The Effect of High School JROTC on Student Achievement, Educational Attainment, and Enlistment

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Abstract

JROTC is a high school initiative funded jointly by local school districts and the federal government. The program serves many at-risk students and its goals range from reducing dropout rates and improving academic achievement to preparing students for military careers. Using data from HSB and NELS we estimate average treatment effects on students attending schools that typically host JROTC. Applying a two stage matching technique, we find that JROTC participants have poorer academic outcomes than other students, although a large portion of these differences is explained by their at-risk status. In addition, program effects appear to differ by demographic group with black participants having lower dropout rates than white participants. The program also appears to improve self-esteem scores of females. Although the majority of JROTC participants do not join the military, we find large marginal enlistment effects.

THE EFFECT OF HIGH SCHOOL JROTC ON STUDENT ACHIEVEMENT, EDUCATIONAL ATTAINMENT, AND ENLISTMENT

1. Introduction

The Junior Reserve Officers' Training Corps (JROTC) is a program for high school students funded jointly by local school districts and the federal government. The national program was established in 1916, although individual programs date as far back as 1874. The program remained small until a major expansion was launched in the mid-1990s. By 2007 the JROTC program enrolled roughly 525,000 students in 3,400 high schools.[1]

The JROTC program is of interest to policymakers for several reasons. First, although it has elements of a military preparation program, its goals are multidimensional and include improving academic outcomes for high school students.[2] Understanding the program's effects on student achievement is especially important given the heavy enrollment of at-risk students: nearly 40% of the high schools that offer JROTC are located in inner-city areas and about one-half of enrollees are minorities. Another indicator of program diversity is that about 40% of enrollees are females. In addition, the program accepts many students who could not qualify for military enlistment (Coumbe, Kotakis, and Gammell 2008). These enrollment patterns in part explain why most students who ever participate in JROTC do not enter the military (about 70% in our data).[3] From a social cost-benefit perspective it is important to understand the full array of program impacts, including effects on both cognitive and non-cognitive outcomes as well as on employment in the military.

[1] Program data are taken from Crawford, Thomas, and Estrada (2004), Center for Strategic and International Studies (1999), Thomas-Lester (2005), and Coumbe, Kotakis, and Gammell (2008).
[2] For example, the Army's mission statement for JROTC is "provide the motivation and skills to remain drug free, to graduate from high school, and to become successful citizens"(Glover 2002).
[3] JROTC participants do not incur any obligation to enter the military. Although surveys indicate that 30-40 percent of JROTC *graduates* intend to enlist (see Bailey et al. 1992; Center for Strategic and International Studies 1999), only a fraction of those actually enlist, and only a fraction of those who ever participate in JROTC complete the full 4-year program.

A broader public policy issue revolves around the value of offering JROTC in public schools, which has been the subject of on-going debate. Proponents argue that the program improves both the cognitive and non-cognitive skills of participants. President George H. Bush described JROTC as a "…program that boosts high school completion rates, reduces drug use, and raises self-esteem…" (cited in Corbett and Coumbe 2001). JROTC includes elements that are similar to better-known high school initiatives, such as career academies and school-to-work programs. Some JROTC activities are comparable to other extracurricular high school activities, such as band, team sports, or school clubs, and many participants engage in community service after school and on weekends. Critics often oppose JROTC in public schools for philosophical reasons, but also on the grounds that there is no evidence that the program improves academic achievement (Lutz and Bartlett 1995).

The opponents are correct about the lack of research on JROTC. To date no analysis has attempted to assess the wider effects of the program using nationally representative data. This study evaluates the impacts of JROTC on a broad list of student outcomes, including in-school and post-school performance, based on data from the High School and Beyond (HSB) and the National Educational Longitudinal Study (NELS). We obtain estimates using a variety of techniques to control for the selection of individuals and schools into the program. Overall, we find mixed program effects on educational outcomes. While JROTC participants in general have poorer academic performance, the program appears to reduce dropout rates and improve graduation rates for black participants and self-esteem scores for female participants. We also find strong enlistment effects across all participants.

II. The Program[4]

[4] In this section we describe the basic features of JROTC that are relevant to our analysis. An in-depth program description is available in Coumbe, Kotakis, and Gammell (2008). Also, Crawford, Thomas, and Estrada (2004)

Prior to 1964 JROTC was a relatively small program operated by the Army. The ROTC Vitalization Act of 1964 expanded the program and extended it to all four military services. The goals of the program at its inception were to instill citizenship values and disseminate information about military careers. After the introduction of the All Volunteer Force in 1973 the program incorporated recruiting incentives by offering advanced military pay grades to those who completed at least two years of JROTC. Currently, Congress has capped the number of JROTC units and several hundred high schools are on a waiting list for a new unit (Coumbe, Kotakis, and Gammell 2008). An incentive for participation of local schools is that the federal government pays a portion of the program costs. In 2005, federal spending on JROTC was about $239 million (U.S. Department of Defense 2008). According to estimates, federal subsidies cover about 40 percent of program costs (Denver Public Schools 1996). Based on these figures, total JROTC spending at all levels would be about $730 million annually. The multidimensional aspects of the program, which combines elective credit with extracurricular activities, helps supplement the standard classroom and extracurricular offerings of local schools. The operation of each unit varies largely by school, military branch, and JROTC instructor. This induces a certain degree of randomness in the way that students enter the program.

Currently, little is known about the program at the national level and even less is known about the careers of participants who do not join the military. Program administrators do not track the careers or educational outcomes of participants. The only participants that can be tracked through administrative data sources are those who report JROTC participation upon enlistment (about 30% of all students who ever participate in the program). However, analyzing the effect of the program on all participants is important for a comprehensive program assessment. In addition,

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provide a comprehensive program overview based on surveys and interviews with students, parents, instructors, and school administrators.

program effects on non-enlistees have implications for local youth labor markets, especially in inner cities and in the South, where the majority of JROTC units are located.

III. Related Initiatives

Educational initiatives with some elements comparable to JROTC are those that assist the transition from school to employment. These include career academies and federally-funded school-to-work (STW) programs – reforms that stress both academic and vocational curricula and that establish formal links with employers. Evidence on the effects of these educational initiatives has been mixed. Attending career academies had no effect on high school completion or post-secondary education, but male attendees experienced higher earnings than non-attendees eight years after high school (Kemple 2008). Similarly, Neumark and Rothstein (2003) found that only two of six STW activities (school enterprises and job shadowing) increased college enrollment, whereas one activity (Tech Prep) reduced college enrollment. With respect to labor market outcomes, co-op education, school enterprises, and internships increased employment, especially for at-risk males (Neumark and Rothstein 2005).

Indirect evidence on the effect of JROTC on academic performance can be gleaned from a pilot program created in 1992 by the Departments of Education and Defense that combined career academies with required JROTC participation.[5] Elliott, Hanser, and Gilroy (2002) found few differences between students in JROTC Partnership Academies and those attending magnet schools or other career academies. However, JROTC Partnership Academy students had better attendance, grades, and graduation rates than students in a general academic track and not in a magnet school or other career academy. Also, outcomes were better for JROTC Partnership Academy students than for students in 'regular' JROTC. Finally, regular JROTC students

[5] The program, called "A Federal-Local Partnership for Serving At-Risk Youth," attempted to combine the strengths of JROTC in terms of discipline, leadership, and extracurricular activities with the career academy focus on work-based learning (Hanser and Robyn 2000).

performed on par with general track students not in a magnet school or in a career academy, suggesting that the career academy component, rather than the JROTC component, accounted for most of the success of the combined JROTC Partnership Academies.

Other analyses of the regular JROTC program have consisted primarily of case studies. One such study compared student outcomes in El Paso and Chicago inner-city schools (Center for Strategic and International Studies 1999). In Chicago, JROTC students performed no better in terms of attendance, grades or graduation rates, whereas in El Paso they had fewer disciplinary problems and better attendance, but lower test scores and college attendance. The analyses in these case studies do not attempt to estimate causal effects or consider the selection of students and schools into the program.

IV. The Data

The HSB and NELS surveys are the only data sets that provide national-level participation rates for JROTC.[6] In addition, these surveys span two decades, allowing for an assessment of changes in program impacts over time. This is important in the case of JROTC because the program expanded substantially in the 1990s.

From the HSB survey we use the sophomore component, which follows a representative sample of tenth graders from 1980 to 1992. About 14,825 sophomores randomly drawn from the original sample were re-interviewed in 1982, 1984, 1986, and 1992. The NELS surveyed a sample of eighth graders in the spring of 1988 from 1,052 schools. We focus on the subsample of 12,144 students who were re-interviewed in 1990, 1992, 1994, and 2000. Both surveys contain detailed information on student backgrounds, academic performance, and post-high school experiences. In addition, both surveys collected information from school officials on school characteristics such as enrollment, educational and special programs, dropout rates, and racial

[6] The NLSY97 does not clearly identify JROTC students in high school.

composition. We merged data from both surveys with information from the High School Transcript Survey to obtain information on courses completed and academic achievement. Transcript data were available for 13,024 HSB sophomores and 10,310 NELS students. Our empirical analysis restricts the samples to students in public schools, resulting in samples of 10,270 and 8,634 for HSB and NELS, respectively.

We use the base year in each survey (1980 for HSB and 1988 for NELS) to observe the characteristics of students and their families. We use transcripts to identify JROTC participants as those who completed at least one JROTC class. To evaluate academic performance, we focus on standardized test scores, graduation rates, and dropout rates.[7] Both surveys administered cognitive tests in reading, math, civics, and science.[8] We also look at non-cognitive outcomes, such as disciplinary problems (suspensions, expulsions, trouble with the law) and self-esteem (measured along the Rosenberg Self-Esteem Scale).[9] To evaluate post-secondary outcomes we focus on college enrollment, degree completion, military enlistment, and civilian employment. Both surveys follow and test high school dropouts, and also obtain transcripts for those who attended high school for at least one term.[10]

Table 1 presents weighted summary statistics for both survey samples. The data show that JROTC enrollees are more likely to be minority males from lower-income families with less-educated parents, and are more likely to live in single-parent households. They also attend

[7] Our definition of dropouts follows that of the National Center for Education Statistics (NCES) and includes students who leave school regardless of whether they later return. We treat GED-recipients as non-graduates.

[8] For HSB, we follow Evans and Schwab (1995) and focus on the sum of the 'formula' score on the vocabulary, reading, and the first part of the mathematics test. For NELS, we combine test scores in reading and math.

[9] Our indicators of in-school performance are selected to reflect the published goals of both JROTC and local school district administrators (Center for Strategic and International Studies 1999; Denver Public Schools 1996; Glover 2002; Bailey et al. 1992).

[10] Many dropouts still have missing values for various important variables. This problem is more severe with NELS data. To avoid bias from omitting dropouts, we impute values of time-invariant variables using responses to identical questions in later follow-ups. In addition, we include dummies for missing observations for important controls. We do not impute values of dependent variables or test scores.

predominantly urban schools with high minority enrollments located in the South. These attributes suggest that JROTC enrollees are more likely to be at-risk students.[11] It also appears that participation is correlated with having a parent in the Armed Forces.

The composition of participants displays some change over time. In HSB a higher percentage of participants are black (39%), whereas in NELS Hispanics and blacks are similarly represented (24% and 27%, respectively). Consistent with rising education levels nationwide, NELS students appear to have better-educated parents than HSB students. However, the parents of JROTC students appear to have less education than the parents of other students. Apart from changes due to overall demographic trends, the data suggest that the program expansion in the 1990s affected the size of the program but not the composition of participants.

JROTC members have significantly lower test scores and higher dropout rates than non-participants. While JROTC students have lower college enrollment rates than their peers, their military enlistment rates are much higher. This, however, may not be entirely due to the program, since in 10th grade JROTC participants report a preference for a military career at a rate several times that of non-participants.

V. Estimation and Selection Issues

Let an outcome y for student i in school j be determined in the following general form:

$$y_{ij} = m_0(X_i, S_j) + \delta(X_i, S_j) JROTC_i + u_{ij} \tag{1}$$

where $u_{ij} = a_i + s_j + e_{ij}$

X_i includes individual characteristics and S_j includes school characteristics. This model assumes that y_{ij} depends on a function of observable characteristics for non-participants, the program effect on students with characteristics (X_i, S_j), and an error term. The error is composed of an

[11] For definitions of 'at-risk' students see National Center for Education Statistics (2006) and Dynarski and Gleason (1998).

individual fixed effect a_i, a school fixed effect s_j, and a random component e_{ij}.

Estimating this model via OLS assumes that $m(\cdot)$ is linear in the parameters and is correctly specified, X_i and S_j are exogenous, and that program participation is uncorrelated with the composite error. Bias in OLS estimates could come from both observables and unobservables. If the treatment and control groups have different background characteristics, OLS would extrapolate too much because it uses non-participant outcomes to approximate participant outcomes under no treatment. In particular, OLS would obtain treatment effects via:

$$y_{ij} = \beta X_i + \gamma S_j + \delta JROTC_i + a_i + s_j + e_{ij} \qquad (2)$$

OLS is also prone to bias from unobservables, in our case the school and individual fixed effects. Although JROTC students appear more at-risk in their observable characteristics (suggesting a negative bias), they may be more motivated than the average at-risk student by virtue of their enrolling in a program that aims to improve career outcomes (suggesting an upward bias). Similarly, JROTC schools may be located in inner cities and enroll many at-risk students, but they may be more proactive than similar schools that do not apply for federally-funded programs. A priori, it is not clear which effect dominates. Our analysis aims to recover the program effect on the average JROTC student enrolled in an average JROTC school.

One approach to mitigate bias from unobservables is to include a rich set of control variables in OLS regressions. We assume that a_i is composed mostly of an ability component, a motivation component, and a component related to taste for the military. We include base year test scores to proxy for ability, a dummy variable for whether the student repeated a grade in elementary school as a proxy for motivation, and a dummy variable for whether the student indicated interest in a military career as a proxy for military taste.

In addition to the individual fixed effect, another source of bias is s_j, which includes unobservable school characteristics that may be correlated with both the school's decision to offer JROTC (and therefore student participation) and with academic outcomes. We believe that school heterogeneity is more problematic in this setting because student participation is overwhelmingly dependent on whether the school offers JROTC and because little is known about how schools obtain a unit.[12] To mitigate this source of bias in OLS regressions, we include several school controls that are correlated with the presence of JROTC in the school. These include student body demographics (percent minority enrollment), location (urban/suburban/rural), percentage of graduating students that join the military, and percentage of graduating students that enroll in college. One problem, however, is that these variables may be endogenous if they are contemporaneous with JROTC participation. The baseline OLS and probit models ignore this source of endogeneity.

Since school characteristics included in s_j vary by school but not by individual, we can remove the bias from the non-random selection of schools by obtaining school fixed effects estimates. This strategy nets out all observed and unobserved school attributes that may be correlated with both student outcomes and the presence of JROTC. For the continuous outcomes (cognitive test scores and self-esteem scores), the school fixed effects method estimates the program effect as follows:

$$y_{ij} = \beta X_i + \delta JROTC_i + \eta_{ij} \qquad (3)$$

where $\eta_{ij} = a_i + \left(e_{ij} - 1/J \sum_{j=1}^{J} e_{ij} \right)$, and J= number of schools

The fixed effects transformation cancels both observable (S_j) and unobservable (s_j) school characteristics. Equation (3) is estimated via OLS with pooled observations from all schools.

[12] Some of the criteria used by administrators to assign a JROTC unit include the demographic composition of the school and the area, recruiting potential, the geographic distribution of JROTC units, cost-effectiveness, and also the time that the school has spent on a waiting list for a unit (Coumbe and Harford 1996).

For the binary outcomes, there is no equivalent way to difference out the unobserved s_j. In addition, estimating s_j alongside β and δ leads to inconsistent estimates of the parameters (the incidental parameters problem). However, the logit model allows for estimation of program effects without any assumptions on the relationship between s_j and the right-hand-side variables (the extreme being the assumption of independence, as in simple OLS estimation). Absorbing the individual fixed effect a_i into the random error component $(\zeta_{ij} = a_i + e_{ij})$ and focusing only on the s_j component we assume that:

$$y_{it} = 1\left\{\beta X_i + \gamma S_j + \delta JROTC_i + s_j + \zeta_{ij} > 0\right\}, \quad i = 1,...,N, \; j = 1,...,J$$

If we further assume that ζ_{ij} follows a logistic distribution conditional on X_i, S_j and s_j, then

$$P\left(y_{ij} = 1 \mid X_i, S_j, JROTC_i, s_j\right) = \Lambda\left(\beta X_i + \gamma S_j + \delta JROTC_i + s_j\right) \qquad (4)$$

can be estimated via conditional maximum likelihood. This approach eliminates both school-specific effects S_j and s_j.

Both linear and non-linear fixed effects methods require that JROTC participation varies within schools. However, fixed effects logit further requires that the binary outcomes also vary within schools. For example, fixed effects logit does not draw any information from schools where all sampled students graduate, or where all sampled students do not enlist. This further addresses the bias due to s_j, since school unobservables could produce a never-enlist or always-graduate outcome. Both the linear and non-linear fixed effects estimates effectively reduce the sample to schools that host a JROTC unit, since very few students participate in JROTC in schools that do not offer the program. Because a school hosts only one JROTC unit (from one of the four services), and employs one or two instructors, school fixed effects estimates also do not depend on service-specific program variation or, for the most part, on instructor heterogeneity.

Compared to baseline estimates, school fixed effects estimates are not contaminated by bias from the possible correlation of $JROTC_i$ with s_j. However, the school fixed effects method still assumes that the model is correctly specified, and may yield biased estimates if there is little overlap in the support of X_i for participants and non-participants.

To relax some of these assumptions and also deal with the two levels of selection of both students and schools we also apply non-parametric techniques. We follow a two-stage process to select the relevant treatment and control groups. In the first stage we identify schools that are similar to JROTC schools based on observed characteristics, while in the second stage we further restrict the control group to students within "JROTC-like" schools who are similar to JROTC students in their observable characteristics. Letting y_1 denote a particular outcome for JROTC students, y_0 denote the same outcome for non-JROTC students, w be a dummy that equals one if the student participates in JROTC, and v a dummy indicating whether the school has a JROTC unit, the average treatment effect on the treated (ATT) is estimated as:

$$ATT = E\left[y_1 - y_0 \mid w = 1, v = 1\right] \qquad (5)$$

The key assumption is that, conditional on both school and student observables, the distribution of the outcomes for participants is no different from the distribution of outcomes for non-participants (the conditional independence assumption) (Rosenbaum and Rubin 1983). Since non-parametric estimation requires no functional form assumptions, and since the propensity of participating in the program can be estimated flexibly, we reduce the bias that could stem from assuming a linear and correctly specified function. More importantly, matching mitigates the bias stemming from the potential non-overlapping support of the observable characteristics X_i and S_j

for participants and non-participants by choosing and reweighing observations within the common support region.[13]

Heckman, Ichimura, and Todd (1997; 1998), Heckman et al. (1998), and Diaz and Handa (2006) find that matching performs well in the presence of a rich set of control variables. Our two-stage sample selection method is similar in spirit to the sample restrictions made by Diaz and Handa when estimating the effect of a poverty program. They use experimental data to evaluate their matching outcomes, and find that restricting the sample to eligible households substantially reduces bias compared to when all households are used in estimations. In a similar vein, by first matching schools, we restrict attention to schools that are likely to host JROTC, and compare students within these schools.

A common shortcoming of all these solutions is that they assume to varying degrees that selection of students into JROTC depends on observables. In our case, this amounts to assuming that among students with similar observed at-risk characteristics, cognitive ability, and military tastes, JROTC participation is random. For the two-stage matching technique we extend this assumption to schools by assuming that JROTC units are randomly distributed among schools with similar student body demographics, academic programs, and recruiting environments. Since selection of students into the program varies largely across schools and regions, it appears reasonable, given the richness of our data, to view participation as random after conditioning upon at-risk characteristics and military tastes.[14] Sensitivity checks indicated that this

[13] The two-stage matching differs from a simple matching of students based on their individual and school characteristics because it excludes students who may be at risk, but who attend schools unlikely to host the program. It further excludes schools that are 'too likely' to offer JROTC based on their characteristics. Matching students on both their individual and their schools' characteristics ensures that average school characteristics of sampled students are balanced. Given that we have school surveys, there is no need to rely on estimates of school characteristics based on averages from sampled students.

[14] We also applied instrumental variable (IV) methods to obtain program effects. We considered the following potential instruments for educational outcomes: the state-wide enlistment rate, whether one parent is in the military, whether the student participated in extracurricular activities in 8th grade (NELS only), whether the school offered

assumption is satisfied.[15] Section VI below discusses the baseline OLS and probit models, Section VII discusses fixed effects estimates, and Section VIII discusses the two-stage matching estimates.

VI. Baseline Models

Table 2 presents simple OLS and probit estimates of program effects, without addressing the aforementioned sources of bias. The results from the HSB sample are presented in column 1, while results from NELS are presented in column 2. All models include demographics (age, gender, and race/ethnicity), whether the student aspires to a military career, whether the student repeated a grade in elementary school, covariates for parents' income, education, and family structure (both natural parents, single mother, single father, other family structure), and school-level variables (region, suburban or rural location, whether the school program is vocational or academic, percent minority enrollment, percent of students enlisting, attending college, or dropping out). All post-high school outcomes include base-year test scores as a proxy for ability.

Although simple comparisons of means suggest that the academic performance of JROTC students is worse than that of their peers, Table 2 indicates that the performance of JROTC students is, for the most part, similar to that of non-participants. Test score differences persist when not controlling for base year test scores (results not shown), but disappear after adding base year test scores. Self-esteem scores appear slightly higher for JROTC participants in the HSB

JROTC (HSB only), number of extracurricular clubs available at the high school (a proxy for choice of extracurricular activities), the student's height-to-weight ratio (a proxy for physical fitness), and percent minority enrollments in the school. For the enlistment outcome, we omitted from the list IVs clearly correlated with a predisposition for the military. Overall, the IV estimates produced inflated coefficients and standard errors (five times the size of the baseline estimates).

[15] Since NELS provides test scores for 1988, 1990, and 1992, we estimated program effects on test scores via (individual) fixed effects. The results were similar to the ones presented in the paper. We also followed Altonji, Elder, and Taber (2005) and estimated bounds on our JROTC effects by assuming that the selection on unobservables is no greater than the selection on observables. We estimated bivariate probit models of binary outcomes and program participation, subject to the above condition. With HSB data, the bounds obtained were too wide and included zero. In the NELS data the correlation in the disturbances from the participation and outcome equations was insignificant. We interpret this result to mean that, conditional on our covariates, individual heterogeneity is uncorrelated with participation.

sample. The incidence of disciplinary problems also appears similar across treatment and comparison groups. JROTC students are much less likely to enroll in a two- or four-year college. To avoid the possibility that this finding is driven by their (likely contemporaneous) higher enlistment rates, we also examine the likelihood of obtaining a degree 8 to 10 years after high school. The estimates indicate that JROTC students are less likely to obtain a post-secondary degree.

These specifications include all relevant controls. In results not shown when we successively added the different categories of controls (demographics, family, and school-level variables), race and family background had the strongest effect on program estimates. The effect of the school variables is also important, especially the school's minority composition and dropout rate. For the most part, JROTC participants exhibit poorer performance in regressions that include smaller subsets of controls, which suggests that disadvantaged family backgrounds and low-quality schools account for JROTC participants' seemingly weak achievement.

Military enlistment presents an exception to this pattern. In Table 2, which includes all controls, JROTC participants are 75-150% more likely to enlist. However, when test scores are added as a proxy for ability the predicted enlistment effect increased by 9%, suggesting that lower-ability students may not meet minimum enlistment criteria, such as AFQT scores and minimum educational attainment. Similarly, when the indicator for disciplinary problems is added as a control, the enlistment effect again increases slightly, suggesting that behavioral problems also disqualify some who desire to enlist.

The enlistment effect also may be prone to taste endogeneity. Students may join JROTC because they intend to enlist and want to either take advantage of the advanced pay grade incentive, or to experience the military without incurring a service obligation. The program may simply identify those who are more likely to enlist, rather than induce participants to enlist. This argument is supported by the finding that the enlistment effect is 17% larger when the military aspirations variable is omitted. To further explore this hypothesis we also estimate the enlistment probability while controlling for whether a parent is serving in the Armed Forces, but we find that this has little effect on predicted enlistments. Finally, we include variables for state-level youth enlistment rates, which reduces the enlistment effect slightly (about 6% in NELS data). Overall, the enlistment effect appears to be robust to these concerns, although its magnitude is subject to the set of controls included in regressions.

Because geographic placement of JROTC programs targets disadvantaged schools and at-risk students, we also analyze whether JROTC effects differ among minority groups. The results of models with race-JROTC interactions are summarized in Table 3, panel A. One of the most salient differences is that black JROTC members have lower dropout rates than white participants (by 9-24 percentage points) and higher graduation rates (by 11-17 percentage points). Interestingly, the enlistment probability is similar across race groups.[16]

Does JROTC improve educational outcomes of minorities? NELS data indicate that among blacks, JROTC enrollees are less likely to drop out, more likely to graduate, and more likely to enlist. These positive program effects may be understated if black JROTC students are more at-risk than black non-participants, which is supported by a higher incidence of disciplinary problems for black JROTC students. When controlling for base-year disciplinary problems, however, their disciplinary issues in 12th grade are no different from other students.

[16] One possible explanation for these differences by race is that JROTC attracts blacks of higher average ability than white enrollees. However, when comparing test scores among JROTC members in different race/ethnic groups, white participants have higher test scores, even after controlling for family and school characteristics. In addition, all outcomes in Table 3 already control for base-year test scores to avoid this problem. If we further assume that JROTC attracts equally at-risk youth across race groups, then the comparison of JROTC students with each other most likely provides accurate causal program effects.

We also investigate program effects by gender. For most outcomes (not shown), female participants appear similar to male participants. However, HSB data indicate that female participants have higher self-esteem scores than both their male counterparts and female non-participants. Although compared to male participants JROTC females are less likely to enlist, they are more likely to enlist than non-participant females (panel B of Table 3).

VII. School Fixed Effects

As discussed in Section V, school fixed effects estimation reduces the sample to JROTC high schools. This method deals with the bias introduced from omitting relevant school controls (observable and unobservable) that are correlated with both academic outcomes and program participation. Table 4 presents the school fixed effects estimates. For continuous outcomes, the program effects are obtained via fixed effects OLS. For binary outcomes, average treatment effects are estimated using fixed effects logit, and Table 4 presents odds ratios from these estimations. With respect to test scores and disciplinary outcomes, fixed effects estimates appear similar to the baseline results. However, in the NELS sample, JROTC participants display a modest (1 point) test score gain after controlling for 8^{th} grade test scores.

When investigating post-secondary outcomes, the school fixed effects estimates amplify the negative academic outcomes of JROTC participants, consistent with the hypothesis that JROTC schools are more proactive in seeking programs to assist their students. Omitting adequate school-level controls thus may have introduced an upward bias in the baseline estimates. HSB data indicate that JROTC students are more likely to drop out. Further, in both HSB and NELS, JROTC students are less likely to pursue postsecondary education. The fixed effects estimates also suggest that the enlistment effect may be even larger than previously estimated: JROTC participants are 2-5 times more likely to enlist than non-participants within the same school. If within a school JROTC attracts students interested in the military, the comparison group may include people who would most likely never enlist. A priori, the alternative could also be true – the presence of a JROTC unit in the school may create spillover effects that stimulate school-wide enlistments. To investigate this issue, we compare the percentage of students expressing interest in a military career in the treatment and control groups. The gap in military aspirations between JROTC participants and non-participants is larger when calculated across all schools rather than within JROTC schools. This suggests that the larger enlistment effect obtained from fixed effects may be explained by program administrators selecting schools with higher enlistment propensities.

Compared to baseline estimates, school fixed effects deals with bias induced from school-level unobservables. However, if within a school JROTC students are relatively more at-risk, their academic outcomes will appear poorer. The school fixed effects method does not address this possible lack of overlap in the support of X_i. In addition, the school fixed effects estimates are obtained parametrically and may be prone to misspecification bias.

VIII. Two-stage matching estimates

JROTC effects derived from OLS and probit depend on the assumption that the outcomes of the control group approximate outcomes for JROTC students, had they not enrolled in the program. Summary statistics suggest that this assumption is tenuous: the difference in averages for the two groups exceeds a quarter of a standard deviation for most variables. Although school fixed effects estimation reduces the gap in observable characteristics between control and treatment groups, it does so indirectly by limiting attention to JROTC schools. Within JROTC schools, however, the support of observable individual characteristics X_i may differ between control and treated students. Therefore, our next set of estimations includes only students from the control group who are similar to JROTC students in their background attributes.

We restrict the sample in two stages: first, we select schools that are similar to JROTC schools in terms of student body characteristics; second, within these schools we identify students who are similar to JROTC participants based on their background characteristics. This two-step method excludes from the control group students who are similar to JROTC students but are enrolled in schools that are unlikely to offer JROTC. Since schools that offer JROTC have varying degrees of at-risk characteristics, we also exclude schools that are very unique in these respects (and, therefore, unlike non-JROTC schools). Since student academic outcomes also depend on school inputs, restricting the sample this way yields more comparable treatment and control groups.

To match schools we use the following variables: school program (vocational, academic, other), location (suburban, rural, urban), region, school size and its square, minority composition (percent black, percent Hispanic, percent other race), percent of graduating class attending college, dropping out, or enlisting, and percent of student body classified as disadvantaged or in a reduced price/free lunch program. We perform an exact one-to-one match of the schools based on these characteristics and keep in the sample only JROTC and non-JROTC schools with similar observables. We believe that these controls contain the most important factors that explain why schools apply for JROTC, and why program administrators accept them.

We then focus on the students who attend the matched schools. For this, we first perform a one-to-one exact match of students in the reduced school sample based on the following characteristics: individual and family characteristics, base-year test scores, whether the student ever repeated a grade in elementary school, taste for the military, and the above school characteristics.[17] Since this two-stage selection method and the exact matching in each stage reduces the sample size substantially, which may affect efficiency, we also match JROTC students to non-participants via stratification matching. This method weights the contribution of each student in the control group based on their similarity to the treated students. We remove students and schools that lie in either extreme of the quality distributions (i.e., we focus on individuals and schools that fall in the common support area). As a result, this method estimates average treatment effects for students in average JROTC schools.

Table 5 presents the two-stage matching estimates. The HSB data indicate that in-school performance of JROTC students is poorer than that of their peers. In particular, JROTC students are more likely to drop out, less likely to complete high school, and less likely to pursue or obtain postsecondary degrees. NELS data do not reveal any differences in dropout and graduation rates, but find lower rates of postsecondary enrollment and degree attainment. Although the baseline estimates revealed no significant differences in graduation and dropout rates, the two-stage matching estimates indicate poorer academic outcomes for JROTC participants, which reinforces the school fixed effects results.

Both samples indicate positive enlistment effects. In HSB, JROTC students are 113-150% more likely to enlist. In NELS, JROTC students are 214-271% more likely to enlist than comparable non-participants. The enlistment effect obtained via two-stage matching is more comparable to the simple probit estimate than to the fixed effects logit estimate. In fact, the fixed effects logit estimate is twice as large as the matching estimate. This suggests that the enlistment effect calculated within schools may overstate the effect calculated across schools, perhaps because some students in non-JROTC schools who enlist would have joined JROTC had the

[17] These variables are only used to categorize students in a way that facilitates good matches. The method does not presume to explain program participation.

program been offered by the school. However, within schools that offer the program, those who do not join JROTC will most likely never enlist.

To investigate the quality of the match, Table 1A in the Appendix summarizes background attributes of students and schools pre- and post-matching. With both datasets, the matching technique appears to balance the observed characteristics of the control and treatment groups, most notably military aspirations, race, gender, and parents' education. Before matching, only 2-3% of the control group has an interest in a military career, compared to the 15-18% of the JROTC students. After matching, both control and treatment groups show a similar military propensity. Matching schools on their background characteristics also reduces differences between JROTC and non-JROTC schools. Most notably, while in NELS school minority enrollments are only 29% in non-JROTC schools versus 50% after matching both types of schools have similar minority enrollments.

Compared to the baseline estimates, the two-stage matching reduces bias stemming from the different background characteristics X_i of the JROTC students and the characteristics S_j of the schools they attend. The advantage of the two-stage matching over school fixed effects is that it reduces the sample to *average* JROTC schools, thus avoiding the possibility of a few unique schools driving our results. In addition, the school-fixed effects method yields unbiased estimates only if all selection is based on school-level attributes, which may be a strong assumption. Overall, controlling for the various sources of bias reveals negative academic outcomes and stronger enlistment effects for JROTC. This suggests that the program may have a sorting effect that channels students with pre-existing military propensity toward military careers and away from academic pursuits.

IX. Conclusions

This study estimates the impact of high school JROTC on students' academic achievement, post-secondary education, and military enlistment. Baseline estimates suggest that JROTC students have higher enlistment rates and lower post-secondary enrollment rates than their peers. School fixed effects and two-stage matching techniques generally confirm these results, but also indicate that JROTC students from the HSB sample are less likely to complete high school. We find noteworthy differences in program effects for blacks and for females. Black JROTC students (about one-third of all participants) have lower dropout rates and higher graduation rates than both white JROTC participants and black non-participants. In addition, females in JROTC (about 40% of participants) display higher self-esteem scores than both female non-participants and male enrollees. The importance of this result is highlighted by recent research that links non-cognitive skills, including self-esteem, with labor market success (Heckman, Stixrud, and Urzua 2006).

While the composition of participants does not change drastically from one survey to the next, program effects do differ. The more recent NELS data reveal no negative program effects on dropout or graduation rates. This could be due to two main factors. First, NELS contains true pre-participation controls, since the survey starts in 8th grade. In contrast, HSB starts in sophomore year, so program effects are obtained from changes in performance between the 10th and 12th grades, rather than throughout high school. In addition, JROTC students in NELS may be more positively selected than in HSB. In the 1990s the military offered enhanced educational benefits to new recruits, which may have resulted in higher-ability recruits and, potentially, higher-ability JROTC participants. Finally, in the NELS period the military rejected more applicants lacking the minimum education requirements for recruitment compared to the HSB years.

The limited academic effects of JROTC are not unexpected since the program tends to be more vocational and extracurricular, rather than academic, in nature. Any academic gains from JROTC are likely to be indirect effects derived from gains in non-cognitive skills and behavioral changes. However, the absence of program effects on academic outcomes also could be due to inadequate controls for the at-risk status of JROTC students. Notably, different estimation strategies aimed at balancing the control and treatment groups on observable characteristics amplified both the negative program effects on academic outcomes and the positive effects on enlistments. One possible explanation is that enlisting in the military and pursuing postsecondary education are mutually exclusive decisions, and JROTC affects student sorting into these two career paths. Evidence from NELS supports this claim, since in this sample we observe less severe academic outcomes. Changes in recruiting policies and enhanced educational benefits offered by the military may have blended the line between these two career choices. Further support for the sorting hypothesis comes from research indicating that JROTC enlistees perform better in the military than other recruits, both in terms of job match (attrition) and performance (promotions) (Pema and Mehay 2006). Research on school-to-work programs has shown that some components (Tech Prep, for example) have a similar sorting effect, resulting in better employment outcomes, but poorer academic outcomes. Compared to STW programs, however, the marginal enlistment effect of JROTC far exceeds employment effects for STW programs (Neumark and Rothstein 2006).

The study provides a starting point for research on a high school initiative that, despite its size and recent growth, to date has escaped analytical scrutiny. It is important to note that this study estimates treatment effects for average JROTC students in average JROTC schools. The estimation techniques eliminate from the sample the tails of both the school and student distributions. The effects of the program may be very different for marginal students and schools. Further research will be needed to confirm the effects obtained here, to expand the set of potential outcomes, and to weigh potential economic gains against program costs. Future policy decisions on the JROTC program, including its size, funding, and choice of high schools for locating new units must await further evidence of program effectiveness and cost.

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Table 1 – Weighted Summary Statistics

	HSB				NELS			
	Non-participants		JROTC participants		Non-participants		JROTC participants	
Variable	Mean	(st. error)	Mean	(st. error)	Mean	(st. error)	Mean	(st. error)
Demographics								
Female	0.50	(0.006)	0.36	(0.042)	0.53	(0.011)	0.43	(0.059)
Black	0.10	(0.004)	0.39	(0.043)	0.12	(0.007)	0.27	(0.050)
Hispanic	0.12	(0.004)	0.12	(0.020)	0.10	(0.005)	0.24	(0.064)
Plan to enlist by age 30	0.03	(0.002)	0.19	(0.038)	0.02	(0.002)	0.20	(0.068)
School								
Urban	0.21	(0.005)	0.55	(0.045)	0.28	(0.009)	0.49	(0.063)
Public	0.90	(0.003)	0.99	(0.004)	0.90	(0.006)	0.98	(0.009)
South	0.31	(0.006)	0.56	(0.045)	0.34	(0.010)	0.65	(0.055)
% minority students	22.17	(0.359)	51.63	(2.759)	26.58	(0.632)	52.80	(4.070)
% college-goers	46.79	(0.263)	46.31	(1.950)	65.87	(0.373)	56.45	(2.704)
% students who enlist	3.67	(0.045)	4.10	(0.316)	4.41	(0.081)	5.35	(0.294)
Family								
Parents completed high school	0.05	(0.003)	0.05	(0.013)	0.22	(0.009)	0.19	(0.037)
Parents college graduates	0.13	(0.004)	0.05	(0.015)	0.27	(0.009)	0.13	(0.043)
Single mother family	0.14	(0.004)	0.19	(0.030)	0.15	(0.009)	0.29	(0.054)
Bottom income category	0.05	(0.002)	0.08	(0.019)	0.07	(0.005)	0.18	(0.041)
Top income category	0.08	(0.003)	0.03	(0.012)	0.22	(0.009)	0.10	(0.045)
Parent in military	0.02	(0.002)	0.11	(0.043)	0.02	(0.002)	0.11	(0.057)
Outcomes								
Test scores:								
8th grade	n/a		n/a		51.26	(0.203)	47.03	(1.232)
10th grade	26.50	(0.202)	19.90	(1.422)	51.01	(0.195)	46.74	(1.108)
12th grade	30.02	(0.235)	21.25	(1.494)	51.27	(0.200)	47.27	(1.426)
Disciplinary problems:								
10th grade	0.26	(0.006)	0.39	(0.044)	0.17	(0.007)	0.22	(0.041)
12th grade	0.20	(0.005)	0.26	(0.046)	0.17	(0.009)	0.30	(0.071)
Self-esteem score:								
8th grade	n/a		n/a		0.01	(0.013)	-0.02	(0.059)
10th grade	0.02	(0.009)	-0.05	(0.071)	0.02	(0.016)	0.08	(0.087)
12th grade	-0.01	(0.010)	-0.09	(0.061)	0.00	(0.016)	0.14	(0.082)
Dropout rate	0.15	(0.005)	0.25	(0.042)	0.13	(0.006)	0.18	(0.042)
Graduation rate	0.83	(0.006)	0.66	(0.045)	0.86	(0.007)	0.74	(0.054)
Attend postsecondary	0.45	(0.006)	0.22	(0.030)	0.66	(0.010)	0.54	(0.060)
Postsecondary degree	0.32	(0.006)	0.10	(0.021)	0.38	(0.009)	0.31	(0.064)
Enlist	0.08	(0.004)	0.24	(0.041)	0.06	(0.005)	0.22	(0.040)
Work	0.86	(0.005)	0. 83	(0.037)	0.87	(0.006)	0.84	(0.038)
Population represented		3,252,524		93,044		1,997,424		53,817

Notes: The samples include all students in HSB and NELS that matched with the High School Transcript Studies. Means and standard errors were estimated using panel transcript weights. School characteristics are obtained from student-level data and represent the characteristics of the schools attended by students in the sample. They are not representative of the population of U.S. schools. Test scores include math and reading scores. Disciplinary problems include expulsions, suspensions, or trouble with the law. Self esteem scores represent standardized scores on the Rosenberg scale. Dropouts include students who leave school, regardless of whether they later return. Graduation from high school is observed two years after senior year and includes only those who obtain a traditional high school diploma. Postsecondary enrollments and degrees refer to two- and four-year colleges. Employment status is observed in 1992 (HSB) and 2000 (NELS) and includes those who report currently working for pay. This variable takes a value of zero for those who are unemployed or out of the labor force.

Table 2 – Baseline OLS and Probit Estimates of JROTC Effects

Outcome	HSB	NELS
12th grade, controlling for base year		
Test scores	-0.84	0.60
	(0.72)	(0.39)
Self-esteem scores	0.11*	0.07
	(0.06)	(0.05)
Any disciplinary problems	0.07	0.11
	(0.11)	(0.11)
	[0.02]	[0.02]
Post- high school outcomes:		
Drop out of high school	0.17	-0.10
	(0.14)	(0.16)
	[0.03]	[0.004]
Graduate from high school	-0.19	-0.003
	(0.14)	(0.16)
	[-0.04]	[-0.001]
Attend postsecondary education	-0.45***	-0.33**
	(0.11)	(0.12)
	[-0.17]	[-0.12]
Obtain a post-secondary degree	-0.43***	-0.19
by 1992 (HSB) or 2000 (NELS)	(0.17)	(0.13)
	[-0.12]	[-0.07]
Enlist in the military	0.41***	0.88***
	(0.12)	(0.11)
	[0.06]	[0.17]
Employed in the civilian sector	-0.20	-0.12
	(0.14)	(0.11)
	[-0.04]	[-0.02]

Notes: See Notes to Table 1 for variable definitions. All regressions control for demographics, family, school characteristics, and military career aspirations. Post-secondary outcomes include base-year test scores. Regressions also include separate categories for missing controls. The sample size in regressions using HSB data varies from 5,989 to 6,908, of which 130-160 are JROTC participants. For regressions using NELS data, sample sizes vary from 6,578 to 8,314, of which 147-190 are JROTC participants. The variation in sample sizes is primarily due to missing values for the dependent variables. Standard errors appear in parentheses and are robust to within-school serial correlation. Binary response models are estimated via probit, and partial effects are in brackets.
***significant at the 1%; **significant at the 5%; *significant at the 10%.

Table 3 – Baseline OLS and Probit Estimates of the Effect of JROTC on Blacks and Females

Panel A.	Survey	*Drop out*	*Graduate*	*Attend postsecondary*	*Enlist*
Race					
JROTC × BLACK	*HSB*	-0.13*	0.06	-0.01	-0.04
		(0.07)	(0.08)	(0.08)	(0.07)
	NELS	-0.08**	0.10**	-0.01	-0.07
		(0.04)	(0.05)	(0.08)	(0.08)
Black participants vs. white participants	*HSB*	-0.24***	0.17**	0.11	-0.01
		(0.07)	(0.08)	(0.07)	(0.07)
	NELS	-0.09*	0.11**	0.01	-0.04
		(0.05)	(0.05)	(0.08)	(0.09)
Black participants vs. black non-participants	*HSB*	-0.04	-0.05	-0.17**	0.07
		(0.05)	(0.05)	(0.05)	(0.06)
	NELS	-0.06**	0.05	-0.01	0.19***
		(0.03)	(0.04)	(0.08)	(0.07)

Panel B.		*Self-esteem*	*Enlist*
Gender			
JROTC × FEMALE	*HSB*	0.20*	-0.11**
		(0.12)	(0.05)
	NELS	0.11	-0.11
		(0.09)	(0.07)
Female vs. male JROTC participants	*HSB*	0.20*	-0.24***
		(0.11)	(0.05)
	NELS	0.04	-0.19***
		(0.09)	(0.07)
Female participants vs. female non-participants	*HSB*	0.22**	0.01
		(0.10)	(0.03)
	NELS	0.15***	0.14***
		(0.06)	(0.04)

Notes: See Notes to Table 2 for model specifications. All interactions are estimated via linear probability models with standard errors robust to both heteroskedasticity and within-school serial correlation. This is done to avoid problems in estimating interaction effects via probit or logit (see Ai and Norton 2003). The differences in outcomes between participant and non-participant blacks, and between black vs. non-black participants are obtained by including separate categories for each minority-JROTC combination. The number of black JROTC participants in HSB models range between 69 and 75, and for NELS the number ranges between 43 and 53. The number of JROTC females in HSB models ranges between 57 and 63, and between 79 and 91 in NELS models.
***significant at the 1%; **significant at the 5%; *significant at the 10%.

Table 4 – School Fixed Effects Estimates

Outcome	HSB	NELS
12th grade, controlling for base year		
Test scores	-0.90	0.99**
	(0.72)	(0.49)
Self-esteem score	0.10*	0.04
	(0.06)	(0.06)
Any disciplinary problems	0.02	0.38
	(0.22)	(0.25)
	[1.02]	[1.15]
Post- high school outcomes:		
Drop out of high school	0.44*	-0.25
	(0.26)	(0.31)
	[1.55]	[0.78]
Graduate from high school	-0.26	-0.06
	(0.24)	(0.29)
	[0.71]	[1.07]
Attend postsecondary education	-0.65***	-0.72***
	(0.23)	(0.21)
	[0.52]	[0.49]
Obtain a post-secondary degree by 1992 (HSB) or 2000 (NELS)	-0.48	-0.38
	(0.30)	(0.24)
	[0.62]	[0.68]
Enlist in the military	1.08***	1.86***
	(0.27)	(0.28)
	[2.94]	[6.43]
Employed in the civilian sector	-0.47*	-0.28
	(0.27)	(0.26)
	[0.62]	[0.76]

Notes: See Notes to Tables 1-2 for variable definitions and model specifications. Continuous outcomes are estimated via OLS, whereas binary outcomes are estimated via fixed effects logit. The fixed effects logit models are identified from within-school variation in JROTC participation and the outcome of interest. Standard errors appear in parentheses and are robust to within-school correlation. Log-odds ratios appear in brackets. Sample sizes in column (1) vary from 5,638 to 7,730, of which between 136 and 180 are JROTC participants. In column (2) the sample sizes vary from 3,935 – 7,222, of which between 105 and 161 are JROTC students.
***significant at the 1%; **significant at the 5%; *significant at the 10%.

Table 5 – Two-Stage Matching Estimates

	High School and Beyond		National Educational Longitudinal Study	
Outcome	one-to-one nearest neighbor matching	one-to-many stratification matching	one-to-one nearest neighbor matching	one-to-many stratification matching
Test scores	-1.16	-2.89	-0.84	-0.40
	(3.46)	(2.64)	(1.58)	(1.01)
Any disciplinary problems	-0.07	0.001	-0.02	0.005
	(0.06)	(0.04)	(0.07)	(0.051)
Self-esteem score	0.22*	0.16	-0.16	0.09
	(0.15)	(0.13)	(0.12)	(0.09)
Drop out of high school	0.16*	0.10**	-0.02	-0.03
	(0.08)	(0.06)	(0.05)	(0.03)
Graduate from high school	-0.23**	-0.12**	0.0001	-0.01
	(0.08)	(0.05)	(0.06)	(0.03)
Attend postsecondary education	-0.09	-0.09**	-0.13**	-0.10**
	(0.08)	(0.05)	(0.07)	(0.05)
Obtain a post-secondary degree	-0.08	-0.14***	-0.07	-0.08*
by 1992 (HSB) or 2000 (NELS)	(0.09)	(0.04)	(0.07)	(0.05)
Enlist in the military	0.09*	0.12***	0.19**	0.15**
	(0.06)	(0.05)	(0.06)	(0.04)
Employed	0.02	-0.05	-0.02	-0.03
	(0.07)	(0.05)	(0.06)	(0.04)

Notes: The sample of students is selected based on demographics, family and school background, and base year test scores. The sample of schools is selected based on school size, urbanicity, region, minority composition of the student body, percent of students who enlist, drop out, go to college, percent of students who are classified as disadvantaged, and school program (vocational, academic, etc.). The number of matched JROTC students varies between 69 and 96 in both surveys. One-to-one matching restricts the sample of comparable non-JROTC students to the same range. When employing stratification matching, the number of control students varies between 828 and 1,132 for HSB and between 3,500 and 3,930 for NELS. Standard errors appear in parentheses and are obtained via bootstrapping and control for clustering at the school level.

***significant at the 1%; **significant at the 5%; *significant at the 10%.

Appendix Table 1: Characteristics of JROTC participants and schools in the matched samples

	High School and Beyond				National Educational Longitudinal Study			
	Original sample		Matched sample		Original sample		Matched sample	
	Non-JROTC	JROTC	Non-JROTC	JROTC	Non-JROTC	JROTC	Non-JROTC	JROTC
A. Characteristics of students								
Plan to enlist by age 30	0.03	0.18	0.15	0.17	0.02	0.15	0.16	0.16
Female	0.50	0.37	0.32	0.37	0.53	0.44	0.47	0.46
Black	0.12	0.39	0.32	0.36	0.09	0.25	0.25	0.25
Hispanic	0.22	0.22	0.20	0.23	0.13	0.26	0.15	0.18
Parents finished high school	0.05	0.07	0.04	0.08	0.22	0.21	0.24	0.22
Parents college graduates	0.11	0.06	0.06	0.07	0.25	0.15	0.17	0.13
Single mother family	0.16	0.24	0.22	0.24	0.14	0.26	0.28	0.28
Bottom income category	0.07	0.10	0.05	0.09	0.07	0.21	0.11	0.14
B. Characteristics of schools								
Urban	0.13	0.45	0.37	0.47	0.28	0.49	0.41	0.48
South	0.23	0.55	0.55	0.57	0.32	0.62	0.57	0.64
% minority students	0.20	0.44	0.35	0.45	0.29	0.50	0.47	0.49
% students in free/ reduced price lunch	0.16	0.25	0.23	0.25	0.22	0.31	0.25	0.28
% students who drop out	0.09	0.13	0.12	0.14	0.08	0.12	0.15	0.14
% students who go to college	0.44	0.43	0.45	0.44	0.62	0.59	0.60	0.58
% students who enlist	0.04	0.05	0.05	0.05	0.05	0.06	0.06	0.05

Notes: Panel A presents summary statistics for the entire HSB or NELS sample before matching, and after exact (one-to-one) matches of JROTC with non-JROTC students. The summary statistics for schools come from school-level data and are not directly comparable to the summary statistics presented in Table 1. The school characteristics in Table 1 come from student-level data, and may not be representative of the schools.