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Encouraging Education in an Urban School District: Evidence from the Philadelphia Educational Longitudinal Study

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ABSTRACT *We study a set of programs implemented in Philadelphia high schools that focus on boosting high school graduation, and especially college attendance, using data from the Philadelphia Educational Longitudinal Study (PELS). We examine the effects of these programs on a set of schooling-related outcomes during and after high school. The PELS data-set contains an unusually large amount of information on individuals prior to program placement. We use this information, in the context of both linear models and propensity score-matching estimators, to attempt to correct for selective participation in these programs. We find evidence of positive effects of these programs on high school graduation and on both academic and non-academic awards in high school, and similar negative effects on dropping out of high school. The results also suggest positive effects on attitudes and expectations about college attendance, and on college attendance.*

KEY WORDS: Transition to higher education; college access; high school graduation

Introduction

The positive effects of high school graduation and higher education on earnings and other life outcomes are well known (e.g., Card, 1999; Deaton, 2002). Yet urban high school students drop out at high rates, precluding college attendance, and minority students, who are concentrated in urban school districts, are far less likely to pursue higher education (e.g., Bowen and Bok, 1998; Massey *et al.*, 2003, chapter 1). In this paper, we study a set of programs implemented in Philadelphia high schools that focus on boosting high school graduation and especially college attendance, which we label 'educational encouragement' (EE) programs. Our goal is to estimate the impact of these programs on schooling-related outcomes during and after high school.

Our analysis compares students involved in EE programs with non-participants, among students who attend public high schools in Philadelphia, using data from

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the Philadelphia Educational Longitudinal Study (PELS). Estimating the effects of these EE programs confronts potentially serious problems of selection, as they may recruit more promising and motivated students who are willing to participate in enriched programs, thus making the programs appear more effective than they might in fact be. Alternatively, there may be negative selection if students enter EE programs because they are encountering difficulties likely to deter high school graduation or post-secondary enrollment. While there are, in principle, a number of approaches to addressing the problem of selection, the best approach available to us is to exploit the rich information available in the PELS, including individual and parental characteristics, early test scores, and—potentially most significant—a set of questions about educational expectations and attitudes asked prior to high school. We use the detailed information in the PELS in the context of both linear models and propensity score-matching estimators to attempt to account for selective participation in examining the impact of these programs on schooling-related outcomes during and after high school. Based on what we regard as plausible identifying assumptions and on our findings, it appears that we are able to account for selective participation in EE programs and to identify the causal effects of participation. However, like all attempts to draw causal inferences, uncertainty inevitably remains as to whether the identifying assumptions hold, limiting our ability to make definitive statements without additional types of evidence.

Data and Methods

Philadelphia Educational Longitudinal Study

The PELS follows a 10% sample of students (approximately 2000 students) in the Philadelphia School District, beginning in the eighth grade. The data-set includes school record information and test scores going back to the second grade (for those in the school system at the time) and parent interviews.[1] The survey began in 1996, surveying eighth graders about the 1995–96 school year during the summer of 1996. Wave 2 was carried out during the ninth-grade academic year. The next four waves were carried out in the summers after Grades 9–12, covering the previous academic year. No survey was done in 2001–02, and a seventh wave covering 2003–04 has just been completed and will be used in future analysis along with the administrative information.

For the purposes of the present analysis, we are able to follow students through Wave 6, by which time most would have graduated from high school (unless they dropped out) and matriculated into college or entered the labor force or the military. A small portion of the sample still remained in school or were neither in school nor employed.

Sample Attrition

Because of flux in the school population, it is not easy to measure sample attrition, especially at the beginning of the study. A substantial number of the approximately 2000 students selected in the original sample did not attend any public high school and were dropped from the study. Conversely, a sizable number of students entered the ninth grade who had not previously been in the Philadelphia School District because they moved in from outside the district or switched from private to public schools. Therefore, we measure sample attrition from Wave 3, the sample of 1561 that was selected from the rolls of the ninth-grade attendees. About two-thirds of those students had been interviewed in Wave 1 and the remaining third were added to the sample at Waves 2 or 3. Of those students interviewed by Wave 3 who were also in Wave 1, we managed to re-interview slightly more than 57% at Wave 6, a respectable response rate for a sample in which we relied on telephone interviews. In fact, our true response rate is higher since an unknown portion of the students moved out of the city or switched to private or parochial schools and would not have been eligible for follow-up. We had only limited success in tracking students who moved from the district to other localities.[2]

Educational Encouragement Programs in the PELS

There are a variety of EE-related programs in which students in the PELS can report participation in Waves 3–5. These are programs that the school district has established over the past decade or so, with support largely from private funders, to motivate students to complete high school and more importantly to obtain post-secondary training or education. Most are efforts to provide academic support, counseling, role-models, and career guidance to students who might not normally get such advice from teachers or family members, as the vast majority of students attending the public schools in our sample do not come from families with a college-educated parent.

Many of the programs are quite small, and they vary considerably in their intensity and comprehensiveness. We will consider program heterogeneity in the analysis that follows. But virtually all of the programs that we have identified share a common set of objectives: reinforcing career objectives, exposing students to knowledge and requirements to enter careers, providing role-models and mentors, exposing students to peers that share their ambitions and expectations, and helping students to garner resources to make the transition to higher education. These programs, and brief descriptions, are displayed in Table 1.

Table 1 also provides information on participation for each of the EE programs. We report figures for the sample restricted to observations with valid data on the variables we use in our empirical analysis, including requiring information on EE program participation in at least one of Waves 3, 4, or 5. But the patterns were very similar for larger samples obtained without imposing this restriction.[3] With the exception of College Access, participation rates for the programs are quite small, and among the others only exceed 1% of the sample for Academics Plus and PRIME. But over one-half of the sample reported in Waves 3, 4, or 5 that they had been involved in at least one EE program. We also observed that participation was higher at the beginning and toward the end of the high school years, suggesting that the programs may have initially aimed at providing orientation to the future and, toward the end of high school, helped to prepare students for the transition to college, further training, or employment.[4]

Based on the reports of a knowledgeable informant, we attempted to identify characteristics of the programs reported by students. It appears that many of the smaller programs were transitory efforts to promote access to higher education through exposing students to role-models, exemplars, and contacts in the workplace, providing information about colleges and universities, offering mentoring and remedial services, and helping to identify sources of financial aid. Programs varied greatly in the type and mix of services. There was no single or consistent

Table 1. EE programs in the PELS

Program	Description	Participation rate in PELS
Any participation in EE programs		0.534
Academics Plus	State-licensed and accredited school that offers private instruction, tutoring, summer school in a variety of advanced courses	0.027
ASPIRA	Develops leadership skills, educational endeavors, cultural awareness, and social action among Puerto Rican and Latino students	0.002
College Access	Provides college readiness services, individual advising, financial aid and scholarship assistance to low-income youth from the most disadvantaged areas of the city, emphasizing those who would be the first in their family to attend college	0.451
LASER	Program to expose Philadelphia high school students to advanced science and engineering	0.002
Legacy	Federal TRIO Program providing comprehensive services to disadvantaged or disabled students to assist in pursuing post-secondary education	0.006
Philadelphia Futures	Offers numerous programs to help disadvantaged Philadelphia teenagers excel in their studies and prepare for college and careers	0.002
PRIME	Enhances minority student skills in mathematics, communications, and engineering, through mentoring, mathematics/science/engineering competitions, and summer programs and internships before starting college	0.055
Say Yes to Education	Sponsors students from very disadvantaged backgrounds, providing educational enrichment, tutoring and mentoring, counseling, and other resources, emphasizing relationship with institution of higher education	0.004
White-Williams Scholars	Program provides disadvantaged Philadelphia public high school students who maintain good grades with modest monthly stipend and school-related expenses such as test and college application fees	0.008
Other		0.114

Notes: For details on the TRIO Program, see US Department of Education (2003). The participation rate is computed for the sample with information on participation in Wave 3, 4, or 5, and valid data on the control variables used in the regressions reported in the following tables (n=528). (In those tables, sample sizes vary across dependent variables owing to missing data.) There is another very small program, 'Upward Bound,' which had no participants in our analysis sample.

model that could be identified across the programs or even within established programs. The largest-scale program was College Access, which by-passed traditional high school counselors. It offered information to students about higher education in resource centers located in some schools and in the community. These centers provided information and assistance in filling out college applications, visits to nearby college campuses, and connections to sources of financial aid. College Access also helped students prepare for the SATs. Many of the smaller programs provided similar types of aid although the mix varied, depending on the site.

The heterogeneity between (and within) programs makes it very difficult to determine just how much of what types of services were offered to particular students in the PELS sample. Hence it is difficult to match particular components of the programs to particular outcomes in the analysis that follows. Nonetheless, we can safely assume that students who participated in the array of programs presented in Table 1 received more encouragement to finish high school and to apply to college, more information about how and where to apply, more assistance in the application process, and more sponsorship in garnering financial aid than non-participants.

Outcome Measures

The PELS data-set is extremely rich, and it is not possible to analyze in a single paper all of the potential available outcomes. We have chosen to concentrate in this analysis on the most obvious measures of academic success or related behaviors that should be linked to exposure to an EE program: dropping out, high school graduation, educational aspirations and expectations, and college attendance. There is also information available in the PELS on current employment and wages for those who stopped attending school and for all respondents in later waves, after-school and summer jobs, participation in the underground economy, criminal behavior, and employment and earnings information from the unemployment records (UI) records. However, as explained in the following subsection, the PELS is uniquely well suited to trying to estimated causal effects of program participation on education-related outcomes.

Identification, Control Variables, and Proxies

Our interest in this paper is in identifying the causal effects of participation in EE programs. Our framework is to estimate linear regression models for the outcomes we study, including a set of control variables on which participants and non-participants may differ. We also implement propensity score-matching estimators that estimate program effects from differences between participants and non-participants matched more closely than may be accomplished by the regression controls. However, these methods only correct for selection on observables, not selection on unobservables that are potentially correlated with both program participation and the outcomes we study, that are conditional on the control variables we include in the regression models or that differ systematically between participants and non-participants who are matched on observable characteristics.

Ideally, to infer causal effects of program participation we would like to have random assignment to these programs; and, in the absence of that, a compelling source of exogenous variation in program participation to use as an instrumental variable. However, the first is unavailable in the context of the programs we study, and indeed in most research on these types of programs.[5] In addition, there is no natural instrumental variable at the individual or family level, given that any such characteristic would probably affect the outcomes we study as well as program participation. Similarly, there is no institutional information of which we are aware that generates differential participation rates across schools; and even if there were, it is not clear that this would be unrelated to school-specific variation in effects on the outcomes we study.[6] Finally, although longitudinal data can often help account for selection bias, in the case of the programs we study they are inapplicable, because there is no meaningful pre-treatment outcome with which to compare post-treatment outcomes, in contrast, say, to a training program for which we might observe wages or employment both before and after the program.

Fortunately, the PELS data-set that we examine in this paper contains an unusual amount of rich information on individuals prior to placement in EE programs. In addition to fairly typical demographic controls, it includes detailed measures of family background and structure, and prior measures of academic achievement, including test scores going back to the second grade. Most significantly for the purposes of identification, it also includes responses to a set of questions about educational expectations and attitudes asked at Wave 1—prior to high school, and therefore prior to participation in the EE programs we study. Specifically, respondents are asked about their disappointment associated with failure to attain specific educational levels, how strongly they believe that doing well in school is important to be successful in life, and the likelihood of graduating from high school and from college by age 25 (details are given later).

Of course, the first line of defense against differences between participants and non-participants in any attempt to estimate the effects of program participation is to introduce an extensive set of controls for the factors that might be correlated with program participation and also affect the dependent variables. Informally, comparing the estimated coefficients of program participation with and without a detailed set of control variables can help to gauge whether biases from remaining unobservables are likely. However, there are conditions under which the control variables can serve as proxy variables for the unobservables and fully correct for the selection problem, and the data on educational attitudes and expectations may satisfy these conditions.

Wooldridge (2002) discusses the two conditions under which the inclusion of these proxy variables (Z) leads to unbiased estimates of the other parameters of interest, in the presence of unobserved factors (q) correlated with program participation (EE, in our case) and the outcomes Y. The first condition is that Z is redundant in the equation of interest. Letting X denote the included controls, the redundancy condition is $\mathrm{E}(Y \mid \mathrm{EE},X,q) = \mathrm{E}(Y \mid \mathrm{EE},X,Z,q)$; that is, conditional on EE, X, and the unobservable q, Z provides no information about Y. This is not a controversial assumption, as the only reason we include Z is because we do not have a measure of q. The second condition is that if we take the linear projection of q on Z, and define the projection error as η, then η is uncorrelated with EE and each of the variables in X. In words, the proxy (or proxies) Z is sufficiently closely related to the unobservable q that, once Z is included in the equation, there is no omitted variable bias in the estimated coefficients of EE and X from the variation in q that is not captured in the linear projection of q on Z.

Clearly, in an informal sense the information on educational attitudes and expectations measured prior to EE program participation should capture a good deal of the information individuals possess about their own education-related goals and aptitudes on the basis of which they might select into these programs, and hence should substantially reduce bias from selective participation. However, they may also satisfy the ideal conditions for proxy variables outlined above. If these attitudes and expectations reported at Wave 1 serve as efficient statistical forecasts of later educational outcomes, incorporating all information available to respondents at the time they are reported, then any remaining variation in the unobservable is orthogonal to EE, and hence does not bias the estimated effects of program participation. In addition, the educational attitudes and expectations should not have any independent effects on outcomes net of the unobserved propensities for post-high school enrollment and other schooling-related outcomes for which they are proxies. These are the two conditions for a proxy variable to eliminate bias due to selection (or for a 'control variable' to function as a 'proxy variable'). Intuitively, if, for example, conditional on educational expectations, EE participants are more likely to be enrolled in college after leaving high school, then it seems sensible to infer a causal effect of EE programs, because the expectations questions should capture unobservables associated with post-high school educational outcomes and program participation.

If the educational attitudes and expectations variables available in the PELS satisfy these conditions, then when they are included we obtain unbiased estimates of the effects of EE program participation. Of course, like all identifying assumptions, there is uncertainty as to whether they hold, and at some core level the assumptions are untestable. For example, one could argue that the assumption that eighth-grade respondents at Wave 1 make efficient forecasts is overly strong. In addition, the attitudes and expectations questions do not refer precisely to all of the outcomes we measure. However, none of the other potential approaches to the selection problem in the context of EE programs avoid this conundrum. The conditions for recovering unbiased estimates from a random assignment design may be violated, and the assumptions underlying an instrumental variable may also fail to hold. We believe that for purposes of estimating the effects of EE programs in the PELS data, the proxy variable assumptions regarding the educational attitudes and expectations variables are more defensible than assumptions that, for example, would use some parental characteristic as an instrumental variable for program participation. Thus, we proceed cautiously with interpreting our estimates as causal effects of EE program participation, while recognizing that this interpretation—as always—is dependent upon identifying assumptions. In the worst-case scenario, our estimates can be interpreted as incorporating many controls for possible selection bias, without necessarily eliminating the bias. Viewed in this light, it is worth noting that the estimates of program effects we report in the next section are not very sensitive to excluding the educational attitudes and expectations, which might be viewed as indicating that selection on unobservables plays a minor role, and thus bolstering a causal interpretation of our findings.

Data Analyses

We first explore the differences between students in our sample who enter EE programs and those who do not. This provides a starting point for assessing the degree to which selectivity is operating in program participation. We then examine a series of models for the key outcomes of high school dropout, high school completion, achievement while in high school, educational aspirations and expectations regarding higher education (subsequent to program participation), and matriculation in college. For each of the outcomes, we include the demographic and family-related control variables, information on early test scores, and the educational attitudes and expectations proxies.[7] We also report on the relationships between the educational attitudes and expectations from Wave 1 and later outcomes, to see how well the early attitudes and expectations predict realized behavior. In addition to regression models, we report estimates of the average treatment effect on the treated from propensity score-matching estimators, to more flexibly control for observable differences between participants and non-participants than is accomplished by adding linear controls to a regression model. Finally, we report some analyses where we estimate separate effects by type of school, by 'at-risk' attributes, and by sex, to examine heterogeneity in the effects of EE programs.

Empirical Results

Descriptive Information on Participants and Non-participants

Table 2 displays descriptive information on the control and proxy variables used in our analyses, for the entire sample and for participants in the EE programs.[8] There are some differences between program participants and the larger PELS sample, but most of the differences are generally quite modest. Participants are quite a bit more likely to be black (0.78 vs 0.69 in the total sample). They are a bit less likely to have a mother who is a high school graduate (or dropout), and correspondingly more likely to have a mother who is a college graduate. On the other hand, they are slightly more likely to have below-median reading scores, and to live in non-nuclear families. Looking at the four variables capturing educational attitudes and expectations at Wave 1, participants have both attitudes and expectations that favor higher education, relative to the full sample. Finally, participants are less likely to be attending magnet schools, but also less likely to be attending vocational schools; conversely, they are more likely to be attending the more common 'neighborhood' schools. Overall, although the figures in Table 2 do not indicate that differences between EE program participants and non-participants invariably favor the EE participants in terms of predictors of academic success, some of the differences point in this direction.

High School Outcomes

We start by looking in Table 3 at the link between EE program participation and high school dropout and graduation. These two outcomes are not simply mirror opposites, because classifying someone has a high school dropout is ambiguous, since one can return to school. Because of the difficulty of classifying dropouts, we use any of three identifiers—a direct survey response, administrative information, or survey evidence that a respondent was out of high school, had not yet completed it, and did not return in the period in which we see them. In contrast, high school graduation is unambiguous, and is based on a survey response.

The estimates in column (1) of Table 3 are from a linear probability model for high school dropout, including all of the controls and proxies, and simply including an indicator for any EE program participation.[9] We find that participation in an EE program is associated with a statistically significant decline in dropout from high school, of about 6.6 percentage points. We do not show results for all of the control variables, but we do show them for the educational attitudes and expectations proxies. The coefficients of these variables are neither individually nor jointly significant, and, given that we include all four, multicollinearity might explain why some show unexpected coefficients. However, the estimated coefficient on the variable regarding the expectation for high school graduation is the largest in absolute value, and is negative, as we would expect (and is nearly significant at the 10% level).

Table 2. Demographic characteristics of EE participants and non-participants: sample in any wave with EE information

	Female	Black	Latino	Asian/other	Lives with biological mother only	Lives with one biological and one step parent	Other living arrangement	Mother high school graduate	Mother some college
Overall sample proportions	0.593	0.686	0.068	0.030	0.352	0.155	0.119	0.386	0.136
Proportion among EE participants	0.606	0.784	0.067	0.018	0.344	0.170	0.135	0.368	0.131
	Mother college graduate	Reading test score below median	Mathematics test score below median	'Very disappointed if do not graduate from college'	'Have to do well in school to be successful in life'	'High chance of graduation from high school by age 25'	'High chance of graduation from college by age 25'	Magnet school	Vocational school
Overall sample proportions	0.170	0.390	0.405	0.735	0.784	0.797	0.648	0.170	0.104
Proportion among EE participants	0.195	0.379	0.401	0.773	0.809	0.823	0.663	0.138	0.078

Notes: There are 528 observations, of which 282 participated in EE programs. 'Some college' includes 2-year degrees. The shares below the median are different from 0.5 because the medians are computed for the full sample of available test scores. There is also a group with mother's education unknown or no response, with 0.21 of the observations. The variable measuring 'disappointment if do not graduate from college' is coded as 1–5, from 'very disappointed' to 'not too disappointed'; whether or not the response was 1 is used. (The PELS also includes a similar variable for disappointment with not graduating high school, which is closely related, with even more respondents indicating that they would be very disappointed not to graduate.) The variable measuring 'Have to do well in school to be successful in life' is coded as 'strongly agree,' 'agree,' 'disagree,' or 'strongly disagree'; whether or not the response was 'strongly agree' is used. The variable measuring 'chance of graduation from high school by age 25' is coded as 'high,' 'low,' or 'in the middle'; whether or not the response was 'high' is used. The variable measuring 'chance of graduation from college by age 25' is coded as 'high,' 'low,' or 'in the middle'; whether or not the response was 'high' is used. These four variables are measured at Wave 1.

Table 3. Effects of EE participation on high school education as of Wave 6

(1)	(2)	(3)	(4)	(5)	(6)	
Dependent variable	High school dropout			High school graduation		
Mean of dependent variable	0.068			0.813		
Estimator	LP	LP	PS	LP	LP	PS
Any EE	−0.066 (0.022)	–	−0.062 (0.026)	0.148 (0.038)	–	0.136 (0.040)
Academics Plus	–	−0.043 (0.029)	–	–	0.087 (0.091)	–
College Access	–	−0.053 (0.021)	–	–	0.112 (0.034)	–
PRIME	–	−0.049 (0.019)	–	–	0.109 (0.050)	–
Combined small programs	–	−0.038 (0.033)	–	–	0.136 (0.074)	–
Other	–	−0.045 (0.023)	–	–	0.040 (0.052)	–
Test equality of EE coefficients (p value)	–	0.996	–	–	0.791	–
Test EE coefficients jointly equal zero (p value)	–	0.001	–	–	0.001	–
'Very disappointed if do not graduate from college,' Wave 1	0.034 (0.023)	0.034 (0.023)	–	−0.087 (0.038)	−0.084 (0.038)	–
'Have to do well in school to be successful in life,' Wave 1	−0.003 (0.029)	−0.003 (0.029)	–	0.085 (0.046)	0.085 (0.046)	–
'High chance of graduation from high school by age 25,' Wave 1	−0.053 (0.033)	−0.058 (0.034)	–	0.079 (0.047)	0.092 (0.048)	–
'High chance of graduation from college by age 25,' Wave 1	0.001 (0.025)	0.001 (0.025)	–	0.017 (0.037)	0.015 (0.037)	–
Test educational expectations/attitudes jointly equal zero (p value)	0.287	0.270	–	0.017	0.015	–
n	528	528	524	496	496	491

Notes: 'LP' indicates ordinary-least squares estimates of a linear probability model. 'PS' indicates propensity score. We used kernel matching with a biweight kernel. In both cases, bootstrapped standard errors are reported, based on 250 replications. Observations on the treatment group that are outside of the support for the control group are dropped. EE program information is available in Waves 3–5. Control variables include all variables presented in Table 2. Aside from the EE program coefficients, only the estimated coefficients for educational expectations/attitudes as of Wave 1 are reported.

In column (2) we expand the specification to allow varying effects of different programs, although we combine the programs with very low participation rates into one catch-all category. The estimates suggest that the effect is roughly the same for most of the programs. Correspondingly, Table 3 also reports that the restriction of equal effects across all programs is not rejected, with a *p* value of 0.996. On the other hand, the joint hypothesis that all of the program effects are zero is strongly rejected, with a *p* value of 0.001. Thus, the simpler model fits the data well, and the results in either case indicate that participation in EE programs reduces high school dropout.

Column (3) of Table 3 reports results from a propensity score-matching estimator. We used a kernel-matching algorithm with a biweight kernel. We report the estimate of the average treatment effect on the treated, with the bootstrapped standard error.[10] There were some significant or marginally significant differences in means between the treated and untreated groups for the unmatched sample,[11] but in every case means for the control variables were not significantly different for these groups after matching. In addition, there was very little non-overlap in the support. We dropped the handful of observations in the treatment group not in the support for the control group.[12] The estimate is column (3) is virtually the same as in column (1), indicating a statistically significant reduction in high school dropout from participation in EE programs.

The evidence on high school graduation is reported in columns (4)–(6). The estimate in column (4) indicates a large and significant positive effect of EE program participation on high school graduation, with an increase of 14.8 percentage points. For this outcome, the Wave 1 educational attitudes and expectations proxies are jointly significant, and there are relatively large positive estimates for the likelihood of high school graduation and believing that doing well in school is important, although the negative estimate for disappointment with not graduating from college is unexpected. As reported in column (5) of Table 3, in this case the estimates for the separate programs are a bit more variable; although all are positive, three are individually significant at the 10% level or better, they are jointly significant, and we again do not reject equality of effects across programs. Participation in College Access and PRIME have the strongest effects in terms of the strength of the statistical evidence. Finally, the propensity score-matching estimate in column (6) is only a shade smaller than the corresponding estimate in column (4) and remains strongly significant; the estimate indicates that EE program participation boosts the probability of high school graduation by 13.6 percentage points.

Note that the means at the top of Table 3 suggest a dropout rate under 10% by Wave 6, and a graduation rate near 80%. The low dropout and high graduation rates in large part reflect attrition from the sample by Wave 6 of those more likely to drop out and less likely to graduate. This is confirmed based on administrative data on dropping out and graduation that are available whether or not one is surveyed in Wave 6. Without exception, these administrative data show that, among those entering the PELS in an early wave, administrative dropout rates are lower and administrative graduation rates are higher for those respondents who had not attrited by Wave 6.[13] A further problem is highlighted by findings reported in Neild *et al.* (n.d.); in particular, administrative dropout rates were higher for the sampling universe of the PELS than for those who were ever surveyed, suggesting that there was also selective inclusion in the sample based on a lower likelihood of dropping out. Thus, the mean dropout and graduation rates displayed in Table 3 are not representative of either the surveyed population or the sample universe. However, given the large set of variables on which we condition (as well as match), we probably capture most of the differences between attriters and non-attriters.[14]

As discussed above, we believe there are reasonable assumptions under which the estimates in Table 3 (and ensuing tables) can be viewed as causal estimates of the effects of EE program participation, but of course we cannot state this definitively. As noted earlier, a causal interpretation is further strengthened by the fact that omitting various sets of controls has little impact on the estimates (see Furstenberg and Neumark, 2005), implying that a story about unobservables driving our results would have to appeal to selection on characteristics largely orthogonal to those for which we are able to control.

Table 4 reports results for other indicators of high school behavior or success, including the number of times a student reports cutting or skipping classes during the year, and the receipt of academic and non-academic awards in high school. For each dependent variable, we report the same three specifications as we did in Table 3, so we summarize the results more succinctly here. As reported in columns (1)–(3), there is not a significant effect of EE program participation on skipping or cutting classes. The estimates are insignificant, and in column (2), for the programs broken down separately, the sign varies.

For both academic and non-academic awards, however, there is statistically significant evidence of positive effects of participating in EE programs. For academic awards, the simple linear regression estimate with a single participation variable indicates a significantly higher probability of receipt, by 10.9 percentage points. With separate EE program variables, all of the coefficient estimates but one are positive, and the effect of College Access is large and significant. We do not reject equality of these coefficients, but we also do not reject the joint hypothesis that their effects are zero, although the p value is relatively small. The estimated coefficients of the educational attitude and expectations proxies are all positive, and are jointly significant. In the propensity score matching estimator, the estimate falls a bit, from 0.109 to 0.085, and is significant only at the 10% level. For non-academic awards, the evidence of positive effects of EE programs is a bit stronger—in particular, the propensity score-matching estimate, which remains strongly significant and is virtually the same as the regression estimate in column (7). Thus, it appears that program participants do indeed receive the kind of reinforcement for investing in productive activities during school that might be predicted from involvement in an EE program. Involvement in the programs along with the mentoring and sponsorship typically provided appears to result in the acquisition of 'social capital' useful for succeeding in high school.

Post-high School Education-related Outcomes

Next, we turn from high school outcomes and achievements to post-secondary education, to see whether participation in an EE program is linked to changes that might be maintained after completion of high school. We look first, in Table 5, at educational aspirations for the years beyond high school. This is an important indicator of educational success because previous research has suggested that aspirations help to predict educational attainment, although the link is not necessarily strong (e.g., Campbell, 1983; Kao and Thompson, 2003). Furthermore, aspirations generally decline during the high school years, especially when students

Table 4. Effects of EE participation on high school behaviors/achievements as of Wave 6

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	Number of times cut or skipped classes			Academic award			Non-academic award		
Mean of dependent variable		6.61			0.500			0.316	
Estimator	OLS	OLS	PS	LP	LP	PS	LP	LP	PS
Any EE	1.83 (1.46)	–	1.41 (1.59)	0.109 (0.044)	–	0.085 (0.048)	0.126 (0.043)	–	0.122 (0.044)
Academics Plus	–	–2.53 (1.71)	–	–	0.047 (0.138)	–	–	–0.042 (0.137)	–
College Access	–	1.65 (1.48)	–	–	0.102 (0.045)	–	–	0.096 (0.044)	–
PRIME	–	0.88 (2.94)	–	–	0.045 (0.106)	–	–	0.149 (0.108)	–
Combined small programs	–	–3.62 (2.96)	–	–	–0.094 (0.145)	–	–	0.205 (0.165)	–
Other	–	–0.56 (1.42)	–	–	0.096 (0.070)	–	–	0.045 (0.068)	–
Test equality of EE coefficients (p value)	–	0.528	–	–	0.725	–	–	0.710	–
Test EE coefficients jointly equal zero (p value)	–	0.603	–	–	0.141	–	–	0.092	–
Number of programs x years	–	–	–	–	–	–	–	–	–
'Very disappointed if do not graduate from college,' Wave 1	–2.95 (1.97)	–2.70 (1.95)	–	0.056 (0.045)	0.056 (0.044)	–	0.061 (0.043)	0.063 (0.042)	–
'Have to do well in school to be successful in life,' Wave 1	0.20 (1.84)	0.20 (1.84)	–	0.099 (0.052)	0.098 (0.053)	–	0.094 (0.049)	0.094 (0.049)	–
'High chance of graduation from high school by age 25,' Wave 1	–1.37 (2.44)	–1.47 (2.49)	–	0.091 (0.056)	0.087 (0.056)	–	0.018 (0.050)	0.037 (0.051)	–
'High chance of graduation from college by age 25,' Wave 1	–0.08 (1.65)	–0.20 (1.66)	–	0.047 (0.048)	0.050 (0.048)	–	0.046 (0.045)	0.041 (0.044)	–
Test educational expectations/attitudes jointly equal zero (p value)	0.500	0.527	–	0.007	0.007	–	0.042	0.025	–
n	429	429	421	528	528	524	528	528	524

Notes: See notes to Table 3. Academic awards include academic honor, award in science or mathematics fair, special recognition for good grades or honor roll, or special recognition for writing an essay or poem. Non-academic awards include elected officer of a school class, named most valuable player on a sports team, community service award, or award in technical or skills competition.

Table 5. Effects of EE participation on educational aspirations, attitudes, and expectations at Wave 6

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	Would like to achieve at least a 4-year college degree			College degree (4-year) is lowest level of education with which satisfied			Expect at least 4-year college degree		
Mean of dependent variable		0.833			0.485			0.762	
Estimator	LP	LP	PS	LP	LP	PS	LP	LP	PS
Any EE	0.067 (0.033)	–	0.073 (0.038)	0.120 (0.046)	–	0.091 (0.050)	0.099 (0.038)	–	0.080 (0.044)
Academics Plus	–	0.149 (0.094)	–	–	0.333 (0.144)	–	–	0.172 (0.098)	–
College Access	–	0.021 (0.032)	–	–	0.063 (0.046)	–	–	0.080 (0.036)	–
PRIME	–	0.127 (0.048)	–	–	0.152 (0.082)	–	–	0.031 (0.078)	–
Combined small programs	–	−0.111 (0.117)	–	–	−0.081 (0.147)	–	–	−0.043 (0.162)	–
Other	–	0.109 (0.034)	–	–	0.113 (0.075)	–	–	0.058 (0.055)	–
Test equality of EE coefficients (*p* value)	–	0.140	–	–	0.325	–	–	0.643	–
Test EE coefficients jointly equal zero (*p* value)	–	0.009	–	–	0.033	–	–	0.126	–
Number of programs x years	–	–	–	–	–	–	–	–	–
'Very disappointed if do not graduate from college,' Wave 1	0.042 (0.039)	0.034 (0.039)	–	0.088 (0.049)	0.082 (0.048)	–	0.032 (0.045)	0.032 (0.044)	–
'Have to do well in school to be successful in life,' Wave 1	0.023 (0.038)	0.020 (0.039)	–	−0.004 (0.048)	−0.004 (0.048)	–	0.043 (0.046)	0.047 (0.047)	–
'High chance of graduation from high school by age 25,' Wave 1	0.098 (0.053)	0.092 (0.053)	–	0.110 (0.057)	0.110 (0.056)	–	0.109 (0.054)	0.109 (0.056)	–
'High chance of graduation from college by age 25,' Wave 1	0.003 (0.039)	0.009 (0.040)	–	0.024 (0.050)	0.031 (0.051)	–	0.021 (0.040)	0.023 (0.040)	–
Test educational expectations/attitudes jointly equal zero (*p* value)	0.269	0.347	–	0.054	0.058	–	0.127	0.120	–
n	503	503	500	501	501	499	484	484	483

Notes: See notes to Table 3. Educational aspirations, attitudes, and expectations elicit information on a variety of educational levels, ranging from no schooling to an advanced degree; we collapse these into a 4-year degree or higher, or less education.

begin to encounter setbacks in school and face the difficult challenges of proceeding on to higher education. Therefore, as measured at Wave 6, the year after expected high school graduation, educational aspirations may be quite informative. We look at two measures: whether the individual reports that they would like to achieve at least a 4-year college degree, and whether a 4-year degree is the lowest level of education with which they would be satisfied. These may seem like relatively high bars, but, as indicated in the top row of Table 5, a large share report aspirations for at least a 4-year college degree.

For the first measure—whether the individual would like to achieve a 4-year degree—the evidence suggests that EE programs boost aspirations. The estimate in column (1) of Table 5 is significant at the 5% level, and the estimate in column (3) at the 10% level. In column (2) the estimated effects of separate programs are jointly significant even at the 1% level, although one of the estimates—for 'combined small programs'—does not follow the overall pattern of the larger programs or those in programs that were not otherwise classified. This disparity may be due to the fact that some of the smaller programs could stress a school-to-work strategy rather than a school-to-higher education approach.

The results for the lowest level of education with which the respondent would be satisfied are similar. In column (4), the estimated effect is positive and strongly significant, although again the propensity score matching estimate is significant only at the 10% level. The last three columns of Table 5 extend the analysis to look at expectations as of Wave 6, rather than aspirations. The findings are quite similar to those just reported, with a strongly significant positive effect of EE program participation resulting from the regression, but a slightly smaller matching estimate that is significant only at the 10% level.[15]

The evidence on the effects of EE program participation on aspirations and expectations regarding higher education is a perhaps a shade weaker statistically than is the evidence for effects on high school outcomes. Nonetheless, we read the overall findings as suggesting that EE program participation reinforces and perhaps strengthens future ambitions and expectations regarding college completion. More important, however, is whether these programs have any effects on actual college enrollment.

Table 6 turns to the evidence on post-secondary education. We estimate models for current enrollment at a college, university, or technical school, enrollment at a 4-year college or university, and full-time enrollment. Across these three different measures of participation in post-secondary education, the estimated effects of EE program participation are always positive (with a few exceptions for the specifications estimating effects of separate programs). In most cases, the estimated positive effects are significant at the 10% level but not the 5% level. Exceptions are the regression estimate of the effect on full-time enrollment, in column (7), and the effect of College Access on 4-year enrollment, both of which are significant at the 5% level.[16] On the other hand, for these three outcomes the propensity score-matching estimates are very similar to the regression estimates.[17] Overall, while again slightly weaker than the evidence for high school outcomes, the evidence in Table 6 suggests that EE program participation has positive effects on various dimensions of college enrollment.

We suspect that students in this school district often experience considerable difficulties in engaging in the planning that it takes to apply for, gain admittance to, and enroll in college, and the EE programs help with this. Thus, it is not necessarily that the non-participants do not want to go on to higher education; rather,

Table 6. Effects of EE participation on current college enrollment at Wave 6, for high school graduates

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Currently enrolled at college, university, or technical school			Currently enrolled at a 4-year college or university			Currently attending college, university, or technical school full-time		
Mean of dependent variable		0.632			0.401			0.567	
Estimator	LP	LP	PS	LP	LP	PS	LP	LP	PS
Any EE	0.071 (0.049)	–	0.070 (0.055)	0.088 (0.049)	–	0.099 (0.055)	0.097 (0.049)	–	0.096 (0.056)
Academics Plus	–	0.021 (0.137)	–	–	−0.151 (0.109)	–	–	0.098 (0.145)	–
College Access	–	0.078 (0.047)	–	–	0.109 (0.048)	–	–	0.088 (0.049)	–
PRIME	–	−0.039 (0.106)	–	–	0.179 (0.102)	–	–	−0.020 (0.100)	–
Combined small programs	–	0.054 (0.173)	–	–	0.147 (0.147)	–	–	0.103 (0.195)	–
Other	–	0.094 (0.069)	–	–	0.101 (0.075)	–	–	0.086 (0.077)	–
Test equality of EE coefficients (*p* value)	–	0.852	–	–	0.258	–	–	0.887	–
Test EE coefficients jointly equal zero (*p* value)	–	0.393	–	–	0.041	–	–	0.189	–
Number of programs x years	–	–	–	–	–	–	–	–	–
'Very disappointed if do not graduate from college,' Wave 1	−0.015 (0.050)	−0.015 (0.051)	–	0.093 (0.048)	0.092 (0.048)	–	−0.014 (0.052)	−0.014 (0.051)	–
'Have to do well in school to be successful in life,' Wave 1	0.020 (0.072)	0.014 (0.073)	–	−0.052 (0.067)	−0.070 (0.068)	–	−0.004 (0.070)	−0.010 (0.070)	–
'High chance of graduation from high school by age 25,' Wave 1	0.068 (0.065)	0.072 (0.067)	–	0.039 (0.067)	0.053 (0.063)	–	0.065 (0.065)	0.074 (0.067)	–
'High chance of graduation from college by age 25,' Wave 1	−0.035 (0.056)	−0.037 (0.057)	–	−0.015 (0.058)	−0.020 (0.058)	–	−0.020 (0.059)	−0.022 (0.060)	–
Test educational expectations/attitudes jointly equal zero (*p* value)	0.809	0.821	–	0.370	0.291	–	0.894	0.871	–
n	394	394	387	389	389	382	393	393	386

Notes: See notes to Table 3. Only high school graduates are included in the samples for this table.

they may be less able to mobilize the resources to succeed in navigating the pathway to a college education. Indeed, College Access appears to be the program most strongly focused on this outcome, and it has its strongest positive effects on high school graduation, on enrollment at 4-year colleges and universities, and on enrollment on a full-time basis, as well as by far the largest participation rate. At the same time, a comparison with the earlier estimates suggests that the effects of EE program participation are larger for high school graduation than for college attendance. This may seem surprising given that the most common program—College Access—is focused on college attendance. However, just because the program focuses on college attendance does not necessarily mean it achieves that goal, and college attendance may be more influenced by financial resources, and so faith. In addition, despite its intentions, the program may do more to increase motivation to finish high school. At the same time, we do not observe these students very long after completing high school, and some may go to college later, especially in the relatively low-income, urban, and minority population that we study.

Disaggregated Analyses

In looking at heterogeneity in the effects of EE programs, we first consider the possibility that the outcomes might have been linked to the type of high school that students attended: whether they went to a magnet school that required tests or screening to be admitted, or whether they attended one of the less-selective neighborhood schools (or vocational schools). We consider evidence on the effects of different types of school by estimating the models with a main effect of EE program participation, main effects for type of school, and interactions between the type of school and EE program participation; the interactions identify differences in the effects of EE programs across school types. We look at what we view as the key dependent variables related to schooling outcomes, as presented in Table 7. In each column, the main effect ('any EE') captures the effect for students in non-magnet, non-vocational schools (i.e., neighborhood schools), and the interactions capture differences in the effects in magnet or vocational schools. The sum of the main and interactive effect is the overall effect for, e.g., students in magnet schools; in square brackets, we report the pvalues for the test that the sum of the main and interactive effect is zero.

The estimates in the first row of Table 7 are for neighborhood schools. For four of the five outcomes—high school dropout, high school graduation, aspirations for a 4-year college degree, and 4-year college attendance—EE programs have significant effects indicating improved educational outcomes for students in these schools. However, for the most part these effects are not present for magnet school students. The estimates of the interactions between attending a magnet school and participating in an EE program are in all five cases the opposite sign of the main effects of EE participation, and, as reported in square brackets, for all of the dependent variables except high school graduation the overall effect of EE participation for magnet school students (the sum of the main and interactive coefficient) is insignificant.

For vocational school students the story is a bit more complex. The effects on high school dropout and graduation are dampened relative to the effects on students at neighborhood schools (as the interactions are the opposite sign from the main effects), and are insignificant for students at vocational schools. But for

Table 7. Effect of EE participation on schooling-related outcomes at Wave 6, controlling for type of school and allowing different effects by type of school

	High school dropout (1)	High school graduation (2)	Would like to achieve at least a 4-year college degree (3)	College attendance (4)	4-year college attendance (5)
Any EE	–0.087 (0.030)	0.170 (0.046)	0.086 (0.042)	0.061 (0.059)	0.120 (0.058)
Any EE x magnet school	0.072 (0.041) [0.598]	–0.031 (0.067) [0.007]	–0.153 (0.070) [0.257]	–0.130 (0.098) [0.419]	–0.183 (0.112) [0.540]
Any EE x vocational school	0.038 (0.077) [0.489]	–0.153 (0.117) [0.879]	0.134 (0.093) [0.009]	0.374 (0.162) [0.004]	0.093 (0.165) [0.168]
Magnet school	–0.105 (0.041)	0.126 (0.059)	0.185 (0.050)	0.117 (0.073)	0.249 (0.083)
Vocational school	–0.023 (0.062)	0.002 (0.083)	–0.036 (0.078)	–0.179 (0.113)	–0.133 (0.094)
Test school type–EE program interactions jointly equal zero (p value)	0.213	0.372	0.012	0.016	0.197
n	528	496	503	394	389

Notes: See notes to Table 3. Other control variables are the same as in those specifications. Linear probability model estimates with bootstrapped standard errors are reported. For the interactions, the p value for the sum of the main and interactive EE participation variables is reported below the standard errors in square brackets. Only high school graduates are included in the last two columns, as in Table 6.

post-secondary aspirations and attendance, the effects of EE programs are stronger for vocational students; in all cases the interactions imply a larger effect than for neighbourhood school students, and for the aspirations measure and simply college attendance the implied effect for vocational school students is strongly significant.

Thus, these findings indicate that the effects of EE programs we have documented thus far arise most strongly for students in neighborhood schools, and to some extent for vocational school students, but tend not to appear for magnet school students. This probably occurs because the non-participants in magnet schools are also having their educational goals and aspirations reinforced by peers in these more selective environments. On the other hand, most of the interactions estimated in Table 7 are not statistically significant, so there is not overwhelming statistical evidence of differences in effects of EE programs across different types of schools.

In contrast to the previous table testing for differences in effects by type of school, we next examine differences in effects by type of student. In particular, we identified a number of variables that might be viewed as prior indicators of 'at-risk' students, and estimated the models adding the at-risk indicator interacted with EE program participation (the main effects for the at-risk indicators are already included). The at-risk indicators included: non-nuclear family, eighth-grade mathematics scores below median, eighth-grade reading scores below median, and mother's education less than high school. In these specifications, then, the main effect of EE program participation ('any EE') measures the effect for the not-at-risk sample, the estimated coefficient of the interaction captures the difference in the effect for those at risk, and the sum captures the overall effect for at-risk students. The results are reported in Table 8.

There is some evidence that EE program participation is more beneficial to at-risk students, although the evidence is not entirely consistent across risk categories and outcomes, is not strong statistically, and occasionally points in the opposite direction. In particular, EE participation appears to have larger effects on the college-related variables for those with low mathematics scores; for the aspirations measure and 4-year college attendance, the estimated overall effect is significant for this at-risk group, and insignificant for the others. On the other hand, we do not find the same result for those with low reading scores, for whom, in contrast, EE program participation appears to do more to reduce high school dropout and increase high school graduation. For students from non-nuclear families, similarly, the point estimates suggest that EE programs do more to reduce dropout and increase high school graduation. Conversely, respondents with mothers who had completed at most high school appear to get a substantial boost in the likelihood of college attendance and 4-year college attendance specifically, although this result did not hold if we defined low mother's education as less than high school. At the same time, there are a couple of cases in which the estimates suggest that EE programs have less salutary impacts on at-risk students, including the effects on college aspirations and college attendance for students with low reading scores and from non-nuclear families; in these cases, EE programs have a rather clear significant positive effect on students who are not at risk, but the effect is offset by a negative interaction for the at-risk group. In addition to these mixed results, in most cases there are not statistically significant interactive effects with the at-risk classification. Together, then, there is at best weak evidence that EE programs are more effective for at-risk youths.

Table 8. Effects of EE participation on schooling-related outcomes at Wave 6, at-risk versus others

	High school dropout (1)	High school graduation (2)	Would like to achieve at least a 4-year college degree (3)	College attendance (4)	4-year college attendance (5)
Non-nuclear family					
Any EE	–0.027 (0.032)	0.125 (0.052)	0.127 (0.058)	0.052 (0.068)	0.136 (0.072)
Any EE x non-nuclear family	–0.061 (0.043) [0.003]	0.036 (0.066) [0.001]	–0.096 (0.070) [0.429]	0.030 (0.093) [0.209]	–0.080 (0.094) [0.379]
n	528	496	503	394	389
Mathematics scores below median					
Any EE	–0.075 (0.023)	0.142 (0.040)	0.035 (0.039)	0.045 (0.053)	0.045 (0.057)
Any EE x low mathematics scores	0.022 (0.032) [0.100]	0.015 (0.053) [0.004]	0.080 (0.060) [0.024]	0.073 (0.086) [0.134]	0.124 (0.080) [0.016]
n	528	496	503	394	389
Reading scores below median					
Any EE	–0.052 (0.022)	0.122 (0.039)	0.079 (0.035)	0.100 (0.054)	0.087 (0.055)
Any EE x low reading scores	–0.037 (0.038) [0.016]	0.069 (0.054) [0.001]	–0.032 (0.059) [0.400]	–0.092 (0.084) [0.909]	0.005 (0.081) [0.214]
n	528	496	503	394	389
Mother's education high school or less					
Any EE	–0.072 (0.041)	0.158 (0.060)	–0.003 (0.051)	–0.080 (0.077)	–0.051 (0.090)
Any EE x low mother's education	0.029 (0.052) [0.171]	–0.026 (0.079) [0.005]	0.064 (0.069) [0.239]	0.242 (0.103) [0.029]	0.233 (0.113) [0.045]
n	416	388	399	315	311

Notes: See notes to Table 3. Other variables included are the same as in those specifications. Linear probability model estimates with bootstrapped standard errors are reported. For the interactions, the p value for the sum of the main and interactive EE participation variables is reported below the standard errors in square brackets. Only high school graduates are included in the last two columns, as in Table 6. For the last specification, low mother's education, observations for which mother's education is unknown or not reported are dropped.

Finally, we also examined the impact of program participation on academic outcomes for males and females separately. Many of the point estimates of the effects were quite close by gender, and none were significantly different. However, the estimated impacts of EE program participation in reducing high school dropout and increasing high school graduation were larger for males, by a factor of nearly two, providing a hint of more beneficial effects for them on this one outcome.[18]

Summary and Conclusion

We estimate the effects of programs that encourage high school graduation and most importantly higher education in the Philadelphia School District, a large and predominantly minority urban district. Our data come from a large and representative sample of Philadelphia high school students that was collected annually from 1996 when a random sample was drawn of students at the end of the eighth grade (the PELS). The data point to positive effects of these EE programs on high school graduation and on both academic and non-academic awards in high school, and similar negative effects on dropping out of high school. The results also suggest positive effects on aspirations for higher education and on college attendance.

Some programs may be more effective than others, but the variability of effects generally did not differ significantly, suggesting that the effects were almost invariably in the predicted direction. We did discover that the setting of the programs matters: the impact of the programs was generally strong in the neighborhood schools that most students attend, but absent in the magnet schools. For the most part, the effects were similar for males and females, although the impact on high school graduation may be more pronounced for males. In addition, there is some indication—although the evidence is quite weak and not always consistent—that these programs are more effective for at-risk youths.

An obvious challenge to these estimates is whether they can be interpreted causally. The evidence does not come from either a random assignment study or an instrumental variables approach exploiting a source of exogenous variation in program participation, so there is the potential for selective participation in the programs we study that may bias their estimated effects. We do, however, have an extensive set of controls in the PELS, including information on family background and structure, early test scores, and, perhaps most important, information about attitudes and expectations toward education that are elicited prior to program participation. Under some assumptions, these latter proxies, in particular, may fully account for selective participation in these programs, and these assumptions are not necessarily less defensible than those that would underlie an instrumental variables strategy. At a minimum, we have certainly controlled for a wide variety of characteristics of individuals that may be associated with educational outcomes and program participation, and the estimates are robust to including different subsets of controls, as well as implementing propensity score-matching estimators. This does not permit us to rule out the possibility that our results are generated by selection on unobservables, but it does make it more difficult to construct arguments about the nature of unobservables that could generate our findings.

Ultimately, there is a need for random assignment studies of such programs, or quasi-experimental research designs that give rise to compelling instrumental variables, to provide evidence complementary to what we report. But given the importance of trying to improve outcomes for students in large urban school districts in the United States, and given that the EE programs we study do not seem simply to select the students most likely to succeed in high school and go on to college, but rather to have beneficial effects (at least as far as we can tell in non-experimental data), the conditions are ripe for such experimentation and further research. Similarly, any further evaluation efforts should be coupled with research of a more qualitative kind that seeks to develop a better understanding of the nature of the programs we study and how and why they work, both for purposes of developing additional hypotheses about their effects and for testing which types of program practices are most beneficial.

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Notes

1. The data have also been augmented with administrative records from the criminal justice system, birth records, and unemployment insurance, although that information is not used in this paper.
2. Where the young adult is not interviewed because he or she could be reached after 20 or more attempts, we collect information on schooling and work from a parent in order not to lose PELS outcome data on participants. However, the parent did not receive the full survey, and those observations are therefore not used in this paper.
3. The longer working-paper version of this study (Furstenberg and Neumark, 2005) reports results for less restrictive samples, and more generally reports a larger set of analyses, specifications, and so on. However, the conclusions are no different from those reported in this paper.
4. As might be expected, participation in EE programs was slightly higher for those interviewed in all of Waves 3, 4, and 5, instead of at least one of these waves, as reported in Table 1. This suggests a modest bias for the programs selecting more stable and committed students; alternatively, programs may have increased school attendance and thus led to a greater likelihood of responding to the survey. (In general, we had somewhat lower success in maintaining involvement in the survey among the students who dropped out or did not attend school on a regular basis.) However, as reported in the longer working-paper version of this study (Furstenberg and Neumark, 2005), the results were not sensitive to restricting attention to students who participated Waves 3–5 (i.e., every wave in which EE program information was collected).
5. An important exception in the broader school-to-work literature is the Manpower Demonstration Research Corporation's random assignment study of the effects of career academies (Kemple, 2001, 2004; Kemple and Snipes, 2000).
6. For a demonstration of this problem in the context of school-to-work programs in the United States, see Neumark and Rothstein (2006).
7. In Furstenberg and Neumark (2005) we show estimates from a variety of models including and excluding various sets of controls; the results are robust.
8. One can easily recover the mean for non-participants, as well as for omitted categories (e.g., male, white, lives with two parents) from the information given in Table 2.
9. The linear probability model is less dependent on distributional assumptions than a probit or logit model, although we verified in this case and all others that the results were very similar using a probit model. Because conventional ordinary east-squares standard errors are incorrect for the linear probability model, standard errors are bootstrapped; for consistency, we do the same even when we estimate models for continuous dependent variables.
10. We implemented this using the PSMATCH2 module in Stata 9.0, using all defaults unless otherwise specified (Leuven and Sianesi, 2003).
11. For different samples, this occurred for black, Asian/other, living arrangement, test scores, mother's education, and the educational attitudes and expectations questions.

12. We verified that results were nearly identical without imposing the common support condition.
13. On the other hand, as of Wave 6 the administrative and self-reported data show similar dropout rates and similar graduation rates, and regressions using the administrative data on dropout and graduation yield quite similar results to those in Table 3.
14. In Furstenberg and Neumark (2005), we present an explicit analysis of attrition bias based on selective attrition on observables, and the results are unchanged.
15. Interestingly, the educational attitudes and expectations proxies enter more strongly for the lowest level of education with which the respondent would be satisfied than for either the first aspirations measure or educational expectations. The difference suggests that these proxies may in some sense be most strongly related to tastes for higher education, and may also help explain why the propensity score-matching estimate falls more relative to the regression estimate for the level of education with which the respondent would be satisfied than for the outcomes we study (in Table 5 and the other tables).
16. Of course, we should not pay too much attention to slight differences in the level of statistical significance, as small differences in the sample, the specification, or even the bootstrapping can result in small changes in standard errors.
17. For these outcomes, the role of the educational attitudes and expectations proxies is considerably weaker; in no case are these jointly significant, and the *p*-values are quite large compared with any of the earlier tables. One interpretation of this evidence is that actual circumstances—financial and otherwise—play more of a role in college attendance than in high school completion or aspirations regarding post-secondary education.
18. See Furstenberg and Neumark (2005) for detailed estimates.

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