The Impacts of Philadelphia's Accelerated Schools on Academic Progress and Graduation

Final Report

November 23, 2010

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Policy Research, Inc.

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CONTENTS

TABLES

FIGURES

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EXECUTIVE SUMMARY

As a key part of its strategy for improving high school graduation rates, the School District of Philadelphia (SDP) has authorized several private firms to establish, manage, and staff a network of alternative high schools known as *accelerated schools*. Accelerated schools serve students who are at high risk of failure to graduate, including those who are significantly overage for their grade, have accumulated few high school credits, have previously dropped out, or are chronically truant. Aiming to provide an expedited path to graduation, accelerated schools offer a curriculum in which enrollees are supposed to graduate within three years of entry. Since their inception in the 2004-05 school year, accelerated schools have steadily grown in number and enrollment; by the beginning of the 2009-10 school year, 14 accelerated schools managed by seven different firms (*providers*) enrolled more than 2,100 students. In light of this growth, there is considerable interest in ascertaining whether these schools have contributed to improvements in graduation rates.

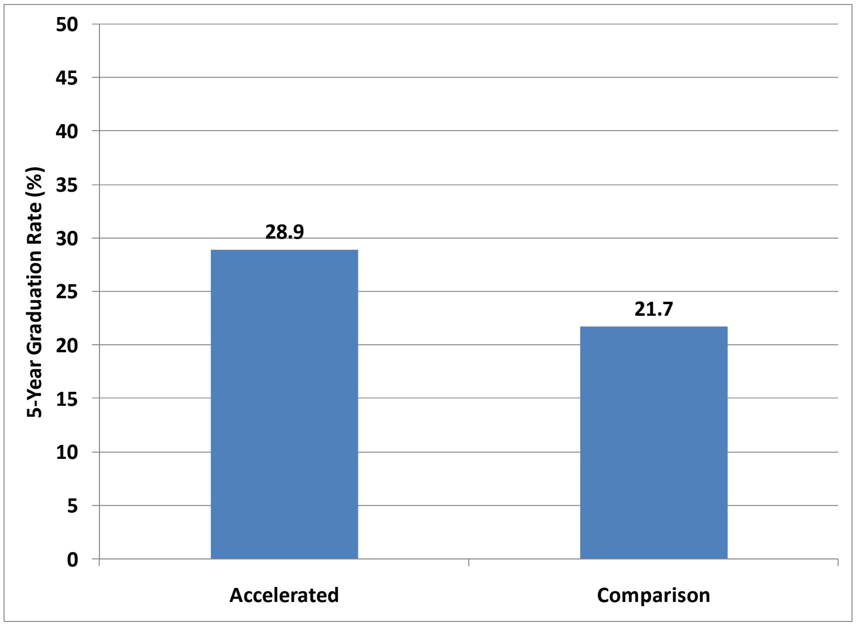
This report evaluates the impacts of Philadelphia's accelerated schools on their enrollees' rates of graduation. In addition, to assess the effects of accelerated schools on more recent enrollees—for whom graduation outcomes are not yet measurable—we also examine the schools' impacts on rates of credit accumulation, a key intermediate indicator of progress toward graduation. Our analyses yield estimates for both the average impacts of the entire network of accelerated schools and the impacts of each individual provider.

The impact analyses are based on a matched comparison design. Each student who enrolls in accelerated education, referred to as an *accelerated student*, is matched with the *nonaccelerated student*—one who never enrolls in accelerated education—exhibiting the most similar set of early risk factors for eventual dropout. Therefore, when accelerated students first enroll in accelerated schools, they are, on average, equivalent to the comparison students—most of whom are enrolled in nonalternative high schools—with respect to cumulative GPA, total accumulated credits, eighth-grade test scores, prior attendance, prior disciplinary history, and key demographic characteristics. Importantly, we assume that accelerated and comparison students are subject to similar academic standards for passing courses and earning high school diplomas. Under this key assumption, differences in outcomes between the accelerated and comparison students capture the impacts of accelerated schools.

A. Key Findings

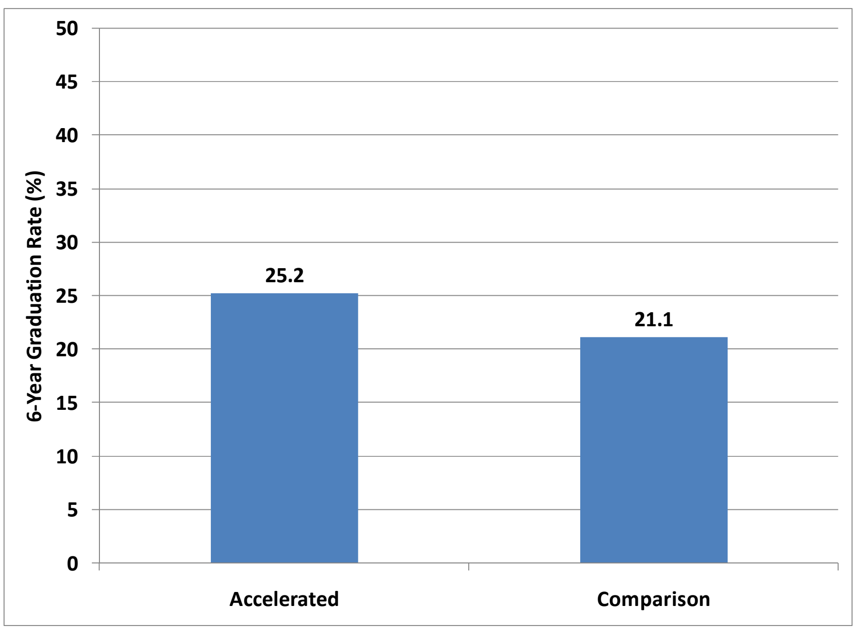
Collectively, accelerated schools demonstrate positive and statistically significant impacts on all outcome measures considered in this study. In the ninth-grade cohorts of 2002-03 through 2005-06, accelerated students had a five-year graduation rate (29 percent) that exceeded the rate for the comparison students (22 percent) by 7 percentage points (Figure ES.1). Likewise, in the ninth-grade cohorts of 2002-03 through 2004-05, the six-year graduation rate was higher by 4 percentage points among accelerated students than among comparison students (Figure ES.2). Considering more recent entrants into accelerated schools, we find that students who entered accelerated schools at the beginning of the 2009-10 school year earned an average of 4.4 credits in that year, amounting to 1.3 more credits than those earned by comparison students (Figure ES.3).

Figure ES.1. Five-Year Graduation Rates of Accelerated and Comparison Students



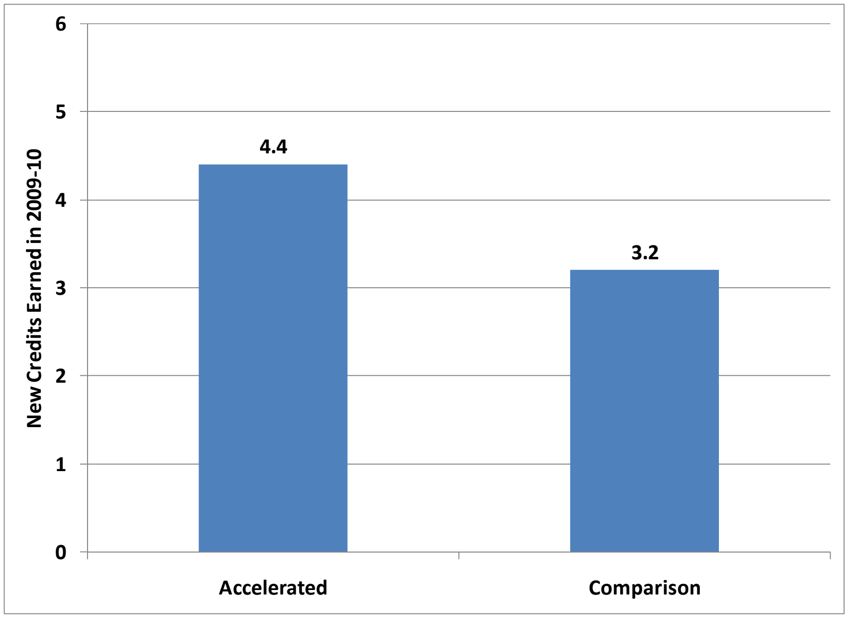
Note: Figure is based on the ninth-grade cohorts of 2002-03 through 2005-06. The outcome difference between accelerated and comparison students is statistically significant at the 0.05 level. Each group consists of 732 students.

Figure ES.2. Six-Year Graduation Rates of Accelerated and Comparison Students



Note: Figure is based on the ninth-grade cohorts of 2002-03 through 2004-05. The outcome difference between accelerated and comparison students is statistically significant at the 0.05 level. Each group consists of 967 students.

Figure ES.3. Average Number of New Credits Earned in the 2009-10 School Year by Accelerated and Comparison Students

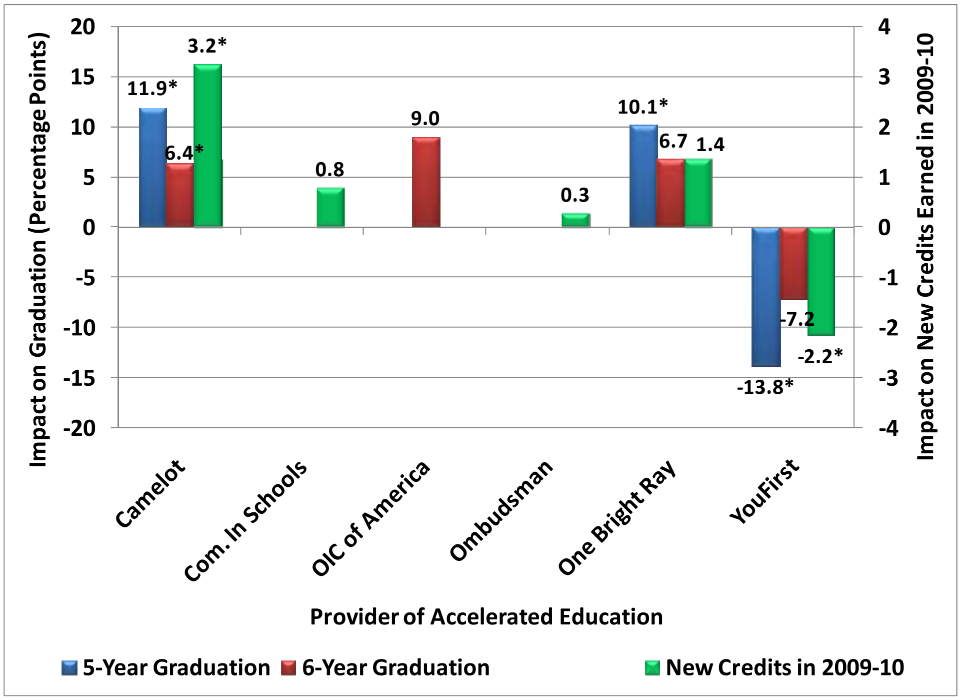


Note: For the accelerated group, the figure is based on students who enrolled in accelerated schools for the first time at the beginning of the 2009-10 school year. The outcome difference between accelerated and comparison students is statistically significant at the 0.05 level. Each group consists of 811 students.

The average impacts of the full network of accelerated schools reflect the net contributions of several providers. Our provider-specific analyses indicate variation among providers in their impacts on graduation and credit accumulation. On all three outcome measures—five-year graduation rates, six-year graduation rates, and new credits earned in the 2009-10 school year—one provider, Camelot, has uniformly positive and statistically significant impacts on its enrollees (Figure ES.4). In contrast, enrollees of schools managed by YouFirst Learning have worse outcomes—in particular, have lower five-year graduation rates and credit-earning rates by statistically significant margins—than comparison students. Due to various background differences among students taught by different providers, the provider-specific analyses are not structured to compare directly the impacts that different providers would have on the same set of students. Nevertheless, the findings provide suggestive indications of possible differences in instructional quality across providers.

Because SDP terminated the contract of YouFirst Learning for reasons of convenience in the fall of 2010, impact estimates that exclude YouFirst schools might be more indicative of potential impacts in future years than estimates that include YouFirst schools. As reflected in Figure ES.4, YouFirst is the clear underperformer among the set of providers, so removing YouFirst schools from the analysis improves the estimated average impacts of accelerated schools. On average, accelerated schools other than those managed by YouFirst improved their enrollees' five- and six-year graduation rates by 11 and 7 percentage points, respectively, and enabled their enrollees to earn 2.2 more credits in 2009-10 than those earned by comparison students.

Figure ES.4. Impacts on Graduation and Credit-Earning Rates, by Provider



Note: The figure includes only impact estimates based on samples in which accelerated and comparison students were equivalent on at least 17 background characteristics. No impact estimates for Big Picture met this criterion.

*Significantly different from zero at the .05 level, two-tailed test.

B. Implications

The evidence from this report supports the conclusion that accelerated schools have raised graduation rates among students in SDP who are at high risk of dropout. In determining how these findings can inform the direction of the district's dropout reduction strategies, policymakers will likely have to assess the substantive importance of the observed impacts. Given that only slightly more than one in five comparison students graduates, the accelerated schools' impacts on graduation rates are sizable in proportion to the comparison group graduation rates. On the other hand, as more than three-fifths of all students in SDP graduate within six years of entering ninth grade, the observed impacts of accelerated schools do not bring the graduation rates of their enrollees close to districtwide graduation rates.

I. INTRODUCTION

As a key part of its strategy for improving high school graduation rates, the School District of Philadelphia (SDP) has authorized the establishment of several alternative high schools known as *accelerated schools*. These schools serve students who are typically overage for their grade, either due to having previously dropped out or having accumulated credits at an insufficient pace for on-time graduation. Aiming to provide an expedited path to graduation, accelerated schools offer a curriculum in which enrollees are supposed to graduate within three years of entry. Because enrollment in accelerated schools has grown rapidly in the past half decade, there is considerable interest in ascertaining whether these schools have contributed to improvements in graduation rates within the district.

This report evaluates the impacts of Philadelphia's accelerated schools on their enrollees' graduation outcomes and intermediate indicators of progress toward graduation. Using a matched comparison group design, we compare the outcomes of accelerated school enrollees with the outcomes of other students who have similar academic trajectories in the early years of high school but who never enroll in accelerated education. The analyses provide the first rigorous impact evaluation of this key component in the district's approach to combating dropout.

In the remainder of this introductory chapter, we provide an overview of accelerated schools in Philadelphia and specify the research questions that guide the analyses of this report.

A. Accelerated Schools in Philadelphia

The growth of accelerated education in SDP has been a policy strategy driven by concerns over the number of dropouts in the district. Although the first accelerated schools were established in the 2004-05 school year, a key catalyst for subsequent expansion of accelerated education was the release of a landmark report by Neild and Balfanz (2006) that documented the large size and predictable characteristics of the district's population of dropouts (see Mezzacappa 2010 for a historical overview of factors that have shaped the district's policies toward dropout). Finding that a substantial number of dropouts are above the normal age of high school education but have accumulated few academic credits, the authors observed that these dropouts "may need programs that allow them to earn high school credits in a more expeditious way" than traditional high school programs do. Accelerated schools have been established as an approach to fill this need.

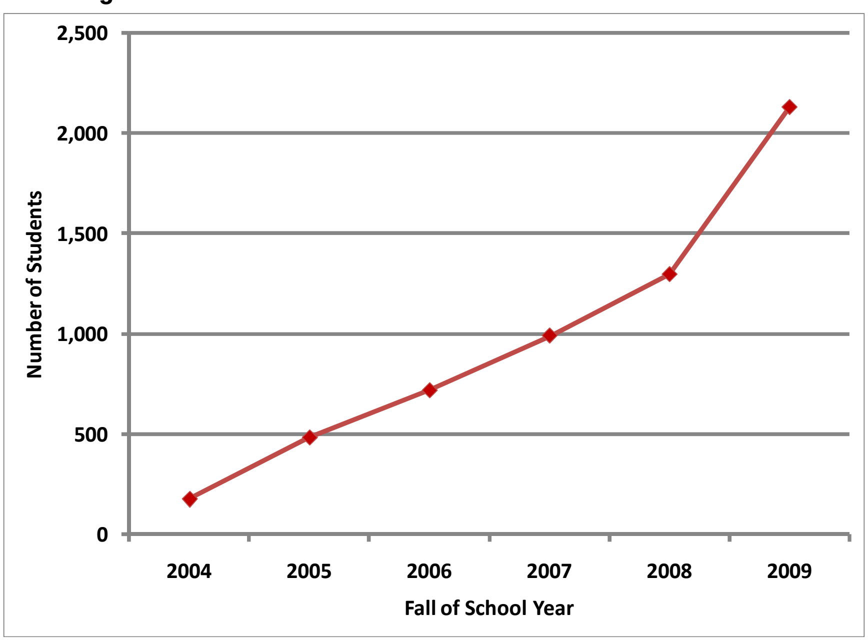
The district's approach has been to contract with private firms—henceforth called providers—that manage and staff the accelerated schools. By the 2009-10 school year, 14 accelerated schools had been established under the management of seven different providers (Table I.1). In every year since the 2004-05 school year, new accelerated schools have been initiated. As a result, enrollment in accelerated schools has climbed rapidly, from fewer than 200 students at the beginning of the 2004-05 school year to more than 2,100 students at the beginning of the 2009-10 school year (Figure I.1).

Table I.1. Accelerated High Schools in SDP, 2004-05 through 2009-10

Name of School	Provider	First School Year	October 2009 Enrollment
Accelerated Learning Academy	YouFirst Learning	2005-06	197
Accelerated Learning Academy South	YouFirst Learning	2007-08	205
Accelerated Learning Academy Southwest	YouFirst Learning	2006-07	180
El Centro de Estudiantes	Big Picture	2009-10	209
Excel Academy	Camelot	2004-05	307
Excel Academy South	Camelot	2009-10	201
Fairhill Community High School	One Bright Ray	2004-05	211
North Philadelphia Community High School	One Bright Ray	2007-08	184
Ombudsman Northeast Accel	Ombudsman	2009-10	29
Ombudsman Northwest Accel	Ombudsman	2009-10	29
Ombudsman West Accel	Ombudsman	2009-10	42
Open Door High School	Camelot	2008-09	50
Opportunities Industrialization Center of America's Career and Academic Development Institute	OIC of America	2004-05	182
Performance Learning Center Southwest	Communities in Schools	2009-10	108

Source: SDP administrative data.

Figure I.1. Number of Students Enrolled in Accelerated Schools at the Beginning of Each School Year, 2004-05 through 2009-10



Note: Figure shows the number of students enrolled on October 1 of the fall term of the indicated school year.

All accelerated schools share two key features. First, they target students who are at high risk of failure to graduate. Students who are eligible to enroll in accelerated programs must demonstrate at least one of several risk factors, including being significantly overage for their grade, having accumulated few high school credits, having previously dropped out, or being chronically truant. Second, these schools offer an accelerated curriculum in which the objective is for students to attain the 23.5 total credits needed for graduation in no more than three years. Beyond these defining features, there are a number of other programmatic similarities among accelerated schools. Because students enter these schools at varying levels of literacy and numeracy, most of the accelerated schools incorporate some degree of individualized instruction, which often takes the form of computer-assisted instruction that enables students to learn at their own pace. Several of the schools also emphasize project-based learning, which promotes individualized inquiry and problem-solving in real-life applications. In addition, accelerated schools hire support service staff who are supposed to connect enrollees with social services and career development resources and to establish communication with the enrollees' families.

SDP has oversight authority over the accelerated schools and, in recent years, has specified performance standards that providers are expected to meet. In particular, providers must demonstrate that their enrollees achieve specified criteria for attendance, performance on the Pennsylvania System of School Assessment (PSSA), academic growth, and graduation. The district can terminate the contracts of providers for poor performance or, more simply, for reasons of convenience. Exercising its authority, SDP terminated the contract of one provider, YouFirst Learning, for reasons of convenience in the fall of 2010.

B. The Current Study: Motivation and Objectives

This study assesses whether and to what extent accelerated schools improve their students' likelihood of graduating. Our evaluation aims to fill a gap in the current set of available information on the performance of accelerated schools. In previous analyses, we have documented that only a minority of students who enroll in accelerated schools eventually graduate (Chiang and Gill 2010). However, these previous analyses do not discern the graduation rates that these students would have attained if they had not enrolled in accelerated schools. To assess the *causal* impacts of accelerated schools, it is essential to compare the enrollees' actual outcomes with an estimate for the outcomes that they would otherwise have had in the absence of accelerated education. Our study carries out this type of causal analysis.

We address three objectives in this study. First, the central aim is to estimate the impacts of accelerated schools on graduation rates, the primary outcome of interest. Second, as graduation outcomes cannot be measured for the most recent enrollees in accelerated schools, we also examine accelerated schools' impacts on rates of credit accumulation, a key intermediate indicator of progress toward graduation. The impacts on credit accumulation reflect whether the most recent enrollees in accelerated education achieve faster academic progress than they otherwise would have achieved outside of accelerated schools. Third, because providers might vary in their effectiveness, we evaluate the effects of each individual provider on graduation and credit-earning rates.

The remainder of this report is structured as follows. Chapter II explains the data and evaluation methods used to estimate the impacts of interest. Chapter III provides a detailed discussion of the impact findings. Chapter IV summarizes the key results and discusses their implications.

II. DATA AND ESTIMATION METHODS

To estimate the impacts of accelerated schools on graduation and credit accumulation, our basic approach is to compare the outcomes of two groups of students: (1) accelerated students, consisting of those who enroll in accelerated schools at some point after ninth-grade entry; and (2) a subset of nonaccelerated students—that is, students who never enroll in accelerated schools—who are similar to the accelerated students with respect to demographic characteristics, eighth-grade achievement, and academic progress and behavior in their early years of high school. This chapter describes the data and empirical methods with which we implement this approach. We begin by discussing the student-level longitudinal data used for the study's analyses. We then specify the key outcomes on which impacts are evaluated, the samples included in the analyses, and the statistical methods used in estimating the impacts.

A. Data

We based all of the study's analyses on SDP's administrative database of student-level longitudinal data covering the full population of students in the district and Philadelphia's charter schools. Our extract of the data includes all students in third through twelfth grades and spans the school years from 2001-02 through 2009-10. Due to the longitudinal structure of the data, we are able to follow several cohorts of first-time ninth graders until the point at which graduation status is measured; we refer to ninth-grade cohorts by the school year in which they are first observed in ninth grade within our data. As we cannot determine whether students in 2001-02, the first school year of our sample, were in high school during the previous year, the 2002-03 ninth-grade cohort is the earliest identifiable cohort in our data.[1]

The administrative data include information relevant to constructing the study's outcome variables and matching accelerated students with similar nonaccelerated students. The records contain annual data on each student's enrollment status, dates of entry into and exit from each school, reasons for exit, attendance, disciplinary incidents, scores on the PSSA, total number of accumulated credits, cumulative grade point average (GPA), and demographic characteristics. Although they are contained in a centralized district database, several of these variables depend on information recorded into the database by school administrators. Conversations with district staff reveal that certain types of information—in particular, credits, GPA, attendance, and disciplinary incidents—were consistently and reliably recorded by alternative schools, including accelerated schools and transition schools that serve students with disciplinary violations, only beginning in the 2009-10 school year; regular SDP schools—that is, nonalternative schools—are presumed to have recorded all information reliably in the entire sample period. As discussed later in this chapter, it is important to account for these data quality limitations when structuring the analyses.

B. Outcomes for Analysis

Because the central objective of accelerated schools is to enable their students to graduate, graduation is the study's primary outcome of interest. In addition, given that graduation status can

[1] A student who is observed in ninth grade in a specified school year is classified into the ninth-grade cohort of that year as long as he or she is not observed in a high school grade in any previous year of the data. The student does not necessarily need to be observed in eighth grade in the previous year.

be measured only after several years have elapsed since students' entry into accelerated schools, it is also useful to examine intermediate outcomes that reflect progress toward graduation. The key intermediate outcome that we examine is the rate at which students accumulate academic credits, which we can measure even for students who enter accelerated schools near the end of our sample period. Both of these outcomes share the key advantage that they can be measured regardless of whether and when students drop out; in contrast, other types of academic outcome measures, such as scores on the eleventh-grade PSSA, are not observed for students who drop out before the time of measurement. However, both graduation and credit accumulation are potentially dependent on schools' own standards for the performance that their students need to demonstrate to earn passing grades and high school diplomas. Because SDP fully recognizes the credits and diplomas attained by students in accelerated schools, we assume that accelerated schools' standards are comparable to those of the nonalternative schools—mainly nonselective, traditional high schools known as neighborhood schools—in which other at-risk students are enrolled, enabling a meaningful impact analysis. If, on the other hand, this assumption is incorrect and academic standards are not similar across the two types of high schools, then the impact estimates will not be based on a fair comparison of outcomes. Given this key assumption, we describe in more detail the manner in which the outcomes of interest are measured.

1. Graduation Status in Spring 2007 through Spring 2010

In order to define graduation status in a uniform manner across all students in a given analysis, we measure graduation status at the end of a specified period after ninth-grade entry, referred to as the *follow-up period*; defining graduation based on a uniform follow-up period allows for greater clarity in interpreting any observed impacts. Policy discussions and prior research on graduation rates have typically focused on whether students attain graduation within four, five, or six years after ninth-grade entry—which we refer to as the students' four-, five-, and six-year graduation status (see, for example, Neild and Balfanz 2006; Engberg and Gill 2006; Booker et al. 2008). Because accelerated schools target students who are not previously on track to attain on-time (four-year) graduation, our analysis focuses on five- and six-year graduation status.

Given the ninth-grade cohorts observed in the data, measurement of graduation status occurs in spring 2007 through spring 2010. In particular, because the earliest ninth-grade cohort in the analysis is the 2002-03 cohort, graduation status is measured no earlier than spring 2007, when the five-year graduation status for the 2002-03 cohort is assessed.[2]

In all analyses, a student's graduation status is captured by a dichotomous indicator for whether the student has graduated within the specified follow-up period. A student is regarded as not having graduated if the student is still enrolled in SDP or a Philadelphia charter school at the end of the follow-up period or if the student's final exit from the combined SDP-charter population of schools in this period is classified by the district as a case of dropout. If a student has transferred out of the combined SDP-charter population of schools, then his or her graduation status is not known with certainty and must be imputed; we discuss the imputation approach later in this chapter.

[2] More precisely, we follow SDP's practice of measuring graduation status as of the September 30 that immediately follows the final spring term of the follow-up period. This approach accounts for students who graduate as a result of taking summer classes. However, for follow-up periods that end in spring 2010, we must assess graduation status as of June 30, 2010 rather than September 30, 2010, because the available data did not extend beyond June 2010 at the time of our analysis.

2. New Credits Earned in 2009-10

To assess the impacts of accelerated schools on their more recent enrollees, our analysis examines the number of new credits that students earn in the 2009-10 school year. For each student, we construct this outcome measure as the difference between the total number of accumulated high school credits at the end of the 2009-10 school year and the total number at the end of the 2008-09 school year. This is the only measure of credit accumulation that can be based on reliable data; in particular, because records of total accumulated credits submitted by accelerated schools are reliable only beginning in the 2009-10 school year, each student's final record for total accumulated credits must come from that year.

Moreover, among accelerated students, only students who enter accelerated schools for the first time in 2009-10 can have reliable measurements for credit-earning rates in 2009-10. Among students who enter accelerated schools in the 2008-09 school year or earlier, those who drop out before the 2009-10 school year do not have reliable final records for total accumulated credits. Thus, the samples of accelerated students for this analysis, defined later in greater detail, can include only first-time enrollees in accelerated schools in 2009-10.

C. The Need for Matched Comparisons

A comparison of outcomes between accelerated and nonaccelerated students represents the core of our approach to estimating the impacts of accelerated schools. A critical element of our approach, however, is that only a carefully selected subset of nonaccelerated students serves as the comparison group with which the outcomes of accelerated students are compared. As described later in this chapter, we match each accelerated student with the nonaccelerated student who is most similar with respect to key early indicators—measured before the accelerated student's entry into accelerated schools—of the likelihood of graduation.

The need for careful selection of the comparison group is underscored by Table II.1, which shows average characteristics at the end of ninth grade—before the start of accelerated education for any student in the sample—among students who eventually do or do not enroll in accelerated schools. On nearly all measures of academic progress or performance in eighth or ninth grade, accelerated students perform substantially worse than the general population of nonaccelerated students. Relative to nonaccelerated students, accelerated students have 2.8 fewer accumulated credits by the end of ninth grade, a ninth-grade GPA that is lower by 1.3 grade points, and an attendance rate that is lower by 17 percentage points. For each characteristic, a useful summary measure of the contrast between accelerated and nonaccelerated students is the standardized difference, computed as the raw group difference divided by the average within-group standard deviation. A general rule of thumb is that standardized differences of 0.25 or larger represent substantively important contrasts (Ho et al. 2007); by this rule of thumb, accelerated and nonaccelerated students differ by a substantively important degree on a majority of the characteristics shown in Table II.1.

Table II.1. Characteristics of Accelerated Students and the Full Population of Nonaccelerated Students at the End of Their First Ninth-Grade Year

	Accelerated Students		Nonaccelerated Students		
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Standardized Difference
PSSA math score in eighth grade (z-score units)	-0.45	0.72	0.01	1.03	-0.53
PSSA reading score in eighth grade (z-score units)	-0.41	0.83	-0.01	1.02	-0.43
Credits earned by end of first ninth-grade year	2.6	2.5	5.4	2.6	-1.12
GPA at end of first ninth-grade year	0.56	0.78	1.81	1.19	-1.25
Age at ninth-grade entry	15.3	0.7	14.7	0.8	0.84
Enrolled days in first ninth-grade year	168	32	170	32	-0.07
Fraction of enrolled days from first ninth-grade year spent in:					
Neighborhood schools	0.94	0.22	0.70	0.45	0.68
Magnet or citywide admission schools	0.05	0.20	0.29	0.45	-0.70
Disciplinary/transition schools	0.00	0.05	0.00	0.04	0.03
Fraction of enrolled days from first ninth-grade year in which student was:[a]					
In attendance	0.64	0.22	0.81	0.21	-0.77
Suspended	0.02	0.03	0.01	0.02	0.44
Female	0.48	0.50	0.50	0.50	-0.03
Black	0.61	0.49	0.66	0.47	-0.11
White	0.10	0.29	0.14	0.35	-0.14
Hispanic	0.27	0.44	0.13	0.34	0.36
Designated with disability	0.17	0.38	0.15	0.36	0.05
Ever designated as Limited English Proficient	0.07	0.25	0.08	0.28	-0.06
Ever received TANF	0.68	0.47	0.53	0.50	0.32
Sample Size	**732**		**52,177**		

Note: Table entries are based on students who are eligible for inclusion in the five-year graduation analysis. The standardized difference is the difference in means between accelerated and nonaccelerated students divided by the pooled standard deviation; the pooled standard deviation is the square root of the unweighted average of the accelerated and nonaccelerated variances.

[a]Values are based only on students' time in regular SDP high schools.

Simple comparisons of graduation outcomes between accelerated students and the general population of nonaccelerated students would therefore confound the true impacts of accelerated schools with the large group differences in background characteristics shown in Table II.1. Our matching-based empirical strategy, known as a *quasi-experimental analysis*, balances observable background characteristics between accelerated and nonaccelerated students; the resulting impact estimates cannot be the artifact of observable differences between the types of students who do and do not enroll in accelerated education. However, our analysis shares the key limitation inherent in all quasi-experimental analyses: unobserved influences on graduation outcomes might still differ between accelerated students and the matched comparison students, and there remains the potential for impact estimates to reflect, at least in part, these unobserved differences. Although randomly assigning students to receive or not to receive accelerated education would eliminate this threat to internal (causal) validity, there is no such randomized design on which we can base our analyses. Thus, our quasi-experimental strategy reaches the maximal internal validity that is achievable in the absence of a randomized design.

D. Defining the Treatment Group for the Analysis

Before obtaining impact estimates, it is important to define precisely the samples of students who constitute the *treatment group*, the group of accelerated students to be included in the analysis, and the *potential comparison group*, the full group of nonaccelerated students from which selected students can serve as matches for the accelerated students. In all analyses, the samples include only students who enroll in a regular SDP high school (neighborhood, citywide admission, or magnet school) at some point in their first ninth-grade year; we do not consider students who transfer into SDP after their first ninth-grade year because we cannot determine the types of schools they attended before entering SDP.[3] Beyond this basic condition, a number of other conditions define the treatment and potential comparison groups for which the main outcomes can be meaningfully measured. Each type of outcome yields a different analysis sample.

We first describe the criteria for defining the treatment group. Three factors determine whether an accelerated student can be included in the analysis: (1) the length of the student's *baseline period*, the period from ninth-grade entry to the student's first enrollment in accelerated schools; (2) the ninth-grade cohort to which the student belongs; and (3) the type of SDP school that the student last attends before entering accelerated schools.

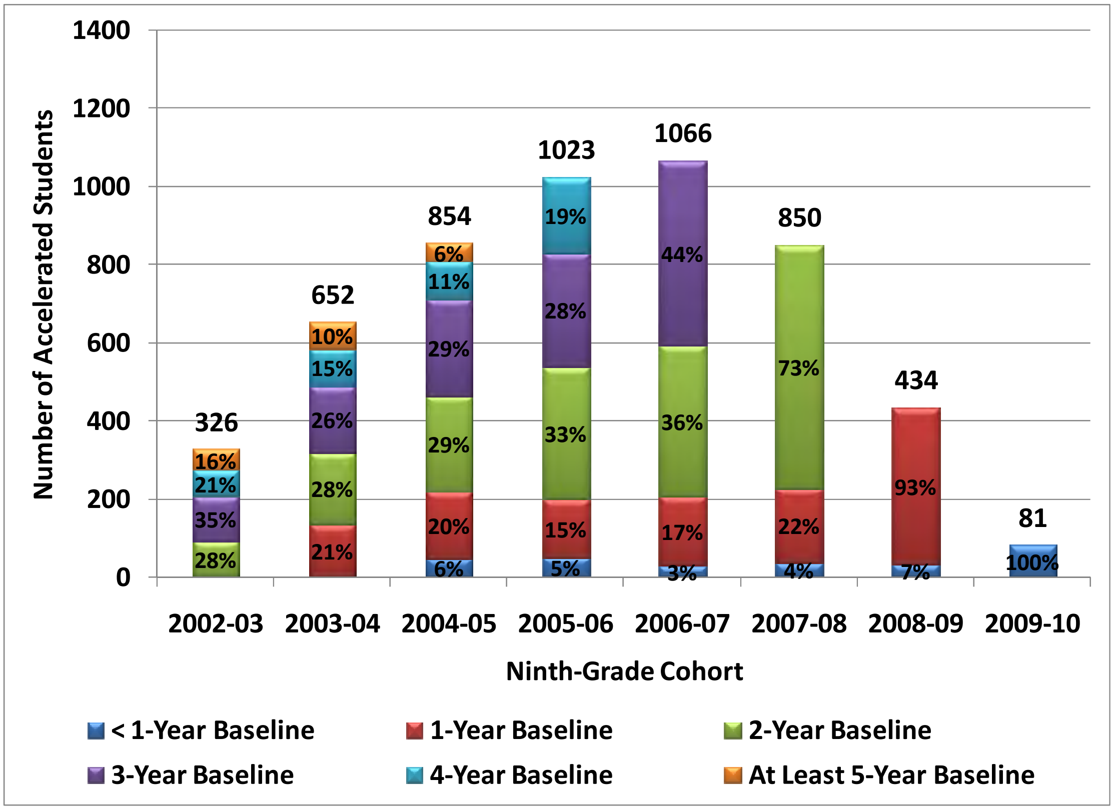
Length of the baseline period. The timing of an accelerated student's entry into accelerated schools determines whether accelerated education could have meaningfully contributed to the student's outcome. For instance, for a student entering accelerated education in his or her fifth year after ninth-grade entry, we would have to measure five-year graduation status at the end of the same school year, rather than after the three-year period in which accelerated schools aim to graduate their enrollees. Therefore, this particular student's five-year graduation status would not be indicative of the full contribution of the student's accelerated school. It is thus important to identify the students for whom a full three-year period can elapse between entry into accelerated schools and measurement of graduation status.

To identify students who can be included in the treatment group, it is convenient to divide into two distinct periods each accelerated student's time from ninth-grade entry to the point at which graduation status is measured: (1) the baseline period, consisting of all years before the year in which the student first enrolls in an accelerated school; and (2) the intervention period, starting from the first year in which the student is enrolled in accelerated schools. This terminology reflects the notion

[3] In addition, only students who are observed in a regular SDP high school at some point in their first ninth-grade year have reliable measures for attendance, credit accumulation, and disciplinary incidents in ninth grade, all of which are variables used to impute values for missing eighth-grade PSSA scores, as described in Appendix B.

that accelerated education represents the intervention of interest. As seen in Figure II.1, most accelerated students have baseline periods ranging from one to four years. For expository convenience, we refer to students with baseline periods of one, two, three, and four years as, respectively, the one-year, two-year, three-year, and four-year *baseline groups*.

Figure II.1. Percentage of Accelerated Students with Specified Baseline Period Durations, by Ninth-Grade Cohort



Note: Figure is based on all accelerated students who enrolled in a regular SDP high school at some point in their first ninth-grade year.

As stated earlier, an accelerated student can be included in the analysis of a particular graduation outcome only if three or more years can elapse after his or her entry into accelerated schools before graduation status is measured. For the intervention period to be at least three years, the following sample conditions are required:

- Accelerated students can be included in the five-year graduation analysis if they enter accelerated education by the beginning of their third year after ninth-grade entry—that is, if they belong to the one- and two-year baseline groups.
- Accelerated students can be included in the six-year graduation analysis if they enter accelerated education by the beginning of their fourth year after ninth-grade entry—that is, if they belong to the one-, two-, and three-year baseline groups.

We exclude the few accelerated students whose baseline period is less than one year so that at least one baseline year of academic progress is available as information for matching accelerated and comparison students. Table II.2 summarizes the sets of baseline groups that pertain to each outcome.

Table II.2. Baseline Groups and Ninth-Grade Cohorts Represented in the Final Analysis Sample, by Outcome

Outcome	Baseline Groups in the Analysis Sample	Ninth-Grade Cohorts in the Analysis Sample
Five-year graduation	1-year, 2-year	2002-03, 2003-04, 2004-05, 2005-06
Six-year graduation	1-year, 2-year, 3-year	2002-03, 2003-04, 2004-05
New credits earned in 2009-10	1-year, 2-year, 3-year, 4-year	Implied by baseline groups

For the analysis of new credits earned in 2009-10, accelerated students from any baseline group could, in principle, be included in the analysis because we always measure the outcome in the first year of a student's intervention period. However, due to small sample sizes, analyses for students with very long baseline periods are subject to greater imprecision. We therefore include in the credits analysis only those accelerated students with baseline periods of one to four years (Table II.2).

Ninth-grade cohorts. The ninth-grade cohort to which an accelerated student belongs is another determinant of whether we can feasibly measure the student's graduation outcome. In order for students' five- or six-year graduation status to be measurable, they must have entered ninth grade at least, respectively, five or six years before the end of the period covered by our data. As shown in the final column of Table II.2, the ninth-grade cohorts of 2002-03 through 2005-06 can be included in the five-year graduation analysis, and the ninth-grade cohorts of 2002-03 through 2004-05 can be included in the six-year graduation analysis.

In the credits analysis, because every accelerated student's intervention period starts in the 2009-10 school year, each student's baseline group fully implies the ninth-grade cohort to which he or she belongs. Specifically, students from the one-year, two-year, three-year, and four-year baseline groups belong to the ninth-grade cohorts of 2008-09, 2007-08, 2006-07, and 2005-06, respectively.

Final SDP school in the baseline period. Several key baseline variables, including a student's GPA and number of accumulated credits at the end of his or her baseline period, depend on information recorded by the final school in which the student is enrolled during the baseline period. Because only regular SDP schools reliably record such information during the baseline periods covered by our analysis, the analysis samples include only students whose final school in the baseline period is a regular SDP school.[4]

After applying all of the sample conditions discussed previously, the final treatment group consists of 732 students for the five-year graduation analysis, 967 students for the six-year graduation analysis, and 811 students for the credits analysis. Appendix A summarizes the extent to which each condition affects the sample size.

[4] A school is not considered regular if it is a charter school, a transition school for students with disciplinary violations, or a night school that offers only evening classes.

E. Defining the Potential Comparison Group for the Analysis

1. Baseline Groups and Ninth-Grade Cohorts

To identify a matched comparison group for the sample of accelerated students, we select nonaccelerated students from a larger group, the potential comparison group, composed of all students who are eligible to serve as matches. For delineating the potential comparison group, the sample conditions determining the admissible sets of baseline groups and ninth-grade cohorts parallel those that define the treatment group.

First, we classify nonaccelerated students into the same baseline groups as those that divide the sample of accelerated students. For each particular outcome, only the nonaccelerated students in baseline groups that also contain treatment group members—as indicated by Table II.2—can be included in the analysis. The assignment of nonaccelerated students to baseline groups, however, is not based on the timing of enrollment in accelerated schools. Instead, a nonaccelerated student belongs to a specified baseline group (for instance, the two-year baseline group) if he or she does not graduate or transfer out of SDP during that baseline period (for instance, during the first two years after ninth-grade entry). By definition, accelerated students in the given baseline group did not graduate or transfer out in the baseline period—and would thus have been highly unlikely to have done so if accelerated schools were nonexistent. Thus, only nonaccelerated students who also do not make these types of exits are reasonably comparable to, and can serve as potential matches for, the accelerated members of the baseline group. Importantly, unlike accelerated students, nonaccelerated students can belong to more than one baseline group; a nonaccelerated student who has not graduated or transferred out of SDP by the end of the third year after ninth-grade entry belongs to the one-year, two-year, and three-year baseline groups.[5]

Other conditions for defining the potential comparison group are identical to those for defining the treatment group. The ninth-grade cohorts relevant to each outcome among accelerated students, as shown in Table II.2, are the same ones to which nonaccelerated students must belong. Moreover, from each baseline group, only nonaccelerated students who last attended a regular SDP school in the baseline period can be included in the analysis.

2. Specifying the Counterfactual

Students who are selected from the potential comparison group to be matched with accelerated students are supposed to represent the *counterfactual*—that is, the set of experiences and outcomes that the matched accelerated students would have exhibited in the absence of accelerated education. Because we do not directly observe the hypothetical scenario in which accelerated students never enroll in accelerated schools, we must use informed judgment to identify the types of experiences among nonaccelerated students that could and could not represent reasonable proxies for the true counterfactual. By specifying the set of admissible experiences that can represent the counterfactual, we further define the types of students who can belong to the potential comparison group.

In our main analyses, we assume a counterfactual in which accelerated students would have enrolled in another type of SDP school at the beginning of their intervention period if they had not

[5] However, as discussed in Appendix B, any comparison student who is matched to an accelerated student in a particular baseline group is no longer eligible to be matched to accelerated students in other baseline groups.

enrolled in an accelerated school. We build this assumption into one additional criterion for defining the potential comparison group: in each baseline group, nonaccelerated students must be enrolled in an SDP school at some point in the first year after the baseline period—the first year in which their accelerated counterparts enroll in accelerated schools. Specifying this criterion carries a key advantage for the analysis: the nonaccelerated students who meet this criterion are more likely to be similar to accelerated students in levels of motivation than the nonaccelerated students who are never enrolled beyond the baseline period—that is, those who have already dropped out. In other words, this sample criterion makes it less likely that treatment-control outcome differences would be the artifact of differences in attitudes toward education or other similar types of unobservable differences.

Our main approach, however, does not account for the possibility that some of the students who are recruited into accelerated schools might have otherwise dropped out of school entirely. By excluding from the potential comparison group any students with a final occurrence of dropout in the baseline period, our main approach could produce conservative estimates for the impacts of accelerated schools. In supplementary analyses, we relax this sample condition to test the sensitivity of the main findings.

F. Matching and Impact Estimation

After defining the treatment and potential comparison groups, estimating the impacts of accelerated schools entails two key steps. First, we match each accelerated student with the single nonaccelerated student who has the most similar characteristics and academic progress from the baseline period. Second, we calculate the average difference in outcomes between the treatment group and the matched comparison group, adjusting the outcome difference for any remaining baseline differences. We repeat these steps, summarized next, for the estimation of every type of impact, including the average impact of all accelerated schools and the impact of each provider. Appendix B provides details of the analytical methods.

1. Matching

The objective of the matching step is to select a group of nonaccelerated students who exhibit, during the baseline period, similar risk factors for eventual dropout as the group of accelerated students. Our analysis approach uses standard methods known as *propensity score matching*. In particular, within each baseline group, we estimate the association between several baseline risk factors and the probability of enrolling in accelerated education. From these estimated relationships, we can identify all students' probabilities of enrolling in accelerated schools—regardless of whether they actually do so—based on each student's unique set of baseline risk factors. We then match each accelerated student with the nonaccelerated student in the same ninth-grade cohort and baseline group with the most similar probability of entering accelerated schools.[6] As a result of this procedure, we expect the risk factors considered in the analysis to be balanced between the treatment group and the matched comparison group.

[6] We employ matching with replacement, meaning that nonaccelerated students can be included in more than one matched pair.

2. Baseline Variables on Which Matches Are Based

The matched comparison group is selected for its similarity with the treatment group based on 18 baseline variables. Baseline variables are defined over a different duration—the full length of the baseline period—in each different baseline group. Because matching occurs only within baseline groups, all of the students who could potentially be matched with each other share the same duration of measurement for the baseline variables. The key advantage of this approach is that it uses information on the entire period before enrollment in accelerated schools to measure an accelerated student's risk factors and to identify the comparison student with the most similar risk factors.

Matching is based on the following 18 variables measured over the full baseline period:

- **Eighth-grade achievement:** Eighth-grade PSSA reading score (expressed as standard deviations from the districtwide mean); eighth-grade PSSA math score (expressed as standard deviations from the districtwide mean); indicator for missing eighth-grade scores
- **Academic progress:** Number of accumulated credits; cumulative GPA
- **Enrollment pattern:** Enrolled days per year after ninth-grade entry; fraction of enrolled high school days in each of three types of schools (neighborhood, magnet or citywide, and transition)
- **Behavior:**[7] Attendance rate in high school; fraction of enrolled days spent in suspension
- **Demographic characteristics:** Age at ninth-grade entry; indicators for females, blacks, Hispanics, students ever designated with a disability, students ever designated as Limited English Proficient, students who ever received Temporary Assistance for Needy Families (TANF)

Because eighth-grade PSSA scores are missing for approximately one-fifth of the students in our analysis, we substitute imputed values for missing eighth-grade scores to ensure that students with missing scores can be included in the analysis. Appendix B discusses the imputation strategy. We also conduct supplementary analyses based only on students with nonimputed eighth-grade scores.

3. Assessing Treatment-Control Balance in Baseline Characteristics

To the extent that there is greater balance (that is, smaller differences) in baseline characteristics between the treatment and matched comparison groups, we can have greater confidence that comparison outcomes represent those that accelerated students would have exhibited in the absence of accelerated education. Therefore, it is important to check the degree of treatment-control balance in each baseline characteristic. Although we do not expect the balance to be perfect, a general rule of thumb, discussed earlier, is that the standardized difference in each baseline variable between the two matched groups should be less than 0.25. For all analyses, we calculate the treatment-control

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[7] We based these variables exclusively on time in regular SDP high schools, the only schools that provide reliable records of these variables in the considered baseline periods.

standardized difference in every baseline variable,[8] and we deemphasize any analyses based on matched samples with standardized differences of 0.25 or more.

Because our main matching approach selects, for each accelerated student, the best match among all nonaccelerated students in the district who belong to the same ninth-grade cohort and baseline group, it ensures the maximal degree of treatment-control balance on the 18 baseline characteristics specified above. Nevertheless, unobserved risk factors might still differ between the treatment and matched control groups. If some of these unobserved factors, such as neighborhood quality, are specific to the high school in which students enter ninth grade, then one approach to achieving potentially better balance on such factors is to match students who share the same initial high school. Therefore, we conduct supplementary analyses based solely on matching within the same high school of entry.[9] However, because this alternative approach considerably restricts the set of nonaccelerated students with whom each accelerated student can be matched, matched pairs formed by this method will tend to be less similar on the 18 observable baseline characteristics than matched pairs in our main matching approach; moreover, there is no way to verify any improved balance on the unobserved risk factors. For these reasons, in most analyses we choose not to impose the requirement that matches be formed within the same initial high school.

4. Impact Estimation

Upon identifying a matched comparison group for the treatment group, comparing the average outcomes of the two groups is the essential basis for estimating the impacts of accelerated schools. However, because the treatment-control balance in baseline characteristics will not be perfect, it is important to adjust for the remaining baseline differences so that the impact estimates will not be the artifacts of these differences. To adjust for these differences, we estimate the relationship between outcomes and baseline covariates; for each comparison student, we then generate an adjusted outcome, representing an estimate for the outcome that the student would have earned if he or she had exhibited exactly the same baseline characteristics as his or her matched counterpart in the treatment group (see Appendix B). The final impact estimate is the difference between the average outcome in the treatment group and the average adjusted outcome in the matched comparison group.

5. Accounting for Students Who Transfer Out of SDP

A final empirical issue that must be accounted for is that outcomes are unobserved for students who transfer out of SDP during the intervention period, referred to as *out-transfers*. For instance, if a student transfers out of SDP in his or her fourth year after ninth-grade entry, we do not observe whether the student graduated in a five-year (or six-year) follow-up period. Likewise, if a student transfers out in the middle of the 2009-10 school year, the student's final number of academic credits at the end of 2009-10 is unobserved.

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[8] Because variables are defined over different durations in different baseline groups, we first calculate a standardized treatment-control difference in each baseline group separately and then take a weighted average of these differences across baseline groups, with weights equal to the number of treatment group members.

[9] More precisely, for each accelerated student, we select the most similar nonaccelerated student in the same ninth-grade cohort and baseline group whose first regular SDP high school is the same as that of the accelerated student.

Although one potential approach is to exclude out-transfers from the analysis, this approach can lead to biased—that is, systematically inaccurate—impact estimates if transfer rates differ between the treatment and matched comparison groups. Suppose, for instance, that baseline characteristics were initially balanced between the full treatment sample and the full matched comparison sample. If transferring were more prevalent in the comparison group than in the treatment group, then students who did not transfer out would be less representative of all students in the comparison group than in the treatment group. As a result, among the students who did not transfer out, the treatment group and matched comparison group would be unlikely to be balanced on baseline characteristics. Indeed, as we document in Chapter III, transfer rates in the matched comparison group exceed those in the treatment group, suggesting potential biases from excluding out-transfers from the analysis.

To avoid the pitfalls of excluding out-transfers, our preferred estimation approach includes all treatment and matched comparison students in the analysis. Using regression methods, we impute the outcomes of out-transfers based on the outcomes of other students with the same treatment status who exhibit similar baseline characteristics. Appendix B provides details on the imputation strategy. As a sensitivity check, we provide supplemental findings based on matched samples that exclude out-transfers.

III. ESTIMATED IMPACTS OF ACCELERATED SCHOOLS

This chapter presents estimates for the impacts of accelerated schools on graduation rates and rates of credit accumulation. We first discuss findings for the average impacts of the entire system of accelerated schools, and then we present the estimated impacts of individual providers. As discussed in Chapter II, each impact estimate is the difference between (1) the average outcome among accelerated students and (2) the average outcome in the matched comparison group, adjusted so that comparison students' outcomes are estimates for the outcomes they would have attained if they exhibited identical baseline characteristics as their accelerated counterparts. We present several of the impact findings by showing both the group averages and the resulting impact estimates.

A. Average Impacts on Graduation Rates

1. Treatment-Control Balance in Baseline Characteristics

Before examining the main impact estimates, it is important to identify threats to the causal validity of these analyses. In particular, as discussed in Chapter II, our confidence in the causal validity of the impact estimates depends on the extent to which baseline characteristics are balanced between the treatment group and matched comparison group. Table III.1 shows group means for several key measures of performance, progress, and behavior in the baseline period. On all measures, treatment-control differences are trivial in magnitude. Both groups score about 0.4 to 0.5 standard deviations below the district average on the eighth-grade PSSA, earn an average of 3.5 (in the five-year sample) or 4.5 (in the six-year sample) credits in the baseline period, and are in attendance for about three-fifths of their enrolled days. More formally, Appendix Tables C.1 and C.2 show treatment-control standardized differences for all 18 baseline covariates in, respectively, the five- and six-year analyses. No standardized difference exceeds 0.1; that is, on all baseline measures, differences between accelerated and matched comparison students are substantively small.

Table III.1. Mean Baseline Characteristics of Students in the Analysis of Graduation Outcomes

	Five-Year Graduation Analysis		Six-Year Graduation Analysis	
Variable Measured in the Baseline Period	Accelerated Group	Matched Control Group	Accelerated Group	Matched Control Group
8th-grade PSSA math score	-0.45	-0.42	-0.45	-0.43
8th-grade PSSA reading score	-0.41	-0.38	-0.43	-0.42
High school credits	3.49	3.49	4.51	4.47
High school GPA	0.56	0.57	0.51	0.48
Annual enrolled days in high school	164	165	161	160
Attendance rate[a]	0.62	0.62	0.61	0.60
Fraction of days suspended[a]	0.02	0.02	0.02	0.03
Weighted sample size	**732**	**732**	**967**	**967**

Note: Control students are weighted by the number of matches in which they are included.

[a] Values are based only on students' time in regular SDP high schools.

2. Main Impact Estimates

Our main impact findings indicate that accelerated schools have a positive, statistically significant impact on both five- and six-year graduation rates (Table III.2). Whereas 28.9 percent of accelerated students graduate within five years of ninth-grade entry, only 21.7 percent of the matched comparison students do so; the difference, 7.2 percentage points, captures the impact of accelerated schools. Accelerated schools generate a somewhat smaller (but still statistically significant) gain, 4.1 percentage points, in their enrollees' likelihood of graduating within six years of ninth-grade entry.

Table III.2. Impacts on Five-Year and Six-Year Graduation Rates

					Weighted Sample Size		
Outcome	Accelerated Group (Percentage)	Matched Control Group (Percentage)	Difference (Percentage Points)	*p*-value	Accel.	Matched Control	Total
Five-year graduation	28.9	21.7	7.2*	0.002	732	732	1,464
Six-year graduation	25.2	21.1	4.1*	0.029	967	967	1,934

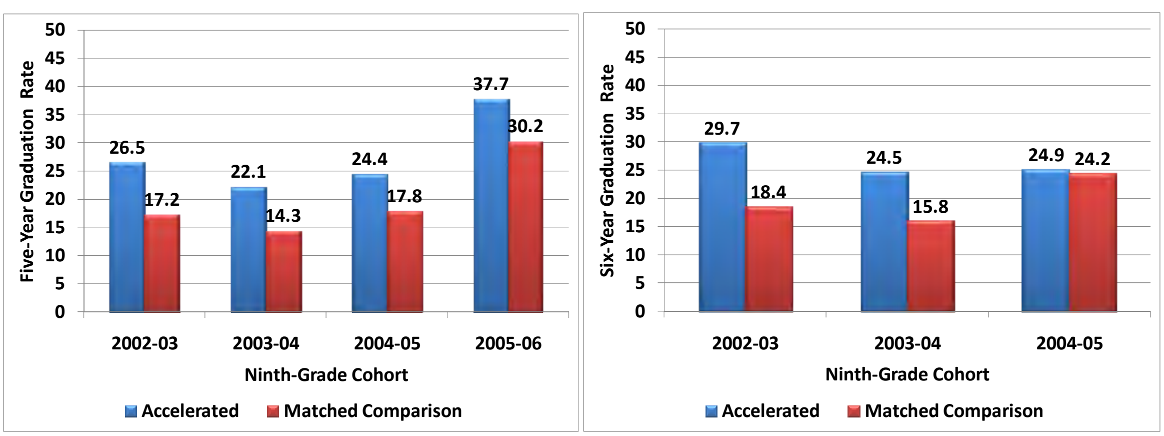
Note: Control group percentages are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

As shown in Table III.2, accelerated students' actual graduation rate in the five-year analysis exceeds that in the six-year analysis. This pattern is due to the fact that the samples for these analyses differ in two respects: (1) the five-year sample includes one additional ninth-grade cohort, the cohort of 2005-06; and (2) the five-year sample excludes students with the longest baseline period, the three-year baseline period, present in the six-year analysis. As shown in Figure III.1, both accelerated and matched comparison students in the ninth-grade cohort of 2005-06 have noticeably

Figure III.1. Graduation Rates of Accelerated and Matched Comparison Students, by Ninth-Grade Cohort

A. Five-Year Graduation **B. Six-Year Graduation**

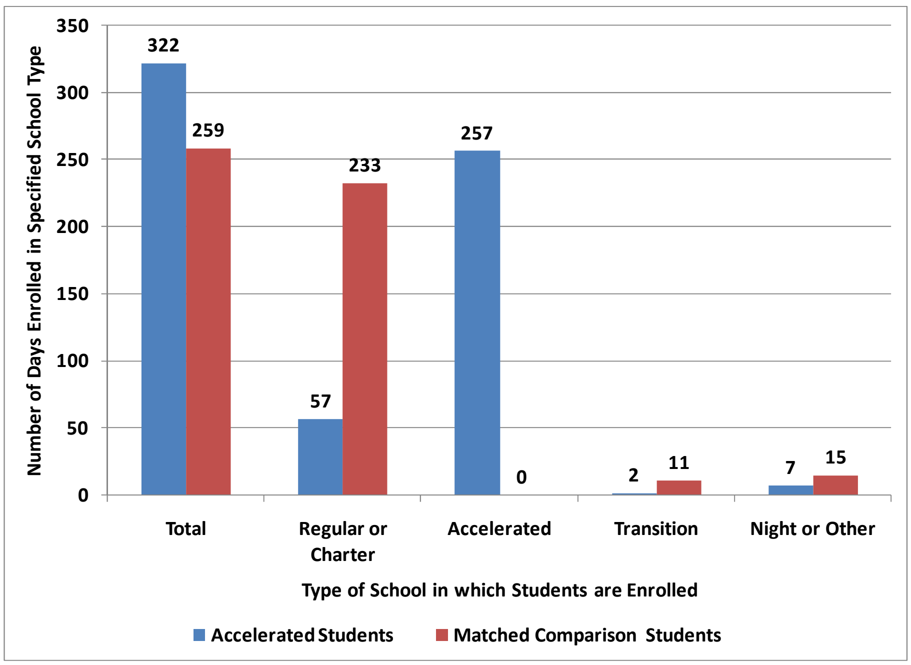


Note: Samples consist only of students whose baseline periods are no longer than two years.

higher graduation rates than those in the older cohorts; when this cohort is excluded, five-year graduation rates are lower than six-year graduation rates, as expected. Within cohorts—in particular, the 2004-05 cohort—treatment-control differences in graduation rates narrow in the sixth year of the follow-up period, as a somewhat higher percentage of comparison students graduate in the sixth year.

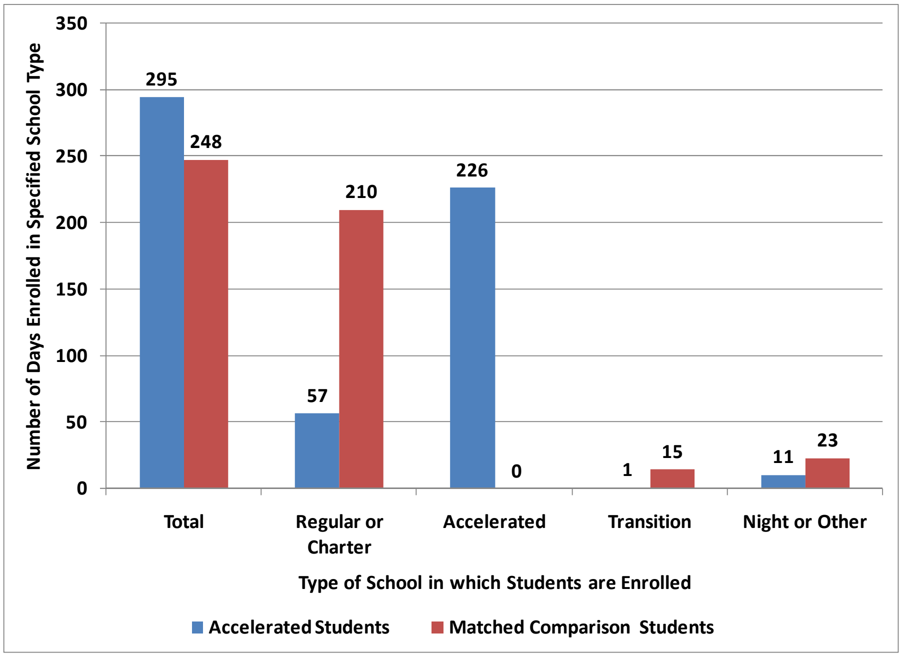
For the sample of accelerated students, the main findings indicate that accelerated schools are more effective at enabling graduation than the school settings in which comparison students are enrolled. To interpret these findings, it is important to identify comparison students' educational settings. Among the matched comparison students in, respectively, the five- and six-year analyses, Figures III.2 and III.3 indicate that the vast majority of their enrolled days in the intervention period occur within nonalternative (regular or charter) schools, rather than transition (disciplinary) or night schools. Thus, the impact estimates can be interpreted as reflecting accelerated students' benefit from enrolling in accelerated schools relative to enrolling in nonalternative schools.

Figure III.2. Average Number of Days Enrolled in Specified Types of Schools During the Intervention Period, Among Students in the Five-Year Graduation Analysis



Note: Regular schools include neighborhood, citywide admission, and magnet schools.

Figure III.3. Average Number of Days Enrolled in Specified Types of Schools During the Intervention Period, Among Students in the Six-Year Graduation Analysis



Note: Regular schools include neighborhood, citywide admission, and magnet schools.

3. Additional Findings

To assess the degree to which the sample definition criteria from Chapter II influence the main impact findings, we generate impact estimates based on alternative samples. We discuss several of these analyses below.

Because SDP terminated the contract of a particular provider, YouFirst Learning, for reasons of convenience in the fall of 2010, samples that exclude YouFirst schools might be more indicative of potential impacts in future years than the full sample that includes YouFirst schools. Table III.3 shows analyses in which the treatment groups exclude students whose first accelerated school is managed by YouFirst. When YouFirst enrollees are excluded, estimated impacts on both five-year graduation (11.1 percentage points) and six-year graduation (6.7 percentage points) are larger than corresponding estimates based on the full sample. These findings indicate—and the provider-specific analyses later in this chapter confirm—that YouFirst schools underperform relative to the other accelerated schools.

We also estimate impacts from several other variants of the main sample. In one alternative sample, the matched comparison students are selected from a potential comparison group that includes students who drop out in the baseline period; the resulting impact estimates (in Appendix Table C.3) are only approximately one-half of a percentage point larger than the main impact estimates (in Table III.2). When comparison students are selected only from students who enroll in the same high schools at ninth-grade entry as their accelerated counterparts, the estimated impacts are either similar to the main estimates or larger by no more than one percentage point (Appendix

Table C.4).[10] We also conduct analyses based only on students with nonimputed eighth-grade scores; the impact estimates do not change in a uniform manner, as we observe larger impacts on five-year graduation rates but smaller impacts on six-year graduation rates in this subgroup than in the full sample (Appendix Table C.5).

Table III.3. Impacts on Five-Year and Six-Year Graduation Rates, Excluding Accelerated Schools Managed by YouFirst Learning

					Weighted Sample Size		
Outcome	Accelerated Group (Percentage)	Matched Control Group (Percentage)	Difference (Percentage Points)	*p*-value	Accel.	Matched Control	Total
Five-year graduation	31.7	20.6	11.1*	0.000	603	603	1,206
Six-year graduation	26.5	19.9	6.7*	0.002	783	783	1,566

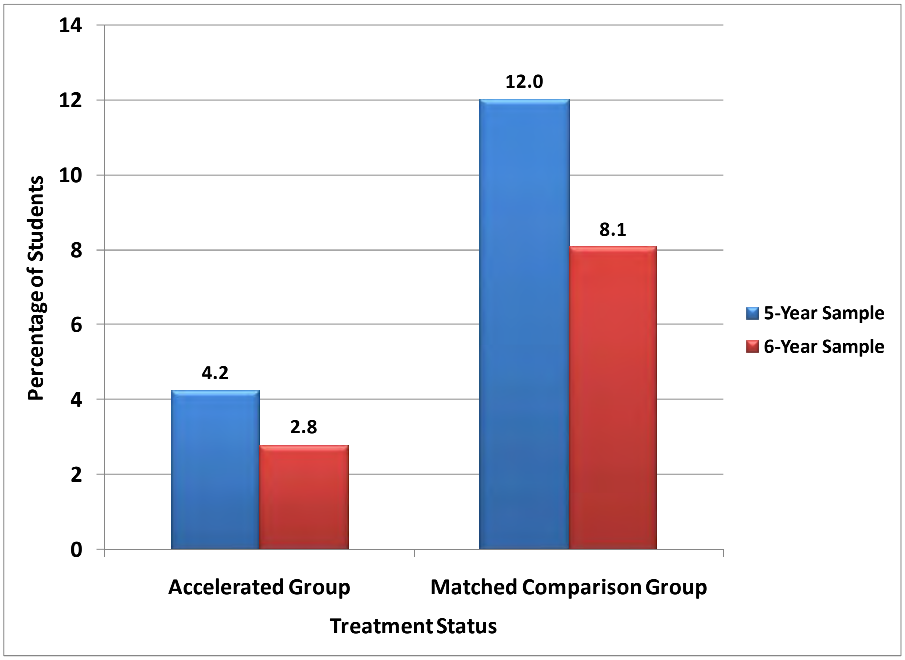
Note: Control group percentages are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

As discussed in Chapter II, the imputation of outcomes for out-transfers is intended to avoid potential estimation biases from excluding out-transfers when the treatment and control groups differ in transfer rates. In the final matched samples for both the five- and six-year graduation analyses, the prevalence of transfer is indeed higher in the matched comparison group than in the treatment group (Figure III.4). Moreover, within each treatment status group, out-transfers differ from other students on several baseline measures (Appendix Table C.6). Because out-transfers are atypical with respect to baseline characteristics, removing a larger number of out-transfers from the control group than from the treatment group would generate baseline imbalance between the remaining members of the two groups. For this reason, impact estimates based on excluding out-transfers from the initially matched samples, shown in Appendix Table C.7, have less causal validity than the main impact estimates; nevertheless, the two sets of estimates are similar in magnitude.

[10] As expected, on a majority of the baseline characteristics, standardized treatment-control differences are somewhat larger when students can only be matched to counterparts within the same initial high school. However, all standardized differences remain well within tolerable ranges; the largest standardized difference is 0.11.

Figure III.4. Percentage of Students in the Final Analysis Samples Who Transfer Out of SDP



B. Average Impacts on Credits Earned

1. Treatment-Control Balance in Baseline Characteristics

As in the analyses of graduation outcomes, the treatment and matched comparison groups in the analysis of credit outcomes are balanced on observable baseline characteristics. In the baseline period, the two groups have similar standardized scores, academic progress, enrollment patterns, attendance rates, and disciplinary records (Table III.4). Among all 18 baseline characteristics, standardized treatment-control differences never exceed 0.1 (Appendix Tables C.8 and C.9). No observable imbalance between the treatment and matched comparison groups threatens the causal validity of the findings that we present next.

Table III.4. Mean Baseline Characteristics of Students in the Analysis of Credits Earned

Variable Measured in the Baseline Period	Accelerated Group	Matched Control Group
8th-grade PSSA math score	-0.45	-0.45
8th-grade PSSA reading score	-0.42	-0.43
High school credits	6.19	6.36
High school GPA	0.71	0.75
Annual enrolled days in high school	154	153
Attendance rate[a]	0.66	0.64
Fraction of days suspended[a]	0.02	0.02
Weighted sample size	**811**	**811**

Note: Control students are weighted by the number of matches in which they are included.

[a] Values are based only on students' time in regular SDP high schools.

2. Main Impact Estimates

Accelerated students earned more academic credits in the 2009-10 school year than similar students outside of accelerated schools (Table III.5). The average number of new credits among accelerated students (4.4 credits) exceeded that among matched comparison students (3.2 credits) by nearly 1.3 credits, a statistically significant difference. Given that matched comparison students were

Table III.5. Impacts on New Credits Earned in the 2009-10 School Year

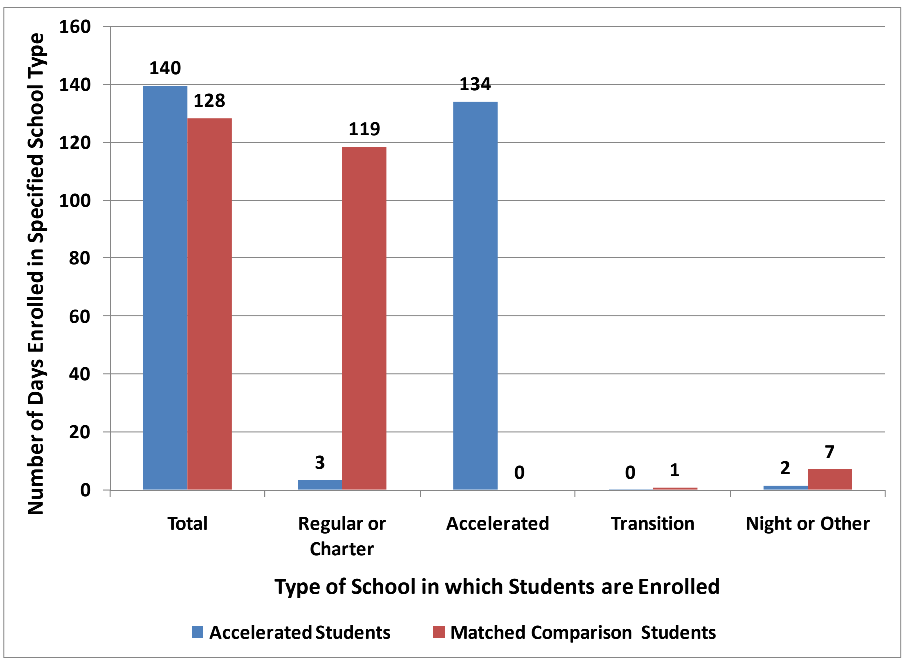
					Weighted Sample Size		
Outcome	Accelerated Group (Mean)	Matched Control Group (Mean)	Difference	*p*-value	Accel.	Matched Control	Total
New credits earned in the 2009-10 school year	4.4	3.2	1.3*	0.000	811	811	1,622

Note: Control group means are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

predominantly enrolled in nonalternative (regular or charter) schools during the 2009-10 school year (Figure III.5), the findings indicate that accelerated schools enable their enrollees to accumulate credits more quickly than they otherwise would have done in nonalternative settings.

Figure III.5. Average Number of Days Enrolled in Specified Types of Schools During the 2009-10 School Year, Among Students in the Analysis of Credits Earned



Note: Regular schools include neighborhood, citywide admission, and magnet schools.

3. Additional Findings

Consistent with the findings for graduation outcomes, estimated impacts on credit-earning rates are larger when enrollees of YouFirst schools are excluded from the analyses (Table III.6). We also estimate impacts from alternative samples that (1) include control students who drop out in the baseline period; (2) originate from matching treatment and control students who share the same initial high school; (3) include only students with nonmissing eighth-grade scores; or (4) include only students who do not transfer out of SDP during the 2009-10 school year. In all of these samples, estimated impacts are statistically significant and represent increments of at least one academic credit (Appendix Table C.10).

Table III.6. Impacts on New Credits Earned in the 2009-10 School Year, Excluding Accelerated Schools Managed by YouFirst Learning

					Weighted Sample Size		
Outcome	Accelerated Group (Mean)	Matched Control Group (Mean)	Difference	*p*-value	Accel.	Matched Control	Total
New credits earned in the 2009-10 school year	5.2	3.0	2.2*	0.000	654	654	1,308

Note: Control group means are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

C. Impacts of Individual Providers

Given that several different providers offer accelerated education in SDP, there might be variation among providers in their impacts on enrollees—variation that would not be reflected in focusing exclusively on the average impacts of the entire accelerated education system. To document this possible variation, we estimate impacts of individual providers on graduation and credits earned.

In these provider-specific analyses, we assign each accelerated student to the provider of the first accelerated school in which the student enrolls. This approach is consistent with a clear empirical pattern: nearly all accelerated students enroll in at most one accelerated school, and nearly all enrollment time spent by accelerated students in accelerated education occurs in their first accelerated school (Table III.7). Thus, it is appropriate that the treatment group for each provider-specific analysis consists of students whose first enrollment in accelerated education occurs in the provider's schools.

Importantly, smaller sample sizes in the provider-specific analyses relative to the full-sample analyses are reflected in two ways: (1) less precision and (2) more prevalent imbalance between the treatment and matched comparison groups. The latter issue merits particular attention because it can pose a threat to the causal validity of the impact estimates. In general, with smaller sample sizes of accelerated students, it becomes more difficult to identify the same (small) number of comparison students who happen to share the unique set of characteristics exhibited by the accelerated group, especially when attempting to balance a large number of characteristics.

Table III.7. Enrollment Patterns of Accelerated Students After Entering Their First Accelerated School

Enrollment Pattern	Accelerated Students in 5-Year Graduation Analysis	Accelerated Students in 6-Year Graduation Analysis	Accelerated Students in Credit Accumulation Analysis
Percentage of students who enroll in:			
Exactly one accelerated school	93.7	94.0	95.6
More than one accelerated school	6.3	6.0	4.4
Average number of enrolled days in:			
First accelerated school	249	218	130
All accelerated schools	257	226	134
Sample size	**732**	**967**	**811**

To reduce the possibility of relying on spurious impact findings, we focus only on provider-specific analyses in which no more than one of the 18 covariates is out of balance—that is, no more than one covariate exhibits a treatment-control standardized difference of 0.25 or more. For each provider-specific analysis of, respectively, graduation outcomes and credit-earning rates, Tables III.8 and III.9 tabulate the number of covariates for which the treatment-control standardized difference falls in a specified range. In the graduation analyses, only one provider-specific analysis, that of OIC of America's impact on five-year graduation rates, is based on a final analysis sample in which more than one covariate is out of balance (Table III.8). Due to smaller sample sizes per provider, treatment-control balance is less prevalent in the credits analyses than in the graduation analyses (Table III.9). In the credits analyses, we do not have confidence in the causal validity of the impact estimates for Big Picture and OIC of America.[11] Beyond these problematic estimates, we regard the remaining provider-specific estimates, discussed next, as informative of the providers' effectiveness.

Table III.8. Balance Between Accelerated and Matched Comparison Students in the Analysis of Graduation Outcomes, by Provider

	Outcome: Five-Year Graduation			Outcome: Six-Year Graduation		
	Number of Covariates for Which the Absolute Value of the Treatment-Control Standardized Difference Is			Number of Covariates for Which the Absolute Value of the Treatment-Control Standardized Difference Is		
Name of Provider	Less Than 0.10	At Least 0.10 and Less Than 0.25	At Least 0.25	Less Than 0.10	At Least 0.10 and Less Than 0.25	At Least 0.25
Camelot	18	0	0	18	0	0
OIC of America	12	4	2	12	6	0
One Bright Ray	14	4	0	17	1	0
YouFirst Learning	13	5	0	11	6	1

[11] Additionally, in the credits analysis pertaining to One Bright Ray, a majority of the covariates exhibit treatment-control standardized differences of at least 0.10. Only one of these differences reaches the threshold—0.25—for indicating a substantively important contrast, so there is sufficient covariate balance to report the impact estimates from this analysis. Nevertheless, the other provider-specific credit analyses (except those for Big Picture and OIC of America) are based on samples with better covariate balance and thus merit greater confidence than the analysis pertaining to One Bright Ray.

Table III.9. Balance between Accelerated and Matched Comparison Students in the Analysis of Credits Earned, by Provider

	Number of Covariates for Which the Absolute Value of the Treatment-Control Standardized Difference Is		
Name of Provider	Less Than 0.10	At Least 0.10 and Less Than 0.25	At Least 0.25
Big Picture	8	6	4
Camelot	18	0	0
Communities in Schools	13	4	1
OIC of America	6	8	4
Ombudsman	13	4	1
One Bright Ray	4	13	1
YouFirst Learning	13	5	0

Table III.10 shows the impacts of each provider on five- and six-year graduation rates. Four of the seven providers are in operation during the time in which sample members from the graduation analyses are entering accelerated schools. Of these four providers, three have impact estimates that are positive in sign, but only Camelot raises both five- and six-year graduation rates by a statistically significant increment. YouFirst Learning, the clear underperformer, is observed to have negative impacts on graduation, one of which is statistically significant.

Table III.10. Impacts on Five-Year and Six-Year Graduation Rates, by Provider

	Outcome: Five-Year Graduation		Outcome: Six-Year Graduation	
Name of Provider	Number of Accelerated Students	Impact in Percentage Points	Number of Accelerated Students	Impact in Percentage Points
Camelot	342	11.9* (0.000)	414	6.4* (0.029)
OIC of America	67	Nonequiv.	121	9.0 (0.083)
One Bright Ray	194	10.1* (0.025)	248	6.7 (0.083)
YouFirst Learning	129	-13.8* (0.012)	184	-7.2 (0.101)

Note: p-values are in parentheses. "Nonequiv." denotes that the treatment and matched control groups exhibit lack of equivalence on more than one baseline covariate; the impact estimate is thus not shown.

*Significantly different from zero at the .05 level, two-tailed test.

Table III.11 presents the impact of each provider on the credits earned by its enrollees in the 2009-10 school year. Camelot clearly raises its enrollees' rate of credit accumulation above the rate they would have achieved in nonalternative settings; in contrast, enrolling in schools managed by YouFirst Learning reduces the rate of credit accumulation. Thus, the provider-specific impacts on credits earned are generally consistent with corresponding impacts on graduation outcomes.[12]

Table III.11. Impacts on New Credits Earned in the 2009-10 School Year, by Provider

Name of Provider	Number of Accelerated Students	Impact
Big Picture	41	Nonequiv.
Camelot	307	3.2* (0.000)
Communities in Schools	88	0.8 (0.144)
OIC of America	52	Nonequiv.
Ombudsman	46	0.3 (0.711)
One Bright Ray	86	1.4 (0.078)
YouFirst Learning	157	-2.2* (0.000)

Note: p-values are in parentheses. "Nonequiv." denotes that the treatment and matched control groups exhibit lack of equivalence on more than one baseline covariate; the impact estimate is thus not shown.

*Significantly different from zero at the .05 level, two-tailed test.

Although these analyses reveal differences among providers in how effectively they educate the particular sets of enrollees who enter their schools, these analyses do not provide a direct comparison of the impacts that different providers would have on the *same* set of students. The reason is that the characteristics of enrollees differ somewhat across providers, as shown in Table III.12. For instance, among the seven providers, the average number of prior credits with which enrollees begin their accelerated education ranges from 3.6 (in schools managed by One Bright Ray) to 7.4 (in schools managed by OIC of America). Eighth-grade PSSA achievement of enrollees also varies across providers; whereas students entering Camelot and Ombudsman schools scored 0.38 standard deviations below the district average on the eighth-grade PSSA math test, enrollees of programs managed by Communities in Schools scored 0.71 standard deviations below the average. Because of these and other background differences among providers' sets of enrollees, our findings do not directly address the question of whether an accelerated student would derive greater benefit from enrolling with one particular provider rather than another. Nevertheless, the findings from Tables III.10 and III.11 provide suggestive—but not definitive—indications of possible differences in instructional quality across providers.

[12] Appendix Tables C.11 and C.12 show provider-specific means of the outcome variables for accelerated students in the graduation analyses and credits analyses, respectively.

Table III.12. Mean Baseline Characteristics Among Accelerated Students in the Analysis of Credits Earned, by Provider

	Variable Mean Among Students who Enrolled with Indicated Provider						
Variable Measured in the Baseline Period	Big Picture	Camelot	Commun. in Schools	OIC of America	Ombuds.	One Bright Ray	YouFirst
8th grade PSSA math score	-0.49	-0.38	-0.71	-0.41	-0.38	-0.50	-0.40
8th grade PSSA reading score	-0.69	-0.35	-0.81	-0.43	-0.48	-0.42	-0.31
High school credits	4.98	7.20	4.64	7.39	6.63	3.61	6.00
High school GPA	1.01	0.77	0.51	0.75	0.55	0.47	0.79
Annual enrolled days in high school	151	161	168	138	167	135	144
Attendance rate[a]	0.62	0.70	0.66	0.63	0.67	0.60	0.64
Fraction of days suspended[a]	0.02	0.02	0.02	0.02	0.02	0.01	0.01
Sample size	**41**	**307**	**88**	**52**	**46**	**86**	**157**

[a] Values are based only on students' time in regular SDP high schools.

IV. CONCLUSION AND IMPLICATIONS

The evidence from this report supports the conclusion that accelerated schools have raised graduation rates among students in SDP who are at high risk of dropout. Our analyses compare two groups: (1) students who enroll in accelerated schools at some point during high school; and (2) students who never enroll in accelerated schools but who have similar prior achievement, academic progress, enrollment patterns, and attendance in the years before their counterparts begin their accelerated education. Outcomes in the second group represent those that accelerated students would have had if they had not enrolled in accelerated schools—in particular, if they had remained in nonalternative schools. For outcomes to be meaningfully compared between the two groups, our analyses rely on the key assumption that both groups are subject to similar standards for earning academic credits and attaining high school diplomas. On the basis of this comparison, we find that the five- and six-year graduation rates of accelerated students are higher by 7 and 4 percentage points, respectively, than they otherwise would have been in the absence of accelerated education.

Our findings on rates of credit accumulation are consistent with our findings on graduation rates: accelerated schools demonstrate positive impacts on the number of new credits that their students earned in the 2009-10 school year. Students who entered accelerated schools at the beginning of that year earned, on average, 1.3 more credits than the comparison students, most of whom were in nonalternative schools at the start of the year. Taken together, the findings on graduation and credit accumulation indicate that accelerated schools have benefited the academic outcomes of both earlier and more recent entrants into their programs.

In determining how these findings can inform the direction of the district's dropout reduction strategies, policymakers will likely need to consider the size of the observed impacts—in particular, the degree to which the impact magnitudes exceed or fall short of potential impacts from other possible strategies. Various ways of benchmarking the impact sizes can yield different interpretations of their substantive importance. Given that only slightly more than one in five comparison students graduates, impacts of 7 and 4 percentage points on five- and six-year graduation rates, respectively, represent proportionally sizable gains amounting to 33 and 19 percent of the comparison group graduation rate. On the other hand, six-year graduation rates among all students in SDP are much higher, at 63 percent (Socolar 2010). Accelerated schools' impact on six-year graduation rates only modestly narrows—by about 10 percent—the gap between districtwide graduation rates and those of the study's at-risk comparison group. Similarly, an increase of 1.3 credits in the annual number of new credits represents an impressive 40 percent rise from the comparison group's credit-earning rate; however, because students enter accelerated schools with an average of only 6 credits, their observed credit-earning rate of 4.4 credits per year still falls short of the pace needed to reach a total of 23.5 credits within three years.

Beyond estimating the average impact of all accelerated schools, we have also documented that impacts vary across providers. A strict interpretation of this evidence would conclude only that certain providers have more beneficial impacts on their particular sets of enrollees than other providers do on their particular sets of enrollees. Our analysis is not structured to determine whether certain providers would be more effective than others at educating the same set of students. Nevertheless, our findings are at least suggestive that instructional quality might vary across providers. A closer examination of the different instructional models used by the various providers and their links to student outcomes could be a promising direction for further analysis to inform the improvement of accelerated education.

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APPENDIX A

SAMPLE CONDITIONS AND RESULTING SAMPLE SIZES

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Table A.1. Number of Students Meeting Criteria for Inclusion in the Treatment Group and Potential Comparison Group: Graduation Analyses

	Number of Students in	
Criterion	5-Year Graduation Sample	6-Year Graduation Sample
Accelerated Students		
In the relevant ninth-grade cohorts; was a ninth grader in a regular SDP school	2,855	1,832
Of whom: In the relevant baseline groups	777	1,075
Of whom: Last school in baseline period is a regular SDP school	732	970
Of whom: All baseline covariates are nonmissing[a]	732	967
Final Sample Size in Treatment Group	**732**	**967**
Nonaccelerated Students		
In the relevant ninth-grade cohorts; was a ninth grader in a regular SDP school	58,319	44,143
Of whom: In the relevant baseline groups[b]	54,263	41,103
Of whom: Last school in baseline period is a regular SDP school	53,308	40,676
Of whom: All baseline covariates are nonmissing[a]	52,177	39,628
Of whom: Enrolled in SDP at some point in the first year of the intervention period	48,883	37,318
Final Sample Size in Potential Comparison Group	**48,883**	**37,318**

[a] Imputed values of the eighth-grade scores are not classified as missing values.

[b] Nonaccelerated students who belong to more than one baseline group are counted only once.

Table A.2. Number of Students Meeting Criteria for Inclusion in the Treatment Group and Potential Comparison Group: Credit-Earning Analyses

	Number of Students
Accelerated Students	
In an accelerated school in the 2009-10 school year; was a ninth grader in a regular SDP school	2,642
Of whom: First-time entrant into accelerated schools in the 2009-10 school year	1,854
Of whom: In the relevant baseline groups (implies the cohorts included)	898
Of whom: Last school in baseline period is a regular SDP school	812
Of whom: All baseline covariates are nonmissing[a]	811
Final Sample Size in Treatment Group	**811**
Nonaccelerated Students	
In the relevant ninth-grade cohorts; was a ninth grader in a regular SDP school	55,089
Of whom: In the relevant baseline groups[b]	41,377
Of whom: Last school in baseline period is a regular SDP school	39,005
Of whom: All baseline covariates are nonmissing[a]	38,076
Of whom: Enrolled in SDP at some point in the 2009-10 school year	31,916
Final Sample Size in Potential Comparison Group	**31,916**

[a] Imputed values of the eighth-grade scores are not classified as missing values.

[b] Nonaccelerated students who belong to more than one baseline group are counted only once.

APPENDIX B

ANALYTIC METHODS

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This appendix provides details on our empirical methods for estimating the impacts of accelerated schools on graduation and credit accumulation. In particular, we describe our methods for (1) propensity score estimation and matching on the basis of the estimated propensity scores; (2) imputation of outcomes for students with missing outcome data; (3) estimation of impacts and standard errors; and (4) imputation of eighth-grade test scores for students with missing eighth-grade data. Unless otherwise noted, all of these procedures are applied both to estimating the average impacts of all accelerated schools and to estimating the impacts of specific program providers.

A. Propensity Score Matching

To construct a matched comparison group for the sample of accelerated students, the two key analytic steps consist of (1) estimating each student's propensity score and (2) matching each accelerated student with the nonaccelerated student who has the most similar estimated propensity score. Next, we describe each of these steps.

We first stratify the samples of accelerated and potential comparison students into baseline groups; propensity score estimation and matching are conducted separately by baseline group. As discussed in Chapter II, potential comparison students can belong to more than one baseline group. However, to enable the final matched samples to be statistically independent across baseline groups, we impose the restriction that any nonaccelerated student selected for a match in a specified baseline group is ineligible for matching within all other baseline groups; this restriction leads to feasible calculation of the standard errors of the final impact estimates, as we describe later.

Within each baseline group, we use logit regression to estimate the propensity scores of the sample members. Specifically, let A_i be a dummy variable for whether student *i* is an accelerated student. Using the full sample of accelerated and potential comparison students within the specified baseline group, we estimate the following logit model:

(1) $$\Pr(A_i = 1) = \Lambda(\mathbf{X}_i\boldsymbol{\beta} + \mathbf{Z}_i\boldsymbol{\delta})$$

where $\mathbf{X}$ is a vector of all covariates discussed in Chapter II (including, in addition, dummies for ninth-grade cohorts); $\mathbf{Z}$ is a vector of selected interaction terms involving various elements of $\mathbf{X}$; and $\Lambda(\cdot)$ is the logit function.

Including the interaction terms, $\mathbf{Z}$, enhances the flexibility of the propensity score model and, hence, the likelihood that students with similar estimated propensity scores will have similar values of $\mathbf{X}$. However, given that $\mathbf{X}$ has 18 variables plus the cohort dummies, it would be infeasible to include a full set of interactions involving all possible pairs of covariates. Therefore, our approach is to prioritize inclusion of interaction terms that involve selected subsets of key covariates and that exhibit sufficiently strong association with A_i. Because it is particularly important to ensure that measures of baseline academic progress are ultimately balanced between accelerated and matched comparison students, we consider only interaction terms in which at least one of the variables is indicative of baseline academic progress. Specifically, we sequentially consider each of the 18 original covariates and, for each covariate, we sequentially consider interactions between the specified covariate and each of the following high-priority variables: high school credits, high school GPA, eighth-grade math score, eighth-grade reading score, and enrolled days per year since ninth-grade entry.[13] Following the approach used by previous studies (see, for example, Tuttle et al. 2010), we iteratively include each interaction term into Equation (1), obtain an updated estimate of Equation (1), and test the null hypothesis that the newly included interaction term has no association with A_i. If the newly included interaction term is statistically significant at the 0.2 level, then it is retained; otherwise, it is dropped from Equation (1).

Because comparison students who are ultimately selected for matches should have covariate values that are similar to those of their matched accelerated counterparts, it is especially important that the estimated coefficients from Equation (1) should be valid for the subpopulation of students who are similar to accelerated students at baseline. However, when Equation (1) is estimated on the full sample, the estimation sample includes nonaccelerated students—including students attending selective high schools—whose values of $\mathbf{X}$ are very different from those of accelerated students and, thus, who have no possibility of being included in a match; therefore, segments of the covariate distributions that are irrelevant to the subpopulation of interest might influence the initial estimates of Equation (1).

We therefore conduct a second round of propensity score estimation using a trimmed sample that excludes nonaccelerated students who are very different from all accelerated students. To identify the trimmed sample, we rank all students in the full sample on the basis of their estimated propensity scores from the first round; within the distribution of the first-round propensity scores, we identify the percentile of the accelerated student who has the lowest propensity score. We then remove all nonaccelerated students whose percentiles are less than nine-tenths of the percentile of the lowest-propensity accelerated student. Using the trimmed sample, we then repeat the iterative procedure for determining the interaction terms to be included in the propensity score model, and we reestimate Equation (1) to obtain a final propensity score estimate for every student in the trimmed sample.

Within the specified baseline group, we then conduct matching separately within each ninth-grade cohort that is represented in the baseline group. We implement nearest-neighbor matching with replacement, such that each accelerated student is matched to the single nonaccelerated student in the same ninth-grade cohort who has the most proximate value for the estimated propensity score. Within a baseline group, nonaccelerated students can be included in more than one matched pair.

The preceding methods for propensity score estimation and matching are used for the analysis of both average and provider-specific impacts; in particular, the entire procedure is repeated for every provider whose impact is estimated. When a provider-specific impact is the focus, only the accelerated students whose first accelerated school is managed by the specified provider are included in the sample used for the propensity score estimation.

B. Imputation Strategy for Missing Outcomes

In the matched sample resulting from the propensity score matching procedures, some students will have missing outcome data due, primarily, to having transferred out of the district before

[13] A variable in the high-priority set of covariates can be interacted with itself, which is equivalent to squaring the variable.

outcome measurement. To avoid potential estimation biases from excluding students with missing outcome data (see Chapter II), we impute the outcomes of these students and include them in the subsequent impact estimation.

Our basic imputation strategy is to replace missing outcomes with regression-predicted outcomes based on students with complete data in the same treatment status group. Within each baseline group, we use students with complete data to estimate the following regression model for the outcome, y_i, of student *i* separately by treatment status:

$$(2) \quad y_i = \mathbf{X}_i \boldsymbol{\gamma} + \varepsilon_i$$

where $\mathbf{X}$ is the same vector of 18 covariates and cohort dummies discussed earlier. Equation (2) is estimated with ordinary least squares if the outcome is credits earned; it is estimated with logit if the outcome is a dichotomous indicator for graduation. For every student *i* in the baseline group, we then obtain a predicted outcome in the treatment condition, $\hat{\mu}_1(\mathbf{X}_i)$, from estimation of Equation (2) on the sample of accelerated students; we also obtain a predicted outcome in the counterfactual condition, $\hat{\mu}_0(\mathbf{X}_i)$, from estimation of Equation (2) on the sample of nonaccelerated students. We then replace missing outcomes among accelerated students with $\hat{\mu}_1(\mathbf{X}_i)$, and we replace missing outcomes among matched comparison students with $\hat{\mu}_0(\mathbf{X}_i)$.

When estimating Equation (2) on nonaccelerated students, the set of nonaccelerated students used in the estimation must be carefully selected. The key consideration is that the coefficient estimates must be valid for nonaccelerated students who are similar to accelerated students at baseline, because only the comparison students selected for matches will be assigned predicted values from these coefficient estimates. Although one option would be to estimate Equation (2) only on the sample of *matched* comparison students, small sample sizes for the matched samples—especially in the provider-specific analyses—would lead to imprecise coefficient estimates. On the other hand, using all potential comparison students in the trimmed sample—the sample used in the second-round estimation of the propensity scores—might still lead to an estimation sample that included nonaccelerated students who were fairly dissimilar with accelerated students. We therefore apply further trimming to the trimmed sample: we remove nonaccelerated students whose percentiles within the distribution of *final* propensity scores are less than nine-tenths of the percentile of the accelerated student with the lowest final propensity score. The twice-trimmed sample of potential comparison students is that which is used to estimate Equation (2) for the counterfactual condition.[14]

C. Estimation of Impacts and Standard Errors

Using the matched samples, we estimate impacts separately within each baseline group and then average the impacts from all baseline groups into an overall impact. Because our estimation

[14] When conducting provider-specific analyses, the twice-trimmed sample of nonaccelerated students varies across providers because each provider-specific analysis produces a different set of estimated propensity scores. However, when estimating Equation (2) on accelerated students, we chose to use a fixed sample of accelerated students—namely, the full sample of all accelerated students in the specified baseline group—regardless of the provider being considered. This approach avoids estimating Equation (2) on very small samples of accelerated students in the provider-specific analyses.

approach is the same regardless of the outcome, we next discuss the estimation methods using a generic outcome variable, *y*.

Within each baseline group, the primary step in obtaining an impact estimate is to adjust the raw difference in outcomes between accelerated and matched comparison students for any remaining difference in covariate values. For each student *j* in the accelerated group, let *k(j)* index the comparison student to whom student *j* is matched. Starting from the comparison student's actual outcome, $y_{k(j)}$, we calculate an adjusted outcome for the comparison student as $y_{k(j)} + \hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_{k(j)})$, where $\hat{\mu}_0(\cdot)$ is defined above (Abadie and Imbens, forthcoming). The adjusted outcome for student *k(j)* is an estimate for the outcome that this student would have exhibited if he or she had possessed the same covariate values as student *j*.

Given the adjustment, the impact estimate in baseline group *b*, $\hat{\beta}_b$, is the average difference between accelerated students' actual outcomes and the adjusted outcomes of the matched comparison students:

$$(3) \quad \hat{\beta}_b = \frac{1}{N_b}\sum_{j=1}^{N_b}\left[y_j - \left(y_{k(j)} + \hat{\mu}_0(\mathbf{X}_j) - \hat{\mu}_0(\mathbf{X}_{k(j)})\right)\right],$$

where N_b is the number of accelerated students in baseline group *b*. We use the approach specified by Abadie and Imbens (2006) to calculate the variance of $\hat{\beta}_b$—denoted by $\hat{V}_b$—but we use only students with nonimputed outcomes in the variance calculation to prevent the imputed data from spuriously inflating the sample size and apparent precision of the impact estimates.[15]

After obtaining an impact estimate in each baseline group, we calculate the overall impact estimate, $\hat{\beta}_{total}$, as a weighted average of the impact estimates from all of the baseline groups, with weights equal to the number of accelerated students in the baseline groups:

$$(4) \quad \hat{\beta}_{total} = (\sum_{b=1}^{B} N_b\hat{\beta}_b)/(\sum_{b=1}^{B} N_b),$$

[15] Specifically, for each student *i* in the matched samples who has a nonimputed outcome, we identify the two other students, $m_1(i)$ and $m_2(i)$, in the same ninth-grade cohort and treatment status group who have the most proximate values for the final estimated propensity score and who have nonimputed outcomes; the students $m_1(i)$ and $m_2(i)$ must belong to the accelerated sample or trimmed sample of potential comparison students, but, in the latter case, need not necessarily belong to the matched sample of comparison students. We calculate the variance of outcomes, $\hat{\sigma}_i^2$, in the trio $\{i, m_1(i), m_2(i)\}$. The estimated variance of $\hat{\beta}_b$ is the weighted sum of $\hat{\sigma}_i^2$ across all students *i* in the matched samples who have nonimputed outcomes, with weights equal to $(M_i / N_{g(i)})^2$, where M_i is the number of matched pairs in which student *i* is included and $N_{g(i)}$ is the number of students in the matched samples with the same treatment status as student *i* and with nonimputed outcomes.

where B is the number of baseline groups. Accordingly, the variance of the overall impact estimate, $\hat{V}_{total}$, is calculated as

$$(5) \quad \hat{V}_{total} = (\sum_{b=1}^{B} N_b^2 \hat{V}_b)/(\sum_{b=1}^{B} N_b)^2,$$

of which the square root is the standard error of the overall impact estimate.

D. Imputation Strategy for Eighth-Grade Test Scores

In the empirical methods described earlier for propensity score matching, imputation of outcomes, and regression adjustment of impact estimates, the covariate sets used in each of these steps include eighth-grade PSSA scores in reading and math. However, as discussed in Chapter II, about 20 percent of students in the initial samples used for propensity score estimation have missing records for eighth-grade scores. In order to include these students in propensity score estimation and all subsequent estimation steps, we impute their eighth-grade test scores. Because eighth-grade scores must be imputed prior to all of the empirical steps previously described, we implement the imputation strategy once—at the outset of the empirical analysis—for ninth-grade cohorts relevant to the graduation analyses, and once for ninth-grade cohorts relevant to the credits analyses.

Separately by treatment status, we use single stochastic regression imputation to generate imputed eighth-grade test scores for students without actual values of these scores. Within each treatment status group, we estimate a regression of the eighth-grade score in a given subject (reading or math) on all of the covariates discussed in Chapter II—with the exception of the eighth-grade test score variables—using the sample of students who have nonmissing values for all variables. Because students from several baseline groups are pooled in the sample used for this regression, we define the covariates on the basis of the first year of high school, the only high school year included in every student's baseline period.[16] We then (1) generate predicted values from the regression; (2) add a stochastic component to the predicted values in order to equate the variances of actual and predicted scores; and (3) substitute the stochastically adjusted predictions for missing values of the eighth-grade test scores.

[16] The covariate set in these regressions also includes cohort dummies and—when the sample pertains to the graduation analysis—two dichotomous indicators for, respectively, graduating and transferring out of SDP within four years of ninth grade. Following standard practice in the imputation literature (see, for example, Puma et al. 2009), we include the endogenous outcome variables in the covariate set in order to improve the accuracy of the imputation.

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APPENDIX C

SUPPLEMENTARY RESULTS

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Table C.1. Baseline Characteristics of Accelerated and Matched Comparison Students in the Analysis of Five-Year Graduation Rates

	Group Means				Overall Standardized Difference Between Accelerated and Control Students
	1-Year Baseline Group		2-Year Baseline Group		
Variable Measured in the Baseline Period	Accel. Group	Matched Control Group	Accel. Group	Matched Control Group	
8th-grade PSSA math score	-0.52 (0.70)	-0.50 (0.65)	-0.35 (0.74)	-0.30 (0.74)	-0.04
8th-grade PSSA reading score	-0.46 (0.81)	-0.43 (0.84)	-0.32 (0.86)	-0.29 (0.88)	-0.04
8th-grade PSSA scores are imputed	0.23 (0.42)	0.20 (0.40)	0.16 (0.37)	0.17 (0.38)	0.03
High school credits	2.55 (2.55)	2.60 (2.61)	4.96 (4.11)	4.88 (4.08)	0.00
High school GPA	0.55 (0.78)	0.57 (0.82)	0.58 (0.79)	0.56 (0.75)	0.00
Age at ninth-grade entry	15.47 (0.71)	15.46 (0.85)	15.13 (0.60)	15.14 (0.70)	0.01
Enrolled days per year after ninth grade entry	166 (34)	165 (36)	162 (32)	165 (28)	-0.03
Fraction of enrolled high school days in specified school type:					
Neighborhood	0.96 (0.19)	0.96 (0.19)	0.94 (0.19)	0.94 (0.19)	0.00
Magnet/citywide	0.03 (0.18)	0.03 (0.17)	0.05 (0.18)	0.05 (0.18)	0.00
Disciplinary/transition	0.00 (0.04)	0.00 (0.04)	0.00 (0.03)	0.00 (0.00)	0.06
Fraction of enrolled high school days:[a]					
In attendance	0.63 (0.23)	0.61 (0.24)	0.62 (0.19)	0.62 (0.20)	0.04
Suspended	0.02 (0.04)	0.03 (0.04)	0.02 (0.02)	0.02 (0.03)	-0.05
Female	0.44 (0.50)	0.42 (0.49)	0.53 (0.50)	0.51 (0.50)	0.05
Black	0.60 (0.49)	0.61 (0.49)	0.63 (0.48)	0.62 (0.49)	0.00
Hispanic	0.28 (0.45)	0.25 (0.44)	0.25 (0.43)	0.23 (0.42)	0.06
Ever designated with disability	0.19 (0.39)	0.19 (0.39)	0.14 (0.35)	0.14 (0.35)	0.01
Ever Limited English Proficient	0.08 (0.27)	0.08 (0.28)	0.05 (0.22)	0.07 (0.26)	-0.03
Ever received TANF	0.67 (0.47)	0.68 (0.47)	0.71 (0.45)	0.67 (0.47)	0.02
Weighted Sample Size	**446**	**446**	**286**	**286**	

Note: Standard deviations are in parentheses. The overall standardized difference is the weighted average of treatment-control standardized differences within the baseline groups, with weights equal to the number of accelerated students. Within each baseline group, the treatment-control standardized difference is the raw difference divided by the square root of the average of the treatment and control variances. Control students are weighted by the number of matches in which they are included.

[a] Values are based only on students' time in regular SDP high schools.

Table C.2. Baseline Characteristics of Accelerated and Matched Comparison Students in the Analysis of Six-Year Graduation Rates

	Group Means						Overall Standardized Difference Between Accelerated and Control Students
	1-Year Baseline Group		2-Year Baseline Group		3-Year Baseline Group		
Variable Measured in the Baseline Period	Accel. Group	Matched Control Group	Accel. Group	Matched Control Group	Accel. Group	Matched Control Group	
8th-grade PSSA math score	-0.52 (0.70)	-0.57 (0.68)	-0.43 (0.74)	-0.39 (0.76)	-0.41 (0.74)	-0.33 (0.90)	-0.02
8th-grade PSSA reading score	-0.48 (0.81)	-0.57 (0.87)	-0.42 (0.89)	-0.37 (0.87)	-0.37 (0.91)	-0.31 (1.01)	0.00
8th-grade PSSA scores are imputed	0.21 (0.41)	0.22 (0.41)	0.19 (0.39)	0.18 (0.38)	0.14 (0.34)	0.15 (0.36)	0.00
High school credits	2.16 (2.10)	1.95 (2.08)	4.55 (3.70)	4.59 (3.68)	7.70 (4.83)	7.72 (4.96)	0.03
High school GPA	0.41 (0.64)	0.30 (0.53)	0.48 (0.67)	0.49 (0.67)	0.71 (0.81)	0.73 (0.78)	0.04
Age at ninth-grade entry	15.34 (0.70)	15.26 (0.87)	14.98 (0.62)	14.98 (0.71)	14.78 (0.59)	14.75 (0.59)	0.04
Enrolled days per year after ninth-grade entry	168 (34)	166 (33)	159 (37)	162 (36)	154 (34)	149 (38)	0.01
Fraction of enrolled high school days in specified school type:							
Neighborhood	0.95 (0.19)	0.96 (0.17)	0.93 (0.22)	0.92 (0.23)	0.89 (0.26)	0.85 (0.31)	0.03
Magnet/ citywide	0.04 (0.18)	0.03 (0.16)	0.06 (0.21)	0.07 (0.22)	0.08 (0.24)	0.12 (0.29)	-0.05
Disciplinary/ transition	0.00 (0.02)	0.00 (0.02)	0.00 (0.03)	0.00 (0.03)	0.01 (0.06)	0.02 (0.10)	0.00
Fraction of enrolled high school days:[a]							
In attendance	0.62 (0.23)	0.59 (0.24)	0.59 (0.20)	0.60 (0.20)	0.63 (0.17)	0.62 (0.18)	0.04
Suspended	0.03 (0.04)	0.03 (0.04)	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)	0.02 (0.03)	-0.02
Female	0.40 (0.49)	0.40 (0.49)	0.49 (0.50)	0.50 (0.50)	0.54 (0.50)	0.53 (0.50)	0.00
Black	0.58 (0.49)	0.62 (0.49)	0.63 (0.48)	0.63 (0.48)	0.73 (0.44)	0.73 (0.44)	-0.02
Hispanic	0.27 (0.44)	0.26 (0.44)	0.25 (0.44)	0.25 (0.43)	0.17 (0.38)	0.17 (0.38)	0.01
Ever designated with disability	0.21 (0.41)	0.22 (0.42)	0.15 (0.36)	0.16 (0.37)	0.18 (0.38)	0.18 (0.39)	-0.02
Ever Limited English Proficient	0.07 (0.26)	0.07 (0.26)	0.05 (0.22)	0.04 (0.19)	0.03 (0.18)	0.06 (0.23)	0.00
Ever received TANF	0.63 (0.48)	0.71 (0.45)	0.72 (0.45)	0.71 (0.45)	0.69 (0.46)	0.73 (0.45)	-0.07
Weighted sample size	**298**	**298**	**456**	**456**	**213**	**213**	

Note: Standard deviations are in parentheses. The overall standardized difference is the weighted average of treatment-control standardized differences within the baseline groups, with weights equal to the number of accelerated students. Within each baseline group, the treatment-control standardized difference is the raw difference divided by the square root of the average of the treatment and control variances. Control students are weighted by the number of matches in which they are included.

[a] Values are based only on students' time in regular SDP high schools.

Table C.3. Impacts on Five-Year and Six-Year Graduation Rates, Estimated from Samples that Include Nonaccelerated Students Who Dropped Out in the Baseline Period

					Weighted Sample Size		
Outcome	Accelerated Group (Percentage)	Matched Control Group (Percentage)	Difference (Percentage Points)	*p*-value	Accel.	Matched Control	Total
Five-year graduation	28.9	21.3	7.6*	0.001	732	732	1,464
Six-year graduation	25.2	20.5	4.7*	0.012	967	967	1,934

Note: Control group percentages are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

Table C.4. Impacts on Five-Year and Six-Year Graduation Rates, Estimated from Samples in Which Matched Students Share the Same High School at Ninth-Grade Entry

					Weighted Sample Size		
Outcome	Accelerated Group (Percentage)	Matched Control Group (Percentage)	Difference (Percentage Points)	*p*-value	Accel.	Matched Control	Total
Five-year graduation	28.9	20.9	8.0*	0.000	732	732	1,464
Six-year graduation	25.2	21.2	4.0*	0.045	966	966	1,932

Note: Control group percentages are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

Table C.5. Impacts on Five-Year and Six-Year Graduation Rates, Estimated Only from Students with Nonmissing Eighth-Grade PSSA Scores

					Weighted Sample Size		
Outcome	Accelerated Group (Percentage)	Matched Control Group (Percentage)	Difference (Percentage Points)	*p*-value	Accel.	Matched Control	Total
Five-year graduation	31.8	22.5	9.3*	0.000	585	585	1,170
Six-year graduation	26.8	23.7	3.1	0.158	788	788	1,576

Note: Control group percentages are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

Table C.6. Standardized Differences in Baseline Characteristics Between Students Who Did and Did Not Transfer Out of SDP, by Estimation Sample and Treatment Status

	Estimation Sample for Analysis of Five-Year Graduation		Estimation Sample for Analysis of Six-Year Graduation	
Variable Measured in the Baseline Period	Accelerated Group	Matched Control Group	Accelerated Group	Matched Control Group
8th-grade PSSA math score	-0.10	-0.24	-0.05	-0.33
8th-grade PSSA reading score	0.06	-0.37	0.26	-0.28
8th-grade PSSA scores are imputed	-0.07	-0.03	0.05	-0.20
High school credits	0.44	0.29	0.15	0.15
High school GPA	0.44	0.07	0.23	0.07
Age at ninth-grade entry	0.42	0.37	0.39	0.29
Enrolled days per year after ninth-grade entry	0.04	0.13	-0.23	0.07
Fraction of enrolled high school days in specified school type:				
Neighborhood	-0.27	-0.01	-0.08	0.14
Magnet/citywide	0.23	0.01	0.01	-0.18
Disciplinary/transition	0.11	0.05	0.09	0.03
Fraction of enrolled high school days:[a]				
In attendance	0.11	0.20	-0.17	0.03
Suspended	-0.51	0.05	-0.70	-0.16
Female	0.35	0.00	0.53	0.15
Black	0.13	0.19	-0.10	0.09
Hispanic	0.02	0.16	0.11	-0.08
Ever designated with disability	-0.24	-0.03	-0.47	-0.06
Ever Limited English Proficient	-0.11	-0.01	0.05	-0.07
Ever received TANF	0.61	0.31	0.60	0.39

Note: Standardized differences shown in the table are weighted averages of standardized differences within baseline groups, with weights equal to the number of accelerated students. Within each baseline group, the standardized difference is the raw difference in the indicated variable between students who did and did not transfer out of SDP, divided by the square root of the average of the treatment and control variances.

[a] Values are based only on students' time in regular SDP high schools.

Table C.7. Impacts on Five-Year and Six-Year Graduation Rates, Estimated Only from Students Who Did Not Transfer Out of SDP

					Weighted Sample Size		
Outcome	Accelerated Group (Percentage)	Matched Control Group (Percentage)	Difference (Percentage Points)	*p*-value	Accel.	Matched Control	Total
Five-year graduation	29.2	21.4	7.9*	0.001	701	644	1,345
Six-year graduation	25.2	20.8	4.4*	0.021	940	889	1,829

Note: Control group percentages are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

Table C.8. Baseline Characteristics of Accelerated and Matched Comparison Students in the Analysis of Credits Earned

	Group Means							
	1-Year Baseline Group		2-Year Baseline Group		3-Year Baseline Group		4-Year Baseline Group	
Variable Measured in the Baseline Period	Accel. Students	Matched Control Group	Accel. Students	Matched Control Group	Accel. Students	Matched Control Group	Accel. Students	Matched Control Group
8th-grade PSSA math score	-0.53 (0.75)	-0.48 (0.80)	-0.44 (0.75)	-0.44 (0.78)	-0.37 (0.75)	-0.38 (0.85)	-0.53 (0.80)	-0.58 (0.89)
8th-grade PSSA reading score	-0.46 (0.82)	-0.38 (0.86)	-0.46 (0.85)	-0.53 (0.85)	-0.33 (0.85)	-0.30 (0.92)	-0.46 (0.98)	-0.51 (0.82)
8th-grade PSSA scores are imputed	0.18 (0.38)	0.21 (0.41)	0.21 (0.41)	0.22 (0.41)	0.18 (0.38)	0.15 (0.36)	0.16 (0.37)	0.17 (0.38)
High school credits	2.81 (2.44)	2.79 (2.62)	5.91 (3.72)	6.14 (3.50)	7.92 (4.86)	8.27 (4.75)	9.05 (6.26)	8.92 (6.09)
High school GPA	0.64 (0.85)	0.66 (0.85)	0.77 (0.80)	0.79 (0.74)	0.69 (0.74)	0.80 (0.78)	0.72 (0.82)	0.68 (0.84)
Age at ninth-grade entry	15.29 (0.68)	15.24 (0.79)	15.00 (0.61)	15.04 (0.72)	14.83 (0.55)	14.83 (0.58)	14.86 (0.55)	14.94 (0.60)
Enrolled days per year after ninth-grade entry	164 (37)	164 (39)	157 (40)	158 (39)	149 (43)	148 (47)	134 (45)	128 (42)
Fraction of enrolled high school days in specified school type:								
Neighborhood	0.95 (0.19)	0.96 (0.17)	0.93 (0.22)	0.92 (0.24)	0.91 (0.24)	0.90 (0.27)	0.92 (0.24)	0.95 (0.17)
Magnet/citywide	0.03 (0.17)	0.02 (0.14)	0.06 (0.21)	0.06 (0.23)	0.06 (0.22)	0.08 (0.25)	0.05 (0.21)	0.03 (0.15)
Disciplinary/ transition	0.00 (0.05)	0.01 (0.05)	0.01 (0.06)	0.02 (0.07)	0.01 (0.07)	0.01 (0.08)	0.02 (0.10)	0.01 (0.06)
Fraction of enrolled high school days:[a]								
In attendance	0.63 (0.22)	0.60 (0.24)	0.68 (0.17)	0.68 (0.18)	0.66 (0.17)	0.63 (0.20)	0.65 (0.17)	0.65 (0.16)
Suspended	0.02 (0.03)	0.02 (0.04)	0.02 (0.03)	0.02 (0.03)	0.02 (0.02)	0.01 (0.02)	0.02 (0.02)	0.03 (0.03)
Female	0.42 (0.49)	0.39 (0.49)	0.47 (0.50)	0.43 (0.50)	0.44 (0.50)	0.42 (0.50)	0.35 (0.48)	0.31 (0.47)
Black	0.57 (0.50)	0.63 (0.48)	0.64 (0.48)	0.70 (0.46)	0.67 (0.47)	0.58 (0.50)	0.77 (0.42)	0.80 (0.41)
Hispanic	0.21 (0.41)	0.18 (0.38)	0.22 (0.41)	0.20 (0.40)	0.20 (0.40)	0.28 (0.45)	0.13 (0.34)	0.12 (0.32)
Ever designated with disability	0.17 (0.38)	0.16 (0.37)	0.15 (0.35)	0.15 (0.36)	0.15 (0.36)	0.13 (0.33)	0.19 (0.40)	0.26 (0.44)
Ever Limited English Proficient	0.08 (0.27)	0.08 (0.27)	0.10 (0.30)	0.08 (0.28)	0.09 (0.28)	0.11 (0.31)	0.02 (0.15)	0.00 (0.00)
Ever received TANF	0.77 (0.42)	0.75 (0.44)	0.76 (0.43)	0.78 (0.41)	0.75 (0.43)	0.74 (0.44)	0.71 (0.46)	0.72 (0.45)
Weighted Sample Size	**179**	**179**	**296**	**296**	**243**	**243**	**93**	**93**

Note: Standard deviations are in parentheses. Control students are weighted by the number of matches in which they are included.

[a] Values are based only on students' time in regular SDP high schools.

Table C.9. Overall Standardized Differences Between Accelerated and Matched Comparison Students in the Analysis of Credits Earned

Variable Measured in the Baseline Period	Overall Standardized Difference Between Accelerated and Matched Control Students
8th-grade PSSA math score	0.00
8th-grade PSSA reading score	0.00
8th-grade PSSA scores are imputed	-0.01
High school credits	-0.04
High school GPA	-0.05
Age at ninth-grade entry	-0.02
Enrolled days per year after ninth-grade entry	0.02
Fraction of enrolled high school days in specified school type:	
Neighborhood	0.01
Magnet / citywide	-0.01
Disciplinary / transition	-0.05
Fraction of enrolled high school days:[a]	
In attendance	0.09
Suspended	-0.03
Female	0.06
Black	-0.02
Hispanic	-0.01
Ever designated with disability	0.00
Ever Limited English Proficient	0.02
Ever received TANF	-0.01

Note: The overall standardized difference is the weighted average of treatment-control standardized differences within the baseline groups, with weights equal to the number of accelerated students. Within each baseline group, the treatment-control standardized difference is the raw difference divided by the square root of the average of the treatment and control variances.

[a] Values are based only on students' time in regular SDP high schools.

Table C.10. Impacts on New Credits Earned in the 2009-10 School Year, Estimated from Alternative Samples

					Weighted Sample Size		
Sample	Accelerated Group (Mean)	Matched Control Group (Mean)	Difference	*p*-value	Accel.	Matched Control	Total
Includes control students who dropped out in the baseline period	4.4	2.1	2.4*	0.000	811	811	1,622
Originates from matching students sharing the same initial high school	4.4	3.1	1.4*	0.000	811	811	1,622
Includes only students with nonmissing eighth-grade PSSA scores	4.5	3.1	1.5*	0.000	658	658	1,316
Includes only students who did not transfer out of SDP	4.5	3.2	1.3*	0.000	767	743	1,510

Note: Control group means are regression-adjusted to account for treatment-control differences in baseline characteristics. Control students are weighted by the number of matches in which they are included.

*Significantly different from zero at the .05 level, two-tailed test.

Table C.11. Five-Year and Six-Year Graduation Rates, by Provider

	Outcome: Five-Year Graduation		Outcome: Six-Year Graduation	
Name of Provider	Number of Accelerated Students	Graduation Rate (Percentage)	Number of Accelerated Students	Graduation Rate (Percentage)
Camelot	342	33.8	414	28.0
OIC of America	67	22.4	121	24.8
One Bright Ray	194	31.1	248	24.9
YouFirst Learning	129	16.1	184	20.0

Table C.12. Average Number of New Credits Earned in the 2009-10 School Year, by Provider

Name of Provider	Number of Accelerated Students	Average Number of New Credits Earned in the 2009-10 School Year
Big Picture	41	4.3
Camelot	307	6.8
Communities in Schools	88	3.6
OIC of America	52	4.7
Ombudsman	46	2.6
One Bright Ray	86	3.9
YouFirst Learning	157	1.2