ERRATA

To: Recipients of MG-533-ANF/WPF/SDP, State Takeover, School Restructuring, Private Management, and Student Achievement in Philadelphia, 2007

From: RAND Corporation Publications Department

Date: February 2007

Re: Corrected page (p. 7, Table 2.1, Provider Summary); Corrected page (p. 23); Corrected page (p. 25); Corrected pages (pp. 35–36, Table 4.4, Achievement Effects by Provider); Corrected page (p. 37)

In Table 2.1, the number of schools assigned to Universal Companies, Inc., in 2002–2003 was three. The number assigned to Victory Schools, Inc., in 2005–2006 was six. The number assigned to Foundations, Inc., was six. The number of Sweet 16 schools in 2005–2006 was 15. The errors in Table 2.1 did not affect the achievement analysis in any way. All of the achievement analyses used the correct numbers of schools, despite the errors in Table 2.1.

On page 23, a footnote, now footnote 1, was added.

On page 25, what was formerly footnote 4 was deleted. The text read, "A proxy for poverty was not included because the measure of poverty changed over time in our data."

In Table 4.4, several of the math and reading coefficients and their t-statistics have been revised. The substantive conclusions and implications remain the same.

On page 37, the text of the sentence beginning, "All of the estimates," has been changed to read, "All of the estimates for Temple and Victory Schools in other years are negative, but are often not statistically significant."

We regret the inconvenience.

Table 2.1
Provider Summary

Provider	Provider Type	Extra Per-Pupil Funds, 2002–2003 ($)	Extra Per-Pupil Funds, 2005–2006 ($)	Number of Schools, 2002–2003	Number of Schools, 2005–2006
Edison Schools, Inc.	For profit	881	750	20	22
Victory Schools, Inc.	For profit	857	750	5	6
Chancellor Beacon Academies, Inc.	For profit	650	n/a	5	0
Foundations, Inc.	Nonprofit	667	750	5	6
Universal Companies, Inc.	Nonprofit	656	750	3	3
Temple University	University	450	450	5	4
University of Pennsylvania	University	450	450	3	2
Office of Restructured Schools	District	550	0[a]	21	0
Sweet 16	District	550	450	16	15

NOTE: n/a = not applicable due to the termination of the contract with Chancellor Beacon Academies after the 2002–2003 school year.

[a] Intervention concluded 2004–2005.

The specific method by which schools were assigned to specific providers or to restructuring was not made explicit by the SRC, but the general understanding was that private providers were given the lowest-achieving schools in the district, while restructured schools were slightly higher achieving. This perception is confirmed by data on student achievement, as we describe in the next chapter.

The Diverse Provider Model in Theory and in Practice

The theoretical literature on the diverse provider model in education, for which Paul Hill is the most notable proponent, describes flexible, competitive school marketplaces in which districts manage a varied portfolio of schools, providers have wide rein to innovate, and both are held accountable for student outcomes by strong contracts and through the availability of meaningful choices for students and parents (Hill, 2002, 2006). It diagnoses urban school failure as the result of a lack of sound management practices by district and school leaders, union contracts that impose narrow work restrictions, and a rigid professional bureaucracy that eschews innovative practices. As an alternative, Hill proposes a diverse provider model that has the following key features (Hill, 2002, 2006):

- Districts build portfolios of schools, which may include charter schools, district-run schools, and schools managed by external providers with which the district contracts.

CHAPTER FOUR

Diverse Providers and Student Achievement

We now turn our attention to the diverse provider model. Specifically, we examine how the change in management affected the achievement of students in the privately managed, restructured, and sweet 16 schools, as well as any differences in achievement among different privately managed schools. This analysis relies on longitudinal student-level achievement data for students in grades 2–8 within SDP (comparable data are not available for the rest of the state).[1]

Several studies have attempted to gauge the achievement effects of the private management of schools in other locations around the country. A 2003 U.S. General Accounting Office report examined student achievement in schools under private management in six major urban districts, finding mixed results (GAO, 2003). Other studies have focused specifically on Edison schools, because Edison has been the largest private manager of public schools across the country as well as in Philadelphia. Again, findings have been mixed (see Gomez and Shay, 2000; Miron and Applegate, 2000; AFT, 1998; Nelson, 2000; Nelson and Van Meter, 2003; Hoxby, 2003; Dryden, 2004; Gill et al., 2005). The largest and most ambitious evaluation of Edison, published by RAND (Gill et al., 2005), found that its schools did not, on average, exceed the achievement gains of comparison schools in the first three years of Edison operation, but that a majority of Edison schools matched or exceeded the gains of comparison schools after four to five years of operation.

One previous study has used longitudinal student-level data to examine achievement differences among schools with different managers in Philadelphia. MacIver and MacIver (2006) examined achievement gains from fifth grade to eighth grade in middle schools and K–8 schools in Philadelphia by eighth-graders in 2003, 2004, and 2005. They divided the external providers into two groups: Edison and all others. They found no statistically significant differences between the achievement gains of Edison middle-school students and district middle-school students in any of the three cohorts, but district middle-school students outgained students in middle schools operated by other external providers in all three cohorts. Results for K–8 schools were mixed, depending on provider types (and they typically had only small numbers of students in relevant comparisons), but likewise suggested that, in most instances, students in district-managed K–8 schools were gaining at least as much from grade five to grade eight as were students in the externally managed schools. Although this analysis provides useful information about the current effectiveness of schools operating under different treat-

[1] High school grades were excluded.

The measure of achievement, A_{jt}, is not an absolute measure, but rather a student's normalized rank among all students taking the same test in that subject and grade in Philadelphia. In the period under examination (2000–2001 through 2005–2006), students in Philadelphia took three different kinds of annual achievement tests in reading and math, varying with the school year and grade. Consequently, we have no consistent scale on which to gauge absolute changes in student achievement over time. We therefore converted all scaled test-score results into rank-based z-scores, by year and grade, with a mean of zero and a standard deviation of one. Specifically, we sorted all student scores by rank and converted them to z-scores normed across the entire districtwide population of tested students in that subject and grade. This conversion does not require that students have the same rank on one test as on another, but it assumes that differences in the distribution of students on different tests are not correlated with the treatment of interest.[2] Random differences in student ranks across different tests would introduce noise, but not bias, to the analysis.

We cannot make claims about the absolute amount of learning in one school or another—there is no common, developmentally valid scale across the three tests used in the district over the period of interest. Converting the scores to a common scale is essential to examine changes in the achievement of individual students over time. Using a method that involves the standardized scaling of student ranks across the district permits an assessment of achievement effects with fewer assumptions than would be needed under other kinds of scaling.[3]

The second set of analyses also uses Equation 4.1, but subdivides privately managed schools into those managed by for-profit firms, universities, and other nonprofit organizations to assess whether different types of operators produce differential achievement effects. We further examine the effects of each individual provider separately—though caution is warranted when interpreting results for individual providers, for reasons discussed later.

A third set of analyses expands Equation 4.1 to include an interaction between demographic categories and attendance in diverse provider schools to examine whether the performance of different managers varies for different kinds of students. Demographic categories examined include race/ethnicity, gender, special education (or individualized education program [IEP]) status, and limited English proficiency (LEP).

Our analysis examines trends over time for the first four years of the operation of the diverse provider model. Existing research on educational management organizations (including RAND's analysis of Edison schools in Gill et al., 2005), charter schools, and comprehensive reforms has suggested that student achievement effects may become evident only after several years following implementation (Slavin et al., 1994; Ross, Nunnery, and Smith, 1996; Catterall, 1995; Sass, 2006; Booker et al., forthcoming; Bifulco and Ladd, 2006; Hanushek et al., 2005; Zhang, Shkolnik, and Fashola, 2005). Consequently, it is important to examine whether trends in achievement effects are evident over time and to distinguish one-year effects from four-year effects rather than combining them in a crude average.

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[2] The within-student correlations of Terra Nova and PSSA results were 0.71 in reading and 0.67 in math, which is consistent with other research that has combined test scores (see May, Supovitz, and Perda, 2004).

[3] For further discussion of the use of rank-based z-scores, see Gill et al. (2005).

of the analysis. In most years and subjects, we cannot detect effect sizes smaller than 0.1 standard deviations.

For most providers, years, and subjects, we detected no statistically significant effects, positive or negative. As in the previous analysis that averaged across all providers, the estimates of greatest interest are those for the fourth year, which represent cumulative effects after four years following the implementation of each provider's intervention. Only two providers show statistically significant results after four years—both in the negative direction. Effect estimates for Temple University are significantly and substantially negative in the fourth year following

Table 4.4
Achievement Effects by Provider

Year	Math Coefficient (t-statistic)	Reading Coefficient (t-statistic)
Chancellor Beacon Academies[a]		
One-year effect	0.09 (1.01)	0.04 (0.55)
Two-year effect	0.08 (0.99)	0.03 (0.76)
Three-year effect	0.09 (0.78)	0.06* (1.98)
Four-year effect	0.01 (0.05)	–0.05 (–0.51)
Edison Schools		
One-year effect	–0.06 (–1.14)	–0.11* (–2.84)
Two-year effect	0.03 (0.65)	–0.05 (–1.36)
Three-year effect	0.05 (0.83)	–0.05 (–0.86)
Four-year effect	0.05 (0.67)	–0.06 (–0.97)
Foundations Schools		
One-year effect	–0.01 (–0.10)	0.01 (0.13)
Two-year effect	–0.08 (–0.83)	0.01 (0.17)
Three-year effect	–0.03 (–0.20)	0.002 (0.03)
Four-year effect	–0.13 (–0.98)	–0.03 (–0.39)

Table 4.4—Continued

Year	Math Coefficient (t-statistic)	Reading Coefficient (t-statistic)
Temple University		
One-year effect	–0.14* (–2.07)	–0.12 (–1.57)
Two-year effect	–0.15 (–0.45)	–0.11 (–1.03)
Three-year effect	–0.23* (–2.20)	–0.23* (–4.05)
Four-year effect	–0.37* (–5.52)	–0.34* (–4.75)
Universal Companies		
One-year effect	–0.03 (–0.40)	0.001 (0.04)
Two-year effect	–0.02 (–0.34)	–0.02 (–0.81)
Three-year effect	0.03 (1.68)	–0.09* (–3.14)
Four-year effect	–0.18 (–0.56)	–0.04 (–0.14)
University of Pennsylvania		
One-year effect	0.01 (0.10)	–0.004 (–0.06)
Two-year effect	0.05 (0.67)	–0.04 (–0.37)
Three-year effect	0.07 (0.59)	–0.04 (–0.44)
Four-year effect	0.01 (0.05)	–0.04 (–0.23)
Victory Schools		
One-year effect	–0.09 (–0.82)	–0.01 (–0.26)
Two-year effect	–0.12 (–1.04)	–0.04 (–0.55)
Three-year effect	–0.05 (–0.44)	–0.03 (–0.27)
Four-year effect	–0.25* (–2.38)	–0.07 (–0.92)

NOTE: * = statistically significant at the 5-percent level.

[a] Chancellor Beacon Academies managed schools only during the 2002–2003 school year; results for later years are for the former Chancellor Beacon schools.

implementation in both math and reading. The fourth-year effect estimate for Victory in math is likewise significantly and substantially negative. Although these three negative coefficients are larger than the estimates for Temple and Victory in previous years, in both cases they are consistent with generally negative patterns. All of the estimates for Temple and Victory schools in other years are negative, but are often not statistically significant.

Achievement Results for At-Risk Student Subgroups

Finally, we examine whether the schools in various treatments had differential effects by gender and on African-American, Hispanic, LEP, and special education/IEP students. We examine these results only for restructured schools and for the entire group of privately managed schools, because most subgroup sizes become small for individual providers. The results are summarized in the remainder of this chapter.

For privately managed schools, across all providers, none of the results for average subgroup achievement are statistically significant, positive or negative. As shown earlier, private management has had neither a positive nor negative effect on the schools managed (relative to the rest of the district), so it is not surprising that we are unable to detect any differential effect for the different demographic categories for any year.

For sweet 16 schools, we found no statistically significant effects, positive or negative, for most subgroups in most subject-year combinations. Among the effects that achieve statistical significance, we found no consistent patterns suggesting unique advantages or disadvantages for particular subgroups of students.

For restructured schools, several of the estimates for subgroup-subject-year combinations are positive and statistically significant. The only subgroups for which more than one subject-year combination achieves statistical significance are African-Americans (in four of eight subject-year combinations) and boys (in three of eight subject-year combinations). This is not surprising, given that most of the students in the restructured schools are African-American, and half are boys. Consequently, we have no strong evidence that the restructured schools have unique benefits for African-Americans or boys; they may simply represent large subgroups for which it is easier to detect effects that are, in fact, general to students of all races and both sexes.