deal with this problem (see Heckman, LaLonde, and Smith 1999). For example, the selection problem we face is analogous to the selection problem that confronts someone estimating the impact of union membership on wages. Workers who select to join a union may have different unobserved productivity than other workers. Jakobson (1991) explains how individual fixed effects pick up these unobserved factors and therefore solve this selection problem.

Following Bloom (1984), we take this logic one step further by estimating district-specific time trends. A selection bias can also arise if the unobserved factors that lead to consolidation also influence the time trend in a district's spending. District-specific time trends capture the role of any unobserved factors that vary linearly with time and therefore eliminate this type of selection bias. In principle, selection bias could still arise if unobserved district-specific factors that vary in a nonlinear way over time influence both the decision to consolidate and per-pupil spending. This possibility strikes us as remote, but we address it by including in our estimation a variable that might be related to consolidation and that has a nonlinear pattern over time, namely a change in the school superintendent.[9]

Empirical Model

Our objective is to estimate the impact of consolidation on educational costs, controlling for school performance. We begin with the standard formulation of an educational cost function (Downes and Pogue 1994; Duncombe and Yinger 1997, 2000; Reschovsky and Imazeki 1997, 1999), in which the cost of providing school services, as measured by school spending per pupil, E, is a function of school performance, S, which is the output; input prices, P; enrollment, N; environmental cost factors, M, which are outside the control of school officials; and school district efficiency, e.

We also add to this framework a consolidation variable, C, to represent the possible costs impacts of consolidation that are not associated with enrollment change. In symbols

$$E = E(S,\ P,\ N,\ C,\ M,\ e) \tag{1}$$

This cost function can be applied to total spending or to functional subcategories of spending, such as administration, instruction, or transportation.

This approach has two key advantages over a production-function approach. First, it can account for more than one performance variable, and it even provides a statistical basis for determining which performance variables are appropriate.[10] Second, it can summarize the costs associated with all a district's activities, including counseling, health, transportation, and administration. Unlike a production function approach, in other words, a cost approach is not limited to production activities in the classroom.

In principle, several input prices could be included in this approach, but reliable information is available only for the main input price, namely teacher salaries. Environmental factors, M, which are also called fixed inputs, reflect the characteristics of the students in a school district, such as the share who live in poverty. Enrollment, N, could be treated as an environmental cost factor; given its central role in the consolidation debate, however, we treat it separately.[11]

The first challenge in estimating equation (1) is that S and teacher salaries, are influenced by the actions of school officials and are therefore endogenous. As discussed more fully below, instruments for the performance variables come from a model of the demand for education, and instruments for teacher salaries come from the observation that these salaries are linked to local labor market conditions.

The second challenge is that school district efficiency cannot be directly observed. In this context, efficiency is defined as not spending any more than necessary, given input and environmental costs, to provide a given level of performance. Thus, efficiency is inextricably tied to the performance variables included in the regression. A school district is inefficient in this sense 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.

Building on the work of Duncombe, Miner, and Ruggiero (1997) and Duncombe and Yinger (1997, 2000), we argue that this type of school district efficiency is a function of 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. In symbols,

$$e = e(Z) \tag{2}$$

Because school district inefficiency cannot be measured directly, we substitute equation (2) into the cost equation (1) and thereby replace the efficiency variable with the exogenous variables that determine efficiency.[12] In symbols:

$$E = E(S,\ P,\ N,\ C,\ M,\ Z) \tag{3}$$

This derivation relies on the assumption that student characteristics, *N* and *M*, influence the production technology (and hence costs), whereas school district characteristics, *Z*, influence school district efficiency. This assumption has considerable support in the literature, both theoretical and empirical, but we cannot test it. However, this assumption is not essential for estimating economies of size. We assume that changes in *N* caused by consolidation alter educational costs through their impact on educational production. Without this assumption, changes in *N* might also influence educational costs through their impact on school district efficiency. In either case, equation (3) picks up the systematic impact of consolidation on educational costs.

This approach to efficiency also leads to a key insight, and methodological challenge, for our simultaneous-equations procedure. In particular, three of the variables in *Z* that have been identified by previous studies are district income, tax price, and state aid. See Duncombe and Yinger (1997, 2000). Higher income and higher state aid appear to lower the incentives of parents to monitor a school district's performance on specific outcome variables, as identified by *S*, and a higher tax price gives voters a stronger incentive to monitor school officials. However, income, tax price, and aid are three of the key variables in any model of the demand for educational performance, and studies have used them as instruments in a simultaneous-equations estimation of equation (1). Our analysis shows that this procedure is not appropriate. Because these variables influence school district spending through their impact on efficiency, they are highly correlated with the dependent variable and cannot be used as instruments. Given this insight, the challenge is to find instruments associated with the demand for school district performance that are not also determinants of school district efficiency.

To be more specific, we select instruments for the performance variables using the following three well-known rules: (1) they make conceptual sense as determinants of the demand for school performance, (2) they help to explain school performance holding other things constant, and (3) they are not statistically significant when included as exogenous variables in the cost equation directly. The third rule is the one that has been ignored in many previous studies. Our list of instruments is presented below. Potential instruments that fail the third rule and that make sense as efficiency variables are included in the final cost regressions.

A third challenge is that even with appropriate instruments for student performance and teacher salaries, equation (3) could yield biased estimates because consolidation, along with its impact on enrollment, may be endogenous. As noted earlier, unobserved factors that lead to consolidation might also influence spending, resulting in a selection bias. Our panel data allow us to solve this problem by estimating a fixed effect and a time trend for each district. The potential endogeneity of consolidation arises because unobserved district-specific factors may influence spending or the trend in spending. District-specific fixed effects and time trends capture the impact of these factors and therefore eliminate these problems.

In symbols, let the subscripts i and t stand for district and year, respectively, and, as above, let C be a dummy variable indicating that a district is part of a consolidated district. Moreover, let α be a district-specific fixed effect β be a district-specific time trend. The cost impacts of consolidation that are not associated with enrollment can be captured by α^* and β^*, which are coefficients for district-specific fixed effects and time trends that apply only after consolidation. In symbols, therefore, our estimating equation is

$$E_{it} = E\left[\alpha_i, \beta_i(t), \alpha_i^*(C), \beta_i^*(C)(t), S_{it}, P_{it}, N_{it}, M_{it}, Z_{it}\right] \tag{4}$$

Following most of the studies in the literature, we specify this equation in log-linear form.

The introduction of district-specific variables leads to a fourth methodological challenge. To preserve all our pre-consolidation information, we retain each district as a separate observation even after it consolidates. However, once a district has consolidated, we assign it the characteristics of the combined district as a whole. This approach allows us to separate the cost effects of consolidation associated with economies of size from other possible effects. This approach also 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.[13] These two steps are also applied to the district-specific time trend. One implication of this approach is that we cannot estimate separate post-consolidation fixed-effects and time trends for two districts that consolidate.

However, we can estimate these effects for each pair and still obtain the impact of consolidation on the fixed effect and time trend of the average consolidating district.[14]

A fifth methodological challenge is that the state aid term is likely to be endogenous. This endogeneity has two sources. First, at least one important aid program, building aid, uses a matching formula, so that the amount of spending and the amount of aid are simultaneously determined. Because we do not know the matching rate for this formula, and because we cannot identify other types of matching aid, we must treat the aid variable as endogenous.[15] In addition, building aid in New York is project-based, which means that a district must submit a capital projects to the state for approval and funding. Aid will be endogenous if post-consolidation capital spending plans result in post-consolidation increases in state building aid. To deal with these issues, we treat aid as endogenous. As explained below, we identify a list of potential instruments using a model of the state aid determination process and then select the final set using the rules given earlier for the performance instruments.

For our purposes, *N* is the key variable in this equation because it picks up the impact of consolidation.[16] 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. 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. With the district-specific fixed effects and time trends in the equation, the coefficient of *N* is identified by the changes in enrollment that accompany consolidation and by nonlinear changes in enrollment in all districts.

A final methodological challenge is that capital spending is lumpy, so that capital spending in a given year is not a good indication of a district's long-term expected annual capital spending. As a result, the dependent variable in our cost regression for capital spending is a nine-year average of capital spending for each district. We can calculate this dependent variable for every year in our panel, because our data on spending, unlike our data on the 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.[17]

The use of nine-year average capital spending necessitates two other changes in our cost model for capital spending. First, 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 nine-year average state aid as our (endogenous) explanatory variable.

Second, the use of a nine-year average requires a change in the district-specific fixed effects and time trends. Recall that these variables must be adjusted after consolidation to account for the fact that post-consolidation spending reflects the unobserved contributions of two districts, not just one. This adjustment is more complicated with capital spending because we must also account for the fact that in consolidating districts, a nine-year average spending variable usually includes some years before consolidation (when the unobserved contributions of each district receive full weight) and some years after consolidation (when the unobserved contributions of each district must be downweighted).[18]

One striking feature of capital spending in our data set is that it often exhibits a large jump after consolidation, usually somewhere between two and eight years after the consolidation took place. In fact, virtually every consolidated district has one or more such "spikes" in capital spending, accompanied, incidentally, by spikes in state aid. On the surface, therefore, it appears as if consolidation results in a large burst of capital spending. However, capital spending takes the form of a spike in non-consolidating districts, too. The real question is whether consolidation alters long-run capital spending, that is, whether it results in higher spikes or in more frequent spikes than would have occurred without consolidation. Our model is ideally suited to answering this question.

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. All of these consolidations involved rural school districts, and approximately 190 other rural districts serve as a control group.[19] The basic data sources are from the New York State Education Department and the New York State Comptroller.[20]

Measures of student performance are required to ensure that we measure the impact of consolidation on costs, holding student performance constant. Previous research on New York has identified three outcome 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, 2000). The first two measures capture the lower tail of the distribution, while the third is a measure of higher levels of performance. 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, we treat the outcome measures as endogenous but cannot use three key demand determinants, namely income, property value, and state aid, as instruments. To solve this problem, we use information on neighboring school districts. We hypothesize that voters' desired level of student performance increases with the performance in adjacent districts. This hypothesis leads to a potential list of instruments for the performance variables that consists of minimum, maximum, and mean values of our three performance variables in adjacent districts.

In addition, we need instruments to identify the coefficient for the endogenous state aid variable. We hypothesize that state legislators are wary of large differences in aid per pupil between similar districts, because of the implications both 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; and the interaction between the previous two variables.

In both cases, 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 for both performance and state aid variables also includes the average, maximum, and minimum value of income, property value, and percent of students receiving a subsidized lunch in adjacent school districts. These initial lists are pared down using the second and third rules outlined earlier. No variable was treated as an instrument unless it was significant (t-statistic greater than 1.5) in at least one of three regressions to explain the three performance measures or a regression to explain state aid. Any variable that passed this test was still not treated as an instrument if it was significant (t-statistic > 1.5) in any of the cost regressions discussed below. This procedure was applied separately to the capital cost regression, so that instruments (and added explanatory variables) in that regression differ from those in the operating cost regressions.

To capture efficiency in the cost model, we start with income, tax price, state aid per pupil, and our variable indicating a change in superintendent. Income is measured by income per pupil, (the inverse of) tax price is measured by property value per pupil, and the state aid variable is total state aid divided by total income, which we call the state aid ratio.[21] In addition, some of the rejected instruments for the performance variables or for state aid were treated as efficiency variables, following the rules given earlier.

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. Teacher's salaries may reflect both the required market wage and the ability of teacher's unions to negotiate higher salaries. Thus, teacher's salaries may also be endogenous. Because comparable private sector wages are not available, we used the minimum, maximum, and mean wage in adjacent counties as instruments. Research on public labor markets has found significant spillovers across adjacent governments, particularly when active unions are present, as in New York (Freeman 1986).

Environmental variables identified in past research include child poverty, incidence of single-parent families, 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 their effect follows a linear trend.

Evaluation Results

Descriptive Analysis. As explained earlier, Tables 2 and 3 compare consolidating and non-consolidating districts. These tables reveal some significant shifts in this comparison between 1985 and 1997. According to Table 2, all categories of aggregate spending per pupil were significantly lower in consolidating districts in 1985 and higher, usually significantly, 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. State aid favored the consolidating districts in 1997 as in 1985, although the difference in 1997 was somewhat larger.[22] Finally, differences in teacher salaries between these two groups of schools also narrowed over this period, which suggests that consolidation does not have a strong wage effect in either direction.

As discussed previously, consolidating districts in 1985 had lower property wealth, a lower ratio of students to administrators, and smaller percent of students going on to college. Table 3 shows that these differences remained in 1997 after consolidation. As expected, however, consolidation also had a significant impact on the size and number of schools in consolidating districts. In consolidating districts, the average number of schools almost doubled and median high-school enrollment increased by over 25 percent. 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 or retention. 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 for consolidating districts on the math PEP tests. 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.

Another way to examine the data is by comparing consolidating districts before and after consolidation. Table 4 shows that, almost across the board, inflation-adjusted expenditure per pupil, revenue per pupil, and average teacher salaries are higher after consolidation. The only exception is expenditure for central administration. However, a similar pattern emerges in non-consolidating districts. One explanation may be 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). Table 4 also shows that capital expenditure and operating and maintenance expenditure rose more rapidly after consolidation than before, but spending for teaching and central administration grew more slowly or even declined. On the revenue side, the large increase in state aid compensated consolidating districts for their lack of growth in local revenue.

Cost Regression Results. The cost models were estimated using 2SLS regression, 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 and employ the same instruments as the operating spending regression.[23]

In interpreting these results, it is important to remember that the regressions include district-specific fixed effects and time trends. Except in the case of the enrollment variables, the estimated coefficients are identified only by nonlinear variation in the explanatory variables and do not provide general tests of the impact of these variables on educational costs. In the operating cost regression, which is in the first column of Table 5, the coefficients of one outcome variable, the PEP test variable, has the expected sign and is statistically significant, whereas the coefficients of other two outcome variables have unexpected signs and one of these coefficient, for the dropout rate, is statistically significant. The coefficients of two cost variables, teacher salaries and the share of students in secondary school, also have the wrong sign and are statistically significant. However, the coefficients of the dropout rate and of the two cost variables are small in magnitude. The core efficiency variables, state aid, property values, and median income, have the expected positive impact on costs and are statistically significant. A change in superintendent also boosts costs, but falls just short of significance at the 5 percent level.

Many of the variables considered as possible instruments also prove to be significant in this regression (and so were rejected as instruments). These variables all 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. Specifically, we find that school operating costs are significantly related to the aid received by various comparison districts (neighbors and those with similar enrollment); to the average performance of neighboring districts; to the average incomes, property values, and teacher salaries in neighboring districts; and to the average share of students with free lunches in neighboring districts. For example, the results for two performance variables, the dropout rate and the percent with a Regents diploma, support the view that the existence of high-performing neighbors puts pressure on school administrators to improve school efficiency.

In the case of capital costs, far fewer variables are statistically significant. See the second column of Table 5. Not surprisingly, the average state aid variable is highly significant with a large coefficient. The coefficient of one of the other basic efficiency variables, district income, also is positive and significant. In addition, three comparison variables have significant coefficients: aid received by districts with similar enrollments, aid received by other districts in the county, and average income of neighboring districts.

Estimated Economies of Size. 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 administrative services. In every other case except one, the estimated relationship between per-pupil spending and enrollment is U-shaped and both enrollment variables are statistically significant. The sole exception is transportation spending, for which the second enrollment variable has a t-statistic of only 1.44.

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 4,699 pupils. Because this minimum point 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 relatively small districts merge. As shown in Table 6, operating cost per pupil declines by 22.4 percent when two 300-pupil districts merge, but the cost savings drop to 8.0 percent when two 1,500-pupil districts merge.

Results for the functional spending categories strongly 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 3,112 pupils and 3,387 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 18.0 percent for instruction and 22.5 percent for teaching. These results clearly 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 noted earlier, the squared enrollment term in this case was dropped, but the enrollment variable is highly significant. Thus, 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 over one third—a sign of extensive "publicness" in administrative services.

The results for transportation services contradict the traditional view, because they also exhibit a U-shape, with a minimum per-pupil cost at an enrollment of 11,417 pupils.[24] See panel E of Figure 1. The cost savings from consolidation can be quite large. As shown in Table 6, these savings range from 32.3 to 18.1 percent for the three hypothetical consolidations in Table 6. Thus, we find clear evidence of economies of size—not diseconomies of size—in the provision of transportation services.

The results for capital spending also indicate a U-shaped pattern, but in this case the implied minimum-cost enrollment is at only 751 pupils. See Table 6 and panel B of Figure 1. Moreover, the per-pupil cost increases rapidly after this point so that it is actually 43.7 percent higher at 3,000 pupils than at 300 pupils. In other words, we find strong economies of size up to 751 pupils and strong diseconomies of scale above that. As a result, consolidations that involve two relatively small districts, such as the one in column 4 of Table 6, result in much lower capital costs per pupil, whereas consolidations that involve larger districts actually raise these costs considerably. See columns 5 and 6 of Table 6.

Because capital costs constitute approximately 9 percent of spending in the average district, these results imply that the enrollment changes associated with consolidation result in cost savings of 22.5, 9.1, and 1.5 percent, respectively, for the three consolidations in Table 6.[25] In other words, net economies of size are very significant when two 300-pupil districts merge, but quite modest when two 1,500-pupil districts come together.

Estimated Cost Impacts of Consolidation. The net cost impact of consolidation reflects both economies of size and cost impacts that are not associated with enrollment. The latter effects are picked up by the post-consolidation changes in the district-specific fixed effects and time trends for each consolidating district. The mean values of these coefficients (along with associated t-statistics) are presented in Table 7.[26] Both operating spending and the functional spending subcategories all 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. Moreover, the gradual decline more than offsets the initial upward shift somewhere between the fourth and seventh year after consolidation, depending on the category. Indeed, for instruction, administration, and transportation, the cost savings beyond those associated with enrollment reach 19 to 34 percent, again depending on the category, by the tenth year after consolidation. In other words, these results clearly indicate that there are short-run adjustment costs associated with consolidation, but that these adjustment costs phase out over time and, indeed are replaced by cost savings from consolidation that are not associated with enrollment change. See panels A and C through E of Figure 1.

These cost savings, which are unrelated to enrollment, are difficult to interpret. One possibility is that the cost savings in, roughly, the fifth through tenth years after consolidation arise because teachers and administrators are so enthusiastic about the possibilities of their new, larger district that they are unusually efficient during these years. Another possibility is that these savings reflect some unidentified feature of consolidation that results in long-run cost savings. Because we do not observe any districts more than ten years after consolidation, we cannot determine statistically which of these possibilities is at work.[27] However, we do not know of any conceptual basis for expecting long-run cost savings from consolidation that are not associated with an enrollment change or with some change in other observable school district characteristics. To put it another way, 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.

The time pattern of the results for capital spending is very different, but the net effect is the same. As shown in Table 7, consolidation results in a large downward shift in capital costs followed by an increase in capital costs over time. Neither of the two estimated coefficients is statistically significant. Taken at face value, these coefficients imply that factors boosting capital costs offset the initial downward shift by the sixth year and result in increased costs in the sixth through tenth year after consolidation. We interpret these findings to mean that districts postpone capital projects in the years immediately after consolidation, but then make up for this with a burst of capital spending thereafter. Presumably, it takes a consolidated districts a few years to figure out exactly how to make use of its merged capital facilities, but then it has to catch up to its long-term capital needs.[28] As in the case of operating costs, we know of no conceptual basis for a long-term increase in capital costs that is not associated with enrollment or some other observable district characteristic, but we cannot rule this possibility out with our data.

These results help us to interpret the widespread appearance of "spikes" in capital spending after consolidation. To some degree, these spikes represent capital spending that would have occurred without consolidation and, in the case of consolidations involving relatively large districts, with capital spending needed to satisfy the higher long-run capital needs associated with a larger student body. However, these spikes also appear to be magnified by the fact that districts postpone capital spending in the years immediately following consolidation and therefore must do some "catch-up" spending when their new capital plans are implemented.

Table 7 presents calculations intended to describe the range of possible interpretations of these results. Specifically, they indicate cost savings from consolidation that are not related to enrollment change as a percentage of the present value of the costs a district would have experienced if it had not consolidated. These calculations are based on the estimated coefficients at the top of the table; in the case of capital spending, therefore, they are imprecise. The first three rows after the regression results are based on a 10-year time trend, with a different discount rate for each row. The next two rows are based on a 30-year horizon and a 5 percent discount rate. The first of these rows assumes that the observed 10-year effects phase out after that time (at the rate indicated by the time-trend coefficient), whereas the second row assumes that the effect observed at eight years continues indefinitely.[29] As a result, the 10-year rows and the first 30-year row correspond to interpreting the results as short-term effects, and the second 30-year row corresponds to interpreting the results as long-term effects.

This table reveals that these post-consolidation, time-trend effects can be substantial for subcategories of spending, but also that these effects are generally small and have little effect on total costs, regardless of our assumptions about time horizon or discount rate. The most dramatic results are in the "capital" and "administration" columns. The time trend effects for capital spending range from –4.3 percent to +12.2 percent, depending on the assumptions. Because we estimate a large increase in capital costs by eight years after consolidation, holding this increase constant until the 30-year mark, results in a significant capital cost increase over the entire period. In contrast, with a 10-year horizon and a high discount rate, which down-weights the capital cost increases in later years, the capital cost savings can be substantial. In the case of administrative costs, we find significant cost savings, between 5 and 18 percent, for all our assumptions. In other subcategories, the cost savings in some years are roughly offset by cost increases in other years, no matter what the assumptions.

The first column of Table 7 indicates that under all assumptions, the time-trend effects for subcategories of spending roughly cancel out so that the impact on total costs is close to zero. As before, the impact on total spending is calculated as a weighted average of the operating and capital cost impacts. As it turns out, the assumptions that drive up the cost increases in capital services (a longer horizon or a lower discount rate) also drive up the cost savings in operating services, so the net impact of these time trends on total costs is close to zero under all our assumptions. Specifically, the net impact ranges from a 3.3 percent cost increase with a 10-year horizon and a 10 percent discount rate to 0.3 percent cost savings with a 30-year horizon and the assumption that costs differences observed in the eighth year continue indefinitely. Regardless of whether the post-consolidation time-trends we estimate phase out after ten years or persist indefinitely, therefore, the long-term impacts of consolidation are closely approximated by the enrollment effects alone.

Conclusions

This paper goes beyond existing education cost studies by examining the cost implications of actual consolidations among rural school districts in New York. Our data cover the 1985 to 1997 period, during which 12 pairs of rural districts consolidated. All other rural school districts serve as our comparison group. Our model is designed to determine the impact of consolidation on costs, holding constant student performance and other factors, such as state and teacher salaries. To eliminate potential biases from endogeneity and unobserved factors, we estimate our model with district-specific fixed effects and time trends and treat student performance, state aid, and teacher salaries as endogenous.

We find that consolidation clearly cuts costs for small, rural school districts in New York. Moreover, the cost savings from consolidation appear to be driven almost entirely by economies of size. Consolidation does affect the time pattern of both operating and capital spending, but in both cases, the initial impact is offset by later changes. Moreover, the time-related impacts on capital spending are roughly offset by the impacts on operating spending. We conclude that consolidation is likely to cut the costs of two 300-pupil districts by over 20 percent, cut the costs of two 900-pupil districts by 7 to 9 percent, and have little if any net impact on the costs of two 1,500 pupil districts.

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 state incentives for consolidation clearly are warranted, but only for relatively small districts. We find no support for the use of state tax dollars to encourage consolidation among districts with 1,500 or more pupils. Overall, our results point toward a state program to encourage consolidation among small, rural school districts, but to eliminate other financial incentives for consolidation.

The consolidation of school districts remains an important issue in state educational policy. This paper shows how the cost impacts of consolidation can be evaluated and shows that consolidation can significantly lower the costs of small, rural school districts. This work obviously needs to be replicated in other states. Moreover, future studies need to consider the impact of consolidation on students' commuting times and on measures of student performance other than test-scores and dropout rates.

District-Specific Variables in a Model of Consolidation

This technical appendix derives district-specific variables to use in a study of school district consolidation.

1. Definitions

Let superscripts define variables and subscripts define observations. Now define the following variables.

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*

= 0 otherwise

w = district weight

= district's share of total enrollment in its consolidated district in the year **before** consolidation

= 0 for districts that do not consolidate

t = time (1985=1)

= value of *t* in the year before consolidation

= 0 in districts that do not consolidate

N = number of districts

M = number of districts that consolidate

Note that if district *i* consolidates;

$$w^i + w^{i^*} = 1$$

That is, enrollment shares for two districts that consolidate add up to 1. Thus,

$$\sum_{i=1}^{n} (w_i + w_{i*}) = M$$

Note that w_i is defined by **district**, not by **observation**; that is, it does not vary with *t*

2. District-Specific Fixed Effects and Time Trends

Before consolidation, a district's fixed effect is just its dummy variable, but after consolidation the dependent variable is the shared spending level. Thus, the unobserved factors for district *i* explain only a portion of the unobserved part of E_i. We set this share at w_i. Moreover, the unobserved factors for district *i* also explain a portion, again w_i, of the unobserved part of $E_{i*} (= E_i)$, which is spending in district *i*'s partner.

Hence,

$$F1^i = \text{district fixed effect}$$

$$= D^i (1 - C) + w_i C (D^i + D^{i*}); \qquad i = 1, N$$

For example, consider three districts over six years. Districts 1 and 2 consolidate in year 4. Enrollment for District 1 is 33 percent of the combined enrollment of Districts 1 and 2 in year 3. The values of the dummy variables for these three districts are as follows:

District	Year	$F1^1$	$F1^2$	$F1^3$
1	1	1	0	0
1	2	1	0	0
1	3	1	0	0
1	4	.333	.607	0
1	5	.333	.607	0
1	6	.333	.607	0
2	1	0	1	0
2	2	0	1	0
2	3	0	1	0
2	4	.333	.607	0
2	5	.333	.607	0
2	6	.333	.607	0
3	1	0	0	1
3	2	0	0	1
3	3	0	0	1
3	4	0	0	1
3	5	0	0	1
3	6	0	0	1

Also,

$T1^i$ = district time trend

= $(F1^i)(t); \quad i = 1, N.$

3. Post-Consolidation Fixed Effects and Time Trends

After consolidation, only shared effects are observed and both consolidation partners have the same dependent variable. As a result, separate affects for the two partners cannot be estimated; instead, we estimate a fixed effect and trend for each **pair**.

In symbols:

j = j^{th} consolidating pair

j_1 = value of *i* for 1st district in pair *j*

j_2 = value of *i* for 2nd district in pair *j*.

Now define

$$F2 = C(D^{j_1} + D^{j_2}); \qquad j = 1, M/2$$

and

$$T2^j = C(D^{j_1} + D^{j_2})(t - t^*); \qquad j = 1, M/2.$$

With these definitions, our regression (with observation subscripts suppressed) can be written

$$E = bX + \sum_{i=1}^{N} \alpha_i F1^i + \sum_{i=1}^{N} \beta_i T1^i + \sum_{j=1}^{M/2} \gamma^j F2^j + \sum_{j=1}^{M/2} \delta^j T2^j .$$

We want estimates and standard errors for two means:

$$\bar{\gamma} = \frac{\sum_{j=1}^{M/2} \gamma^j}{M/2} = \frac{2\sum_{j=1}^{M/2} \gamma^j}{M}$$

$$\bar{\delta} = \frac{\sum_{j=1}^{M/2} \delta^j}{M/2} = \frac{2\sum_{j=1}^{M/2} \delta^j}{M}$$

Because γ^j and δ^j are estimates of the average effect for districts j_1 and j_2, these formulas indicate that $\bar{\gamma}$ and $\bar{\delta}$ are averages both across pairs and across all consolidating districts.

Note also that these averages are not weighted. We want the spending shift per pupil in the average consolidating district. The *w* variable is irrelevant for this purpose.

Now let

$$F2^* = \sum_{j=1}^{M/2} F2^j$$

$$= \sum_{j=1}^{M/2} C(D^{j_1} + D^{j_2})$$

$$= C\sum_{j=1}^{M/2} (D^{j_1} + D^{j_2})$$

$$= C,$$

since $D^{jk} = 1$ requires $C = 1$, and $C = 1$ requires one and only one $D^{jk} = 1$.

Similarly,

$$T2^* = \sum_{j=1}^{M/2} T2^j = \sum_{j=2}^{M/2} C(D^{j_1} + D^{j_2})(t - t^*)$$

$$= C(t - t^*) \ .$$

Now, re-write the regression as follows,

$$E = bx + \sum_{i=1}^{N} \alpha^i F1^i + \sum_{i=1}^{N} \beta^i T1^i + \gamma^* F^* + \sum_{j=2}^{M/2} \gamma^{*j} F2^j + \delta^* T^* + \sum_{j=2}^{M/2} \delta^{*j} T2^j$$

In this set up, it is obvious that

$$\begin{aligned} \gamma^* &= \gamma^1; & \delta^* &= \delta^1; \\ \gamma^* + \gamma^{*2} &= \gamma^2; & \delta^* + \delta^{*2} &= \delta^2; \\ \gamma^* + \gamma^{*M/2} &= \gamma^{M/2}; & \delta^* + \delta^{*M/2} &= \delta^{M/2}. \end{aligned}$$

So,

$$\bar{\gamma} = \frac{\gamma^* + \sum_{j=2}^{M/2} \left(\gamma^* + \gamma^{*j} \right)}{M/2}$$

$$= \frac{(M/2)\gamma^* + \sum_{j=2}^{M/2} \gamma^{*j}}{M/2} = \gamma^* + \frac{2\sum_{j=2}^{M/2} \gamma^{*j}}{M}.$$

Similarly,

$$\bar{\delta} = \delta^* + \frac{2\sum_{j=2}^{M/2} \delta^{*j}}{M}.$$

Hence, we need to add the second terms of these expressions to γ^* and δ^*, respectively, as written to obtain the relevant means. Adding and subtracting these terms yields (for the last four terms),

$$\left(\gamma^* + \frac{2\sum_{j=2}^{M/2}\gamma^{*j}}{M}\right)F2^* + \underbrace{\sum_{j=2}^{M/2}\gamma^{*j}F2^j - \left(\frac{2\sum_{j=2}^{M/2}\gamma^{*j}}{M}\right)F2^*}_{A}$$

$$+\left(\delta^* + \frac{2\sum_{j=2}^{M/2}\delta^{*j}}{M}\right)T2^* + \underbrace{\sum_{j=2}^{M/2}\delta^{*j}T2^j - \left(\frac{2\sum_{j=2}^{M/2}\delta^{*j}}{M}\right)T2^*}_{B}.$$

Now recall that $F2^* = C$ and $T2^* = C(t-t^*)$. It follows that

$$A = \sum_{j=2}^{M/2}\gamma^{*j}F2^j - \frac{2\sum_{j=2}^{M/2}\gamma^{*j}C}{M}$$

$$= \sum_{j=2}^{M/2}\gamma^{*j}\left[F2^j - \frac{2C}{M}\right]$$

and

$$B = \sum_{j=2}^{M/2}\delta^{*j}T2^j - \frac{2\sum_{j=2}^{M/2}\delta^{*j}C(t-t^*)}{M}$$

$$= \sum_{j=2}^{M/2}\delta^{*j}\left[T2^j - \frac{2C(t-t^*)}{M}\right].$$

In sum, to estimate the desired average effects:

1. Drop one post-consolidation district effect and time trend.

2. Replace them with

$$F2^* = C$$
$$T2^* = C(t-t^*)$$

3. Redefine the district effects and trends as follows. The second term in each expression is "on" for every observation with $C_{it} = 1$.

$$F2^{*j} = F2^{j} - \frac{2C}{M}$$

$$T2^{*j} = T2^{j} - \frac{2C(t-t^{*})}{M}$$

After these steps, the coefficients of $F2^{*}$ and $T2^{*}$ are average effects, and their standard errors are the appropriate standard errors for the average effects.

5. District-Specific Variables for Capital Spending

For capital spending,

$$E_{it} = \sum_{t'=t-8}^{t} [E_{it'}] \frac{1}{(1+d)^{t-t'}},$$

where *d* is an assumed depreciation rate for school capital.

Thus, the dependent variable in consolidating districts often blends information from before and after consolidation. For example, the dependent variable in the first year after consolidation is influenced by nine pre-consolidation years. Five years after consolidation the average includes five years before and four years after consolidation.

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, the district fixed effects are

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

where *d* is the same depreciation rate used to define average spending.

In addition, the district time trends are

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

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

$$F2^j = \frac{1}{9}\sum_{t'=t-8}^{t}\left[C_{it'}(D^{j_1}+D^{j_2})\right]\frac{1}{(1+d)^{t-t'}}$$

$$T2^j = \frac{1}{9}\sum_{t'=t-8}^{t}\left[C_{it'}(D^{j_1}+D^{j_2})\right]\frac{t'-t^*}{(1+d)^{t-t'}}.$$

Note that these variables, unlike $F1^i$ and $T1^i$, have many zero entries in the summations, corresponding to years before consolidation with $C_{it'}=0$. Not surprisingly, this formulation increases the complexity of the specification needed to obtain average effects.

To estimate the average effects, define

$$F2^* = \sum_{j=1}^{M/2} F2^j$$

$$= \sum_{j=2}^{M/2}\frac{1}{9}\sum_{t'=t-8}^{t}\left[C_{it'}(D^{j_1}+D^{j_2})\right]\frac{1}{(1+d)^{t-t'}}$$

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

since, as before, the *D*'s add no additional information. Now suppose $d=0$. Then for any observation, the value of $F2^*$ equals 1/9 the number of years in the nine-year period ending in *t* [the year for that observation] during which district *i* [the value of *i* for that observation] was consolidated.

Similarly,

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

Following the formulation on pages 4-7, with these new expressions for $F2^*$, $F2^j$, $T2^*$, and $T2^j$, we find that,

$$A = \sum_{j=2}^{M/2} \gamma^{*j} F2^{j} - \left(\frac{2\sum_{j=2}^{M} \gamma^{*j}}{M} \right) F2^{*}$$

$$= \sum_{j=2}^{M/2} \gamma^{*j} \left[F2^{j} - \frac{2}{9M} \sum_{t'=t-8}^{t} \frac{C_{it'}}{(1+d)^{t-t'}} \right].$$

As before, the second term is "on" for all districts that consolidate, not just for those in pair j, the pair associated with the variable. If $d = 0$, then this expression collapses to $1/(2M)$ times the share of the nine years up to and including t [the year for the observation] during which district i was consolidated.

Turning to the time trends, we can readily see that

$$\beta = \sum_{j=2}^{M/2} \delta^{*j} T2^{j} - \frac{2\sum \delta^{*j}}{M} T2^{*}$$

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

Those results lead to the following procedure for the capital spending regression.

1. Drop one post-consolidation district-pair effect and time trend.

2. Replace them with:

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

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

3. Redefine the district effects and trends:

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

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

With this formulation, the coefficient of $F2*$ is the average post-consolidation shift in the district-fixed effect and $T2*$ is the average post-consolidation shift in the time trend. The estimated standard errors are appropriate for these averages.

Endnotes

*The authors would like to acknowledge the valuable assistance of Richard Glasheen and Ron Danforth at the New York State Education Department with the education data, as well as the research assistance provided by Matthew Andrews. The comments of Dan Black, Thomas Downes, William Fowler, Jan Ondrich, Lori Taylor, and Doug Wolf have also been very helpful.

1. Estimates of the effect of class size below 20 students are hard to come by because classes of that size are not usually observed. Ferguson and Ladd (1996) find that reading performance is about the same in classes between 19 and 25 students, but declines for larger sizes, all else equal. In contrast, they find that performance in mathematics declines continuously as class size increases from 19 to 29.

2. An increase in school district size might also add to the time costs paid by students and parents who have longer travel times to school (Kenny 1982). Our analysis is limited to costs that appear in the school budget.

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. Only a few studies (Downes and Pogue 1994; Duncombe, Ruggiero, and Yinger 1996; Duncombe and Yinger 1997, 2000; Reschovsky and Imazeki 1997, 1999) have treated teacher salaries as endogenous.

5. Two studies have employed stochastic frontier regression methods (Deller and Rudnicki 1992; Duncombe, Miner, and Ruggiero 1995) to take efficiency into account in the estimation of the cost function. They generally did not find large differences between the results of the frontier regression and OLS regression with regard to the enrollment variables. Downes and Pogue (1994) employ panel data methods to control for district specific effects, and they 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. They view the DEA measure as serving a similar role to a fixed-effects model, by capturing the effects of omitted variables including efficiency.

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 the district consolidated. We have school aid data available back to 1981, and information on school consolidations since 1979. A number of districts that were receiving reorganization building aid in 1981, were still receiving this aid in 1997. It appears most of these district 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.

8. Technically, two of the consolidating districts, namely 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.

9. Specifically, this variable was coded "1" if there was a superintendent change in the previous two years. A more general way to deal with this problem is to estimate a first-stage model for the choice to consolidate in each year and then include a selection correction derived from this model in the second-stage cost regression. This approach, which is reviewed in Heckman, LaLonde, and Smith (1999), is not possible in our case because we do not observe the characteristics of individual school districts after they consolidate.

10. As shown by Duncombe and Yinger (1997), with standard assumptions about school production, the coefficients of the performance variables in the cost equation can be given a demand interpretation. Hence, this statistical test can be thought of as a way to determine whether households place a significant value on a given performance variable.

11. 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.

12. Unobserved determinants of school district efficiency are not a source of bias in our regressions because they are captured by the district fixed effects, which are discussed below.

13. Formal definitions are given in the Technical Appendix.

14. See the Technical Appendix.

15. Even if we did know the matching rate, building aid in New York uses a closed-ended matching formula, and we cannot identify the districts that are at the maximum, where the matching rate no longer applies.

16. The size of schools themselves may affect student performance and costs. We calculated measures of the median high school and elementary schools size in the district and included them in the cost equation. Generally, they were not statistically significant, and including them did not materially affect the regression results for enrollment or the consolidation variables. These variables are not included in our final regressions because they may be endogenous.

17 . The 2 percent rate was based on the assumption of a 50-year useful life and linear depreciation. We found little difference in the results with 5 percent depreciation or no depreciation.

18. See the Technical Appendix.

19. 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 control group.

20. 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.

21. Standard theory calls for median income instead of income per pupil and defines tax price as the ratio of median to mean property value. These variables are not available in our data set, but the variables we use are highly correlated with the theoretically preferable ones. Standard theory also indicates that the income term should be median income plus the product of state aid per pupil and tax price. To approximate this additive income term in our multiplicative estimating equation, we divide the aid by income. For more on these specification issues, see Ladd and Yinger (1991) and Duncombe and Yinger (1997, 2000).

22 This larger difference appears to reflect both building aid and reorganization aid given to newly consolidated districts. However, our data do not provide consistent measures of the various state aid subcategories across time, so we cannot directly observe the impact of consolidation on the composition of districts' aid.

23. Regression results for the subcategories are available from the authors upon request. The instruments for all regressions except the one for capital spending include ten variables for adjacent districts: minimum teacher salary, maximum teacher salary, maximum drop-out rate, minimum drop-out rate, minimum PEP score, minimum share of Regents diplomas, maximum property value, and minimum state aid ratio, and maximum share of students receiving subsidized lunch. This list also includes the interaction of aid to districts with similar enrollment and aid to districts in the same county. The instruments in the capital spending regression include all those on the above list, except the interaction interaction of aid variables, along with a variety of variables for adjacent districts: the minimum, maximum, and mean values of aid, the mean values for the three performance variables, teacher salaries, and property values.

24. Our results do not, of course, address the possibility that consolidation raises the time costs of transportation for parents or students.

25. In the average non-consolidating school district, capital spending was 9.2 percent of total spending in 1997. The average for consolidating school districts is higher, of course, because of the burst of capital spending that occurs right after consolidation; this is obviously not a good indication of the long-run average.

26. Our procedure for estimating these coefficients and t-statistics is presented in the Technical Appendix.

27. We estimated our models with a quadratic post-consolidation time trend to determine whether the cost savings turned back toward zero as a district approached ten years after consolidation. The squared time term was not significant for any spending category.

28. A district may also have an incentive to "catch up" relatively fast. In order to receive consolidation-based building aid, districts must have capital projects approved within ten years after consolidation.

29. We use eight years instead of ten because we have very few observations in the ninth or tenth year after consolidation.

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

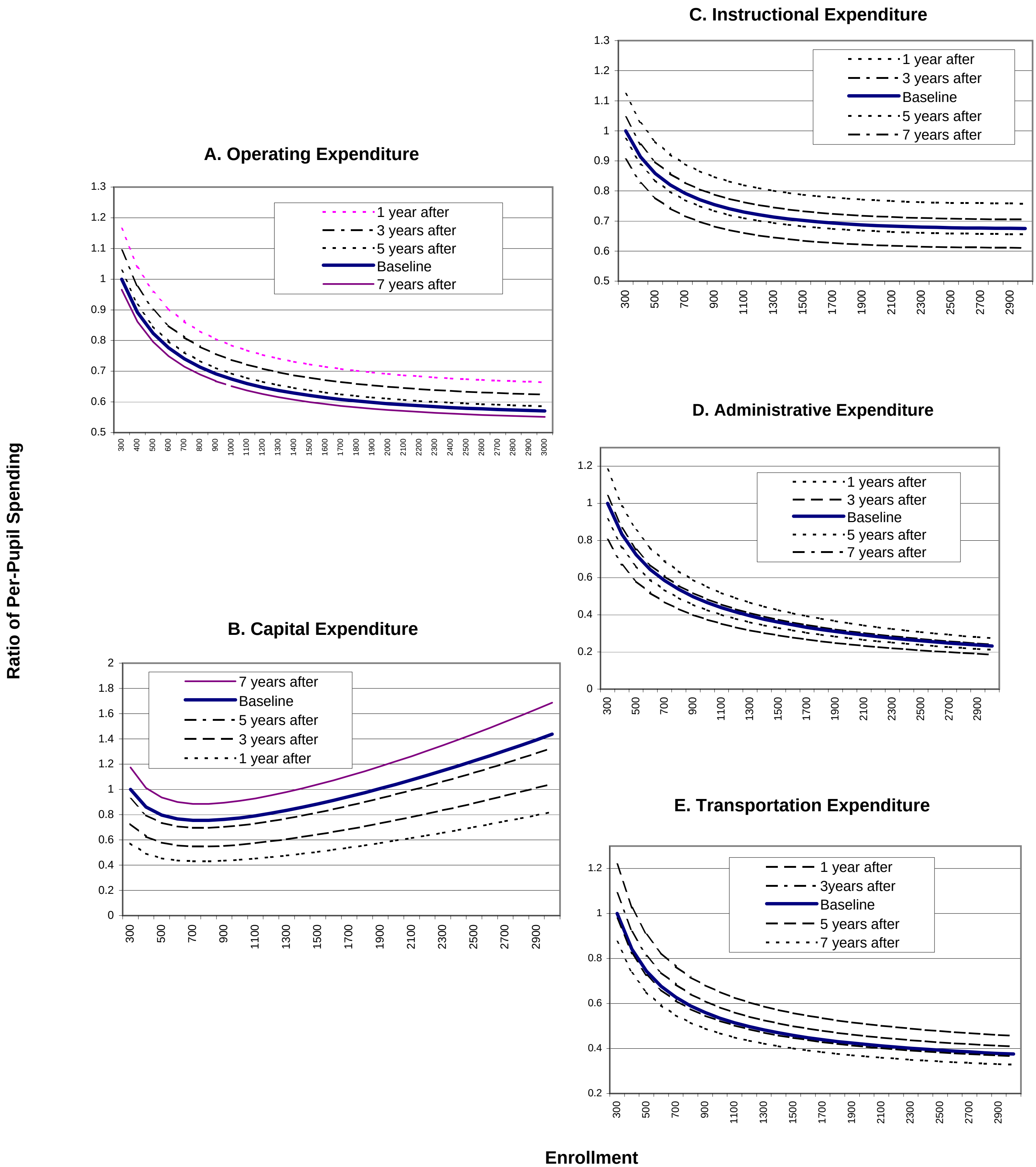

Table 1
New York School Districts Consolidating Between 1987 and 1995

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
Levels and Trends of Per-Pupil Spending and Revenue Levels
For Consolidating and Non-consolidating School Districts in New York in 1985 and 1997[a]

	1985			1997		
Expenditure Category (Inflation-adjusted dollars)[b]	**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 signficance level.
** Means for consolidating and non-consolidating districts are statistically different at 10 percent signficance 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 comparision. Sample size is 2,747.

[b] Adjusted using the fixed weighted GNP price deflator for state and local government purchases published by the U.S. Bureau of Economic Analaysis.

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]

	1985			1997		
District Characteristics	**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 enrollmentc	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 signficance level.

** Means for consolidating and non-consolidating districts are statistically different at 10 percent signficance 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 comparision. 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 Analaysis.

[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
Levels and Trends Comparison of Per-Pupil Spending and Revenue
For Consolidating Districts in New York Before and After Consolidation[a]

Expenditure Categories	Inflation-adjusted Dollars[b] Districts Before Consolidation	Districts After Consolidation		Annual Percent Change (inflation-adjusted)[b] 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 signficance level.

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

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

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

Table 5
Cost Regression Results
For Consolidating and Non-consolidating School Districts in New York, 1985 to 1997 [a]

Variable	Operating Coefficient	Operating t-statistic	Capital Coefficient	Capital t-statistic
Intercept	12.96937	12.40	-7.54643	-1.17
Log of enrollment	**-1.28676**	**-5.16**	**-4.46139**	**-3.83**
Square of log of enrollment	**0.07609**	**4.09**	**0.33690**	**3.80**
Outcomes				
Percent of students below minimum competency on PEP tests (3rd and 6th grades)	-0.00143	-2.22	-0.00580	-1.35
Dropout rate (percent)	0.00436	2.48	-0.01520	-1.28
Percent of graduates receiving Regents Diploma	-0.00095	-1.35	0.00187	0.78
Log of teacher salaries (1-5 years)	-0.04417	-2.19	-0.24487	-1.78
Other cost factors				
Percent secondary students	-0.00131	-2.31	-0.00052	-0.15
Percent receiving subsidized lunch	-0.00010	-0.47	-0.00263	-1.79
Efficiency factors				
Total state aid ratio[b]	0.33468	1.98	23.12392	7.01
Log of property values	0.11669	11.76	0.13058	2.00
Log of average income	0.06914	2.05	2.82352	6.51
Superintendant change in last 2 years (1=yes)	0.00404	1.93	-0.01828	-1.22
Averages of adjacent districts:				
Percent of students below minimum competency on PEP tests (3rd and 6th grades)	-8.951E-04	-6.19		
Dropout rate (percent)	2.491E-03	1.98		
Percent of graduates receiving Regents Diploma	-6.538E-04	-2.04		
Log of teacher salaries	6.060E-06	7.05		
Log of average income	2.200E-06	3.10	-1.550E-05	-2.40
Percent receiving subsidized lunch	-7.247E-04	-2.85	2.391E-03	1.37
Log of property values	-8.400E-05	-3.40		
Total state aid ratio	7.798E-01	9.43		
State aid reference group:				
Districts with similar enrollment	0.47791	5.03	-1.60654	-3.09
Districts in same county	-0.01484	-0.06	4.19554	2.49
Maximum aid ratio of adjacent districts	-0.22063	-5.19		
SSE	3.72		193.53	
Adjusted R2	0.9386		0.8201	
Sample size	2747		2745	

[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.

[b] 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.

Table 6
Coefficients for Enrollment Variables from the Cost Models and Estimates of Economies of Size Effects, New York School Rural Districts, 1985 to 1997

Expenditure Category (Inflation-adjusted dollars)[b]	Regression Coefficients: Enrollment	Regression Coefficients: Enrollment Squared	Minimum Cost Enrollment	Economies of Size Effects[a]: From 300 Pupils to 600 Pupils	Economies of Size Effects[a]: From 900 Pupils to 1800 Pupils	Economies of Size Effects[a]: From 1500 Pupils to 3000 Pupils
Spending by object:						
Operating (all but capital)	-1.287	0.076	4699	-22.4%	-12.9%	-8.0%
(t-statistic)	(-5.161)	(4.085)				
Capital spending	-4.461	0.337	751	-23.4%	27.9%	62.4%
(t-statistic)	(-3.831)	(3.804)				
Spending by function:						
Instructional	-1.154	0.072	3112	-18.0%	-8.5%	-3.7%
(t-statistic)	(-4.015)	(3.341)				
Teaching	-1.440	0.089	3387	-22.5%	-11.3%	-5.6%
(t-statistic)	(-5.023)	(4.136)				
Non-instructional						
Central administration	-0.635		na	-35.6%	-35.6%	-35.6%
(t-statistic)	(-12.837)					
Transportation	-1.598	0.086	11417	-32.3%	-22.9%	-18.1%
(t-statistic)	(-2.013)	(1.442)				

[a]Calculation of percent change is based on enrollment coefficients. Estimated cost change from consolidation is divided by pre-consolidation cost.

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

Table 7
Average District-Specific Consolidation Effects and the Non-Enrollment Cost Effects of Consolidation, New York Rural School Districts[a]

	Total[b]	Operating	Capital	Instructional	Teaching	Administration	Transportation
Regression coefficients							
Average intecept	na	0.183	-0.678	0.152	0.180	0.232	0.253
(t-statistic)		(8.91)	(-1.41)	(6.42)	(7.64)	(4.23)	(3.83)
Average time trend	na	-0.031	0.120	-0.036	-0.029	-0.064	-0.055
(t-statistic)		(-4.67)	(0.54)	(4.67)	(-3.86)	(-3.49)	(-2.58)
Cost savings with a 10-year time horizon							
2% discount rate:	2.2%	2.1%	3.3%	-3.4%	2.7%	-9.0%	-2.9%
5% discount rate:	2.6%	2.8%	0.3%	-2.6%	3.4%	-7.6%	-1.7%
10% discount rate:	3.3%	4.0%	-4.3%	-1.3%	4.5%	-5.5%	0.3%
Cost savings with a 30-year time horizon (5 % discount rate)							
Phase out after 10 years[c]	1.0%	0.7%	4.3%	-3.0%	1.1%	-7.3%	-3.0%
Constant after 8 years	-0.3%	-1.5%	12.2%	-7.8%	-0.7%	-17.8%	-9.9%

[a] The non-enrollment effects are calculated by taking the average intercept coefficient and adding it to the time trend coefficients from the regressions multiplied by the number of years after consolidation. Since dependent variable is in logarithms, this total is the percent change in costs. These are calculated for each year and the present value is calculated for the given number of years and discount rate. This present value is divided by the sum of the discount factors for the same period of time.

[b] Since the regressions for capital and operating are based on different models, the estimate of the total effect is based on a weighted average of the effects for capital and operating separately, with the weights based on the expenditure share for capital (9.2 %) and operating (90.8%) for non-consolidating districts in 1997.

[c] Phased out at the rate given by the time trend coefficient (with the opposite sign).

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