What's in a Job? Evaluating the Effect of Private Sector Employment Experience on Student Academic Outcomes

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Online Appendix

A. Program Context

As a Workforce Development Board and a school-to-career intermediary, the Boston Private Industry Council (PIC) provides a variety of work-based learning activities for Boston Public School (BPS) students. Starting in the fall, PIC career specialists conduct outreach in 31 BPS high schools, working with roughly 2,600 students to prepare them for work through a series of work readiness workshops and career exploration activities. At the same time, the PIC employer engagement team secures hiring commitments from over 150 top employers, which range from Fortune 500 companies to hospitals to technology start-ups. In the spring of 2015, PIC staff coach students to apply to at a least three private sector internships based on their interests and skillsets and all applicants are given the opportunity to interview before employers make their final selection.[1] During the summer of 2015, the PIC made 1,301 direct employer-paid internship placements across 155 private sector firms. Of these, 726 students were in grades 8 through 11 during the year prior to the program and we were able to match almost all of them (723) to the DESE data.[2]

These employment experiences expose students to potential career paths in a wide array of professional workplace settings across key industries such as healthcare; finance, insurance, and

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[1] Students who are not placed in a private sector internship are offered subsidized employment with a nonprofit organization in the community or coached through the process of applying for jobs outside of the PIC's network of employers.

[2] Among the 1,301 students placed in a private sector internship for the summer, we also exclude form our analytic sample those who also had a summer job through the Boston SYEP (45 students).

real estate; and professional, scientific, and technical services which account for 75 percent of the PIC summer internships. Within these industries students worked in a variety of settings including hospitals, banks and finance companies, law firms, life science companies, and technology firms (Boston Private Industry Council 2015).[3] Students typically work 30-35 hours per week for six weeks, from early July through mid-August, and are paid at least the Massachusetts minimum wage.[4] To ensure firms are providing meaningful employment experiences, the PIC encourages employers to assess students across six skill categories using the Massachusetts Work-Based Learning Plan (WBLP) with youth typically show large improvements in critical thinking and problem solving; teamwork and collaboration; and understanding workplace policy, culture.[5]

B. Quasi-Experimental Design and Data Sources

1. Quasi-Experimental Design

Of the handful of studies evaluating the impact of private sector internships to date, all but one have a quasi-experimental design and ours is no exception. This is because randomly assigning youth to jobs would undermine the credibility of the PIC brokering process which is essential to maintaining consistently strong private sector engagement every summer. In the absence of an experimental design, students who are ultimately placed in a private sector internship by the PIC are likely to differ across both observable and unobservable characteristics

[3] Top employers included Massachusetts General Hospital, Tufts Medical Center, State Street Corporation, Bank of America, the Federal Reserve Bank of Boston, Boston Bar Association, Vertex Pharmaceuticals, Sanofi Genzyme, and General Electric.

[4] The context in which the Boston SYEP was delivered during the summer of 2015 is noteworthy. In 2015 the Massachusetts minimum wage was $9 per hour. In rare instances, some employers may offer a higher wage depending on the student's qualifications. Despite the labor market having largely recovered from the 2007-2008 Great Recession, the youth unemployment rate remained elevated at 8.7 percent in Massachusetts (Governing Magazine. Youth Unemployment Rate, Figures by State. https://www.governing.com/archive/youth-employment-unemployment-rate-data-by-state.html).

[5] These include attendance and punctuality, motivation and initiative, communication, teamwork and collaboration, critical thinking and problem solving, and workplace policy, culture, and safety.

that affect both their decision to apply as well as their likelihood of being chosen by an employer. However, the direction of this selection bias is unclear a priori. On the one hand, students who apply to the PIC program might be those who are less able to secure a private sector internship on their own which might negatively affect their post-program outcomes relative to the general population of the BPS students. On the other hand students who are ultimately selected for an internship might be those who are more motivated or career oriented which might positively affect their post-program outcomes relative to the general population of the BPS students.

2. Data Sources

Yet unlike other studies, we have access to a rich set of administrative data that enable us to identify the direction of the selection bias and employ several empirical strategies to address these concerns. Because the PIC works closely with BPS high schools, they are able to obtain each student's unique State Assigned Student Identifiers (SASID) that can be used to match PIC participants to school records maintained by the Massachusetts Department of Elementary and Secondary Education (DESE). These records include information on all public-school students within the state of Massachusetts, including those attending charter schools, reporting their attendance, course grades, statewide test scores, dropout status, and high school graduation for one year prior to and up to four years after participation in the program. DESE merges these data with records from the National Student Clearinghouse (NSC) which provides information on post-secondary outcomes including college enrollment and persistence by institution type (e.g., two-year versus four-year, and private versus public). Table A1 provides a full list of outcomes used in this study including how they were constructed from each of the administrative data sources.

A simple comparison of the baseline characteristics between the PIC participants and the BPS student population reveals that the selection bias is not so clear cut. Table A2 shows that PIC participants are typically older with a greater share of students in grades 11 or 12 which aligns with anecdotal evidence that employers tend to select more experienced students whereas younger students are more likely to be placed in an entry-level subsidized position either by the PIC or through the Boston SYEP. Yet PIC participants are by no means more advantaged than the general BPS population with a greater proportion (nearly 70 percent) who are non-white or of low socioeconomic status (as proxied by their receipt of free or reduced priced lunch) and a higher share who are male (62 percent)—all characteristics that are correlated with a lower likelihood of attending college (Autor and Wasserman 2013). However, examining the pre-program baseline outcomes helps to resolve the ambiguity of the direction the selection bias. During the school year prior to their internship, PIC participants have higher attendance rates than the general BPS population, which would positively affect both the student's decision to apply and their likelihood of being selected by an employer, as well as their post-program outcomes.

C. Empirical Strategy

Given that PIC participants are positively selected relative to other students attending the same BPS high schools, simply using OLS would produce estimates of the PIC program that are upwardly biased. To address these selection issues, we use a quasi-experimental design to disentangle these confounding factors to generate a range of estimates that bound the causal relationship between private sector summer job participation and high school students' academic outcomes. For high-school outcomes that can be measured repeatedly over time, such as attendance, we generate estimates using OLS, matching, and fixed effects models to bound our estimates of the impact of receiving a private sector placement on student academic outcomes. We use the fixed effects model as a way to validate our preferred matching model which uses Mahalanobis distance matching (MDM) that incorporates both observable characteristics such as school, grade, and race as well as proxies for unobservable characteristics using pre-program attendance. We then use this matching model to estimate the impact of having a private sector placement on outcomes for which we cannot use a fixed effects approach such as on-time high school graduation and post-secondary enrollment, that occur only once.

A. Fixed Effects Models

For outcomes that can be measured repeatedly over time such as attendance and course failures, we generate fixed effects estimates using equation (1):

$$Y_{it} = \alpha_i + \beta_1(T_i * post_t) + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the outcome variable[6], T_i is the treatment indicator for students placed in a private sector internship by the PIC.[7] The student fixed effect is captured by α_i and $post_t$ is an indicator equal to 1 if the academic year, t, is 2015-16 and 0 otherwise. The error term ε_{it} capture the standard errors which are robust and clustered at the student level. The coefficient of interest is β_1 which captures the change in the outcome over time for PIC participants relative to other BPS students. A positive and significant coefficient indicates that participating in a private sector internship improves students' academic performance.

[6] When the outcome variable Y_i is dichotomous (e.g., course failure), equation 1 is run as a logistic regression.

[7] Note that we do not control for changes in academic characteristics such as ELL or special education status since there are no or only one or two of students who change status. We are do not control for changes in free and reduced price lunch status since Boston Public Schools subsequently moved to a universal free school lunch program. In addition, prior research has shown that ever having received free or reduced price lunch is a better measure of socioeconomic status than the contemporaneous measure since students who dip in and out of eligibility due to small changes in household income remain low-income.

B. Matching Models

We also make use of a matching model that generates a comparison group of BPS students to provide a plausible counterfactual against which to measure the effect of having a PIC-brokered private sector internship. Our preferred model (MDM) matches observations based on minimizing the distance between a vector of observed covariates for PIC participants and the general population of BPS students. For comparison, we also implemented two other matching techniques—coarsened exact matching (CEM), and propensity score matching (PSM).

1. Mahalanobis Distance Matching (MDM)

MDM is built on a notion of matching observations based on minimizing the distance between the vector of covariates for treated and control units. Specifically, MDM matches are created by minimizing the Mahalanobis distance measure, $M(X_i,X_j)$, between two vectors of characteristics X_i and $X_j \in X$ for individuals *i* and *j*, respectively:

$$M(X_i, X_j) = \sqrt{(X_i - X_j)' S^{-1} (X_i - X_j)},$$

where S^{-1} is the inverse of the sample covariance matrix of X.

We know from Table A2 that observable characteristics such as gender, grade, race, English proficiency, and socioeconomic status are correlated with applying to the PIC program and se we include them in our matching model. In addition, to control for the heterogeneity between schools and the exposure that students get from the PIC career specialists, we also match on which school each student attended prior to the program. Nevertheless, we also know from Table A2 that there are likely to be unobservable factors beyond the scope of our dataset such as motivation that may affect both selection into the program as well as post-program outcomes. To address this concern, we also match on baseline pre-program attendance to proxy for these unobservable characteristics.

When implementing MDM, we conduct one-to-many matching, allowing for a vector distance between covariates of treatment and control units of up to 1.5. We experimented with a series of alternative bandwidths and found that between a bandwidth of 1.25 and 1.5, we get improved balance and a larger number of matches, as indicated by the number of covariates with significant differences post-matching. Between a bandwidth of 1.5 and 1.75, although we get more matches per treatment, our matches are less balanced due to the larger number of observations in our regressions.

2. Coarsened Exact Matching (CEM)

CEM is a "Monotonic Imbalance Bounding" (MIB) class of matching methods for causal inference, introduced by Iacus, King and Porro (2011). Unlike MDM, CEM guarantees that the imbalance between the matched treated and control units will be bounded ex ante at a user-specified level. CEM is implemented by, first, temporarily coarsening each covariate when two values of a particular variable are substantially indistinguishable. For example, when matching on students' prior attendance records, attendance days may be coarsened into increments of 3 days because a student who attended 97 days of school is likely not substantively different from one who attended 94 days. This coarsening allows for better matches and less trimming of the dataset. Next, strata are formed, where units with the same values for all coarsened characteristics are grouped together.

To illustrate, in our dataset, all female, Asian students in grade 10, with perfect attendance records are likely placed in the same stratum. When a stratum does not have at least one treated unit and one control unit, it is pruned from the dataset. Weights are assigned as follows: treated units receive a weight of one. Control units are weighted as the number of treated units in its stratum divided by the number of control units in the same stratum, normalized so that the sum of the weights equals the total matched sample size[8].

For the purpose of our analysis, we restrict further coarsening on all of our covariates except for continuous baseline variables in select models such as pre-period attendance days, truant days, weighted mean GPA, and course failures. In other words, we are forming strata with exact matches on the dichotomous variables for students' gender, race, grade, school, and participation in ELL, special education, and free or reduced lunch. Once strata are formed and unmatched units are pruned, we proceed to estimating the average treatment effect on the treated.

3. Propensity Score Matching (PSM)

PSM is the approach most commonly used for estimating causal effects from non-experimental data (Heckman 1977; Rosenbaum and Rubin 1983). Two conditions must be met for PSM to correctly estimate the impact of a program: (1) the Condition Independence Assumption (CIA) and (2) the Common Support Condition (CSC). The CIA holds when observations are assigned to treatment based only on observable characteristics. If CIA doesn't hold, or in other words, participation in the treatment is likely driven by unobservable factors, then the matching estimator may be seriously biased. Given our rich administrative dataset that encompasses many observable characteristics that would predispose students' assignment to treatment, we believe CIA is satisfied. Unobservable factors such as motivation or ability that affect selection are proxied for using baseline attendance and GPA in relevant models.

The CSC requires that a substantial overlap exist between propensity scores of treated and untreated individuals. If this condition fails to hold, then we cannot construct a counterfactual comparison group to estimate the impact of the intervention. In our setting, only those enrolled in

[8] Thus, in a stratum containing one PIC participant and 5 matches, each matched unit will receive a weight of one-fifth.

a Boston public high school were eligible to participate in PIC and, to our knowledge, each BPS is assigned at least one career coach responsible for disseminating information about the PIC summer jobs program to students. To ensure that untreated students in our sample would have some propensity to participate in the program, and to meet the CSC requirement, we limit our comparison group to only schools that have at least one PIC participant.

4. Comparison of Matching Models

Matching across any of these three techniques significantly reduces the difference in means between the treatment and comparison groups, although not entirely. The first column of Table A3 shows the mean difference in pre-program characteristics between PIC participants and the unmatched population of BPS students attending the same schools who also appear in the data post-program. The remaining three columns show the differences between the PIC participants and the comparison group generated using each of the three matching techniques. While PSM results in the fewest number of significant differences between the treatment and comparison groups, this technique leaves some key demographic characteristics mis-matched such as gender and also results in far fewer observations thereby reducing power. Both the CEM and MDM techniques match more precisely on demographic characteristics but not on grade level. Although our preferred specification is MDM because this method offers the greatest flexibility and power, we show in the appendix that all three techniques yield very similar results.

While PSM appears superior when considering post-matching balance in our sample, Nielsen and King (2019) point out that the method has several weaknesses, and should be used only in conjunction with other matching methods. One weakness is that PSM approximates random matching, rather than a fully blocked experiment. Complete randomization balances the treated and untreated units on average, whereas a fully blocked experiment exactly balances the covariates for the observed treated and untreated units. King et al. (2009) find that standard errors in a fully blocked experiment are, on average, 600% smaller. PSM is efficient relative to complete randomization, but it is inefficient compared to a fully blocked experiment.

PSM also suffers from the “PSM Paradox": as the propensity to be treated or untreated approximates randomization (or a propensity score of 0.5), it gives rise to more pruning at random, which in turn increases imbalance, inefficiency, model dependence, and bias. In addition, PSM is susceptible to the curse of dimensionality: as the number of covariates increases, the logit regression may become worse at predicting the probability of treatment (especially with irrelevant covariates), and the PSM paradox gets significantly worse. These weaknesses are attributable to PSM’s two-stage procedure. Much valuable information is lost in the first-stage logit regression, where all covariates playing a strong role in selection into treatment are reduced to a one dimensional propensity score.

MDM and PSM are similar in that for both methods, the researcher may set the caliper width and number of matches per treatment for matching ex ante. Matching and checking imbalance occurs ex post. For CEM, the researcher specifies the desired balance ex ante by restricting matching within strata based on a collection coarsened covariate characteristics. The number of matches are realized ex post. King et al. (2011) conclude that researchers should not necessarily discard PSM as a matching method, but use it in combination with other techniques to compare results.

MDM and CEM both approximate a fully blocked experiment. In the literature, it is not quite clear which method is more superior. After iterating on multiple caliper bandwidths for MDM, we find that post-matching balance is very similar to that of CEM. In addition, for our key academic outcome variables, our MDM regressions capture more matches and, thereby, have more power than our CEM regressions. We find that MDM allows for more flexibility and precision in matching because we can specify which covariates we wish to exactly match on as well as the bandwidth of the Mahalanobis distance. With these considerations in mind, our preferred specification for our analysis is MDM.

D. Additional Results Not Reported in Main Text

Tables A4-A7 report additional results from the models described above for both secondary and post-secondary academic outcomes that were not included in the main text due to constraints on length. For attendance outcomes that can be observed repeatedly over time, we compare results across the naïve OLS estimation model, the matching (MDM) model, and the fixed effects (FE) model.[9] Table A4 shows compares the results across the different models, revealing the degree of positive selection present. The naïve OLS model results in column (1) would suggest that students participating in a PIC brokered private sector internship during the summer of 2015 attended nine additional school days compared to the general BPS student population during the year after participating in the program, driven in part by reductions in truancy, thereby boosting attendance rates by 4 percentage points. However, once we control for both demographics and baseline attendance, these post-program improvements are reduced by one-third to one-half. Employing the MDM matching model further reduces these gains to just 2 additional days of attendance with only one less day of truancy. The fixed effects (FE) model confirms the MDM results and even suggests that the matching model may be over-controlling by limiting the comparison to students attending the same schools. This gives us some confidence that the matching model can generate plausible estimates and be applied to other outcomes for which there are no baseline (pre-program) observations.

For other outcomes for which we lack baseline data, we present results comparing the naïve OLS model to our preferred matching model (MDM). Table A5 shows that when we examine the impact of the PIC program on student performance on the Massachusetts Comprehensive Assessment System (MCAS), a statewide standardized test, we find only a small increase of just a few points in the normalized test

[9] A full comparison of our results using the PSM and CEM models are available upon request.

scores relative to the MDM comparison group. Our MDM model indicates that while this boosts the percentage of students deemed "proficient" by the MCAS, it does not make it more likely that students will receive a passing grade needed for graduation. One caveat is that because students typically take the MCAS during the spring semester of their sophomore year, we are only able to measure impacts on only rising ninth and tenth graders—about one-quarter of the PIC participants in our sample. In contrast, prior studies of the New York City SYEP are able to observe whether students take any of the annual statewide Regents exams and find small (1-3 percent) but significant increases in the likelihood of taking and passing both the math and ELA exams (Leos-Urbel 2014; Schwartz et al. 2021).

In terms of college preparation, we estimate the program's impact on SAT scores among those who choose to take the exam which was widely required when applying to college pre-pandemic, particularly among four institutions including the UMass system. Table A6 shows that PIC participants are 4 percentage points more likely to take the SAT than the comparison group of BPS students. However, neither the overall SAT score nor any of its sub-components showed any improvement for PIC participants compared to the MDM comparison group.

Subgroup analyses for the primary outcomes for which we found evidence of program impacts can be found in Table A7. PIC participants who attended regular BPS public schools experienced greater impacts compared to students attending the city's three exam schools (Boston Latin Academy, Boston Latin School, and the John D. O'Bryant School of Mathematics and Science). Column (4) of Table A7 shows that virtually all of the program's impacts on subsequent increases in college enrollment were driven by students attending regular BPS schools. These results are consistent with the anecdotal evidence that PIC brokered private sector internships places less advantaged students in professional settings with greater structure than entry level jobs and introduces them to a wider array of occupations and industries that require post-secondary education.

However, not all groups of students are affected similarly by their private sector experiences. For example, column (5) of Table A7 shows that while ELL students who participated in a PIC brokered private sector internship experienced marginal increases in attendance, they were slightly less likely to graduate from high school on time compared to the full group of PIC participants. This finding is consistent with other emerging research which suggests that private sector employment during the summer can actually slow down high school graduation for some students particularly if they continue to work during the school year (Heller and Kessler, 2022).

E. References

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Table A1. Definition of Outcome Measures

	Definition (1)	Time Period (2)	Source of Data (3)
Panel A. Short-Term Outcomes			
Attendance			
Attendance rate	Number of days attended / Number of days in membership at all schools	One year (2015-16) and post-program	Massachusetts administrative school records
Total days attended	Number of days attend in a school year		
Total days of unexcused absences (truancy)	Number of days of unexcused absences in a school year		
Standardized test scores - Math and ELA			
Normalized scaled score	Raw scores converted to standardized units (mean 0, variance 1)	Spring of 10th grade year post-program for 8th and 9th graders	Massachusetts administrative school records
Proficient or better	Score was classified as “proficient” or “advanced” by DESE in the exam year		
Failing	Score was classified as “failing” by DESE in the exam year		
Panel B. Mid-Term Outcomes			
High School Graduation			
Graduated on time during the post observation period	Graduated as expected by 12th grade given their pre-period grade level	Full post-program observation period 2015-16 through 2018-19 school years	Massachusetts adminsitrative school records
Dropout			
Dropped out one year post-program	Enrollment status listed as dropped out for any reason	One year post-program at end of 2015-16 school year	
College entrance exam scores			
Overall score	SAT raw overall score	Full post-program observation period 2015-16 through 2018-19 school years	Massachusetts adminsitrative school records
Math score	SAT math section score		
Reading score	SAT reading section score		
Writing score	SAT writing section score		
Panel C. Long-Term Outcomes			
College Enrollment			
Any	Student enrolled in college at any time after graduation from high school	Enrollment data range between May 2015 and August 2019.	National Student Clearinghouse
Two-Year	Student enrolled in a two-year college		
Four-year	Student enrolled in a four-year college		

Table A2. Descriptive Statistics: Baseline Characteristics

	BPS Students		PIC Participants		Difference	
	Mean	Std. Dev.	Mean	Std. Dev.	(Treated - Untreated)	
Demographic Characteristics						
Male	0.48	0.50	0.64	0.48	0.162	***
Female	0.52	0.50	0.36	0.48	-0.162	***
Race: Asian	0.10	0.30	0.17	0.38	0.071	***
Race: Black	0.32	0.47	0.44	0.50	0.121	***
Race: Hispanic	0.30	0.46	0.27	0.44	-0.028	
Race: White	0.23	0.42	0.07	0.25	-0.165	***
Race: Other	0.05	0.22	0.05	0.22	0.001	
Grade: 8	0.07	0.26	0.01	0.10	-0.062	***
Grade: 9	0.35	0.48	0.09	0.29	-0.258	***
Grade: 10	0.29	0.45	0.27	0.44	-0.022	
Grade: 11	0.28	0.45	0.62	0.48	0.343	***
Free or Reduced Lunch	0.55	0.50	0.69	0.46	0.139	***
English Language Learner (ELL)	0.18	0.39	0.13	0.34	-0.051	***
Special Education	0.18	0.38	0.12	0.32	-0.062	***
Baseline (pre-program) Outcomes						
Attendance Days 2014-15	149.22	48.32	168.87	14.59	19.653	***
Attendance Rate 2014-15	0.86	0.24	0.94	0.06	0.084	***
Truant Days 2014-15	10.94	19.50	7.43	10.23	-3.514	***
Truancy Rate 2014-15	0.07	0.16	0.04	0.06	-0.033	***
Course Failure	0.33	0.47	0.22	0.42	-0.108	***
	17,549		722		18,271	

Note: The sample includes youth who were in grades 8 through 11 prior to the program and able to be matched to the school record data. *Indicates significance at the 10 percent level, **at the 5 percent level and ***at the 1 percent level.

Source: Program participation data were provided by the Boston Private Industry Council. Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table A3. Sample Balance Pre- and Post-Matching

	Difference in Means (Treated - Untreated)			
Covariates	Unmatched	PSM Matched	CEM Matched	MDM Matched
Male	0.162 ***	0.085 ***	0.019	0.028
	(0.019)	(0.020)	(0.023)	(0.020)
Female	-0.162 ***	-0.085 ***	-0.019	-0.028
	(0.019)	(0.020)	(0.023)	(0.020)
Free or Reduced Lunch	0.139 ***	-0.002	0.020	-0.020
	(0.019)	(0.019)	(0.021)	(0.019)
English Language Learner (ELL)	-0.051 ***	-0.034 **	0.023 *	0.021
	(0.015)	(0.015)	(0.014)	(0.013)
Special Education	-0.062 ***	-0.038 ***	0.031 ***	0.070 ***
	(0.014)	(0.014)	(0.010)	(0.010)
Race: Asian	0.071 ***	0.024 *	0.000	0.023
	(0.011)	(0.014)	(0.018)	(0.015)
Race: Black	0.121 ***	0.023	0.047 **	0.010
	(0.018)	(0.020)	(0.023)	(0.021)
Race: Hispanic	-0.028	-0.027	0.010	-0.014
	(0.017)	(0.018)	(0.020)	(0.019)
Race: White	-0.165 ***	-0.020 *	-0.070 ***	-0.058 ***
	(0.016)	(0.011)	(0.015)	(0.013)
Race: Other	0.001 ***	-0.001	0.013 **	0.039 ***
	(0.008)	(0.009)	(0.005)	(0.006)
Grade: 8	-0.062 ***	-0.005	-0.005	-0.002
	(0.010)	(0.005)	(0.005)	(0.005)
Grade: 9	-0.258 ***	-0.055 ***	-0.027 *	-0.038 ***
	(0.018)	(0.014)	(0.015)	(0.014)
Grade: 10	-0.022 ***	-0.079 ***	-0.076 ***	-0.070 ***
	(0.017)	(0.019)	(0.022)	(0.020)
Grade: 11	0.343 ***	0.138 ***	0.108 ***	0.111 ***
	(0.017)	(0.020)	(0.023)	(0.021)
Attendance Days 2014-15	19.653 ***	4.382 ***	-0.204	-0.838 *
	(1.802)	(0.869)	(0.512)	(0.488)
Observations	18,271	5,333	2,725	3,412

Note: The sample includes youth who were in grades 8 through 11 prior to the program and able to be matched to the school record data. *Indicates significance at the 10 percent level, **at the 5 percent level and ***at the 1 percent level.

Source: Program participation data were provided by the Boston Private Industry Council. Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table A4. Estimates of Program Impact on School Attendance One Year Post-Program

	Coefficient on Participant Dummy				
	OLS1	OLS2	OLS3	MDM	FE
Total days attended	9.295 ***	9.377 ***	6.148 ***	2.272 **	3.696 ***
	(0.809)	(0.875)	(0.773)	(0.811)	(0.750)
Attendance rate	0.038 ***	0.040 ***	0.024 ***	0.008 **	0.034 ***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Total days truant	-2.962 ***	-4.524 ***	-1.840 ***	-1.100 *	-1.184 **
	(0.003)	(0.004)	(0.489)	(0.648)	(0.477)
Truancy rate	-0.026 ***	-0.032 ***	-0.018 ***	-0.007 **	-0.008 **
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
Percent failing a course	-0.145 ***	-0.147 ***	-0.070 **	-0.037 *	-0.057 **
	(0.033)	(0.027)	(0.025)	(0.015)	(0.017)
<u>Controls/Matching Variables</u>					
Demographic/academic characteristics	No	Yes	Yes	Yes	No
Baseline (pre-program) attendance outcomes	No	No	Yes	Yes	Yes
School attended pre-program	No	Yes	Yes	Yes	No
Number of PIC participants	722	722	722	722	722
Number of BPS comparison youth	17,549	17,549	17,549	2,690	17,549
Total number of observations	18,271	18,271	18,271	3,412	18,271

Note: The sample includes youth who were in grades 8 through 11 prior to the program and able to be matched to the school record data. Each coefficient is from a separate regression where the dependent variable is the outcome listed. The demographic and academic characteristics are those listed in Table A2. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors, clustered at the student level, are in parentheses. *Indicates significance at the 10 percent level, **at the 5 percent level and ***at the 1 percent level.

Source: Program participation data were provided by the Boston Private Industry Council. Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table A5. Estimates of Program Impact on Standardized Test-Taking Required for High School Graduation

	OLS3	MDM
Percent taking MCAS exam	0.023 *	0.004
	(0.012)	(0.005)
Normalized scaled score		
Math	3.785 ***	3.183 **
	(0.844)	(1.027)
ELA	3.132 ***	2.869 ***
	(0.475)	(0.500)
Percentage scoring proficient or better		
Math	0.081 **	0.068 **
	(0.031)	(0.023)
ELA	0.082 **	0.066 **
	(0.026)	(0.021)
Percent failing the exam		
Math	-0.041 **	-0.025
	(0.019)	(0.015)
ELA	-0.045 *	-0.019
	(0.027)	(0.055)
Controls/Matching Variables		
Demographic/academic characteristics	Yes	Yes
Baseline (pre-program) attendance outcomes	Yes	Yes
School attended pre-program	Yes	Yes
Number of PIC participants	722	722
Number of PIC participants taking MCAS exam	250	250
Number of BPS comparison youth	8,612	1,085
Total number of observations	8,862	1,335

Note: The sample includes youth who were in grades 8 through 11 prior to the program and took the MCAS exam post-program. Test scores are conditional on having taken the exam post-program. Each coefficient is from a separate regression where the dependent variable is the outcome listed. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors, clustered at the student level, are in parentheses. *Indicates significance at the 10 percent level, **at the 5 percent level and ***at the 1 percent level.

Source: Program participation data were provided by the Boston Mayor's Office of Workforce Development (OWD). Administrative data on school records were provided by the Massachusetts Department of Elementary

Table A6. Estimates of Program Impact on SAT College Entrance Exam Scores

	OLS3	MDM
Percent taking SAT exam	0.048 ***	0.041 **
	(0.007)	(0.013)
SAT raw score		
Overall	25.130 *	17.270
	(10.410)	(12.220)
Math	8.857 *	5.261
	(4.036)	(3.128)
Reading	7.092	4.392
	(4.132)	(5.584)
Writing	9.301 *	7.746
	(4.080)	(5.746)
Controls/Matching Variables		
Demographic/academic characteristics	Yes	Yes
Baseline (pre-program) attendance outcomes	Yes	Yes
School attended pre-program	Yes	Yes
Number of PIC participants	722	722
Number of PIC participants taking SAT exam	412	412
Number of BPS comparison youth	3,150	1,192
Total number of observations	3,562	1,604

Note: The sample includes youth who were in grades 8 through 11 prior to the program and took the MCAS exam post-program. Test scores are conditional on having taken the exam post-program. Each coefficient is from a separate regression where the dependent variable is the outcome listed. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors, clustered at the student level, are in parentheses. *Indicates significance at the 10 percent level, **at the 5 percent level and ***at the 1 percent level.

Source: Program participation data were provided by the Boston Private Industry Council. Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).

Table A7. Estimates of Program Impact on Outcomes by Subgroup using the MDM Matching Model

	Male	Black	Hispanic	Non-Exam Schools	ELL	Low-Income
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome = Days Attended						
SYEP 2015 participant dummy	0.241	1.428	2.228	2.037	3.048 *	1.473
	(2.007)	(1.567)	(1.306)	(1.469)	(1.183)	(1.792)
SYEP 2015 participant dummy * group dummy	3.811	2.775	1.722	1.074	-2.940	1.739
	(2.035)	(2.067)	(2.128)	(1.980)	(4.439)	(1.884)
N comparison	2,690	2,690	2,690	2,690	2,690	2,690
N treated	722	722	722	722	722	722
N treated * subgroup	460	320	193	455	95	497
Outcome = Graduated on time during follow-up period						
SYEP 2015 participant dummy	0.038 **	0.047 **	0.056 ***	0.064 *	0.070 ***	0.038
	(0.014)	(0.016)	(0.010)	(0.030)	(0.013)	(0.025)
SYEP 2015 participant dummy * group dummy	0.026	0.012	-0.009	-0.012	-0.048 **	0.019
	(0.021)	(0.022)	(0.025)	(0.032)	(0.019)	(0.027)
N comparison	2,690	2,690	2,690	2,690	2,690	2,690
N treated	722	722	722	722	722	722
N treated * subgroup	460	320	193	455	95	497
Outcome = College enrollment (ever enrolled)						
SYEP 2015 participant dummy	0.019	0.035	0.034 *	-0.024	0.029	0.022
	(0.029)	(0.022)	(0.017)	(0.028)	(0.019)	(0.038)
SYEP 2015 participant dummy * group dummy	0.033	0.005	0.011	0.083 ***	0.058	0.020
	(0.034)	(0.028)	(0.023)	(0.029)	(0.043)	(0.033)
N comparison	2,183	2,183	2,183	2,183	2,183	2,183
N treated	631	631	631	631	631	631
N treated * subgroup	407	277	166	389	71	436

Note : The sample includes youth who were in grades 8 through 11 prior to the program and able to be matched to the school record data. Probit is used to estimate results for binary outcomes. For these non-linear specifications, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors, clustered at the student level, are in parentheses. *Indicates significance at the 10 percent level, **at the 5 percent level and ***at the 1 percent level.

Source: Program participation data were provided by the Boston Private Industry Council. Administrative data on school records were provided by the Massachusetts Department of Elementary and Secondary Education (DESE).