The Impact of the Gates Millennium Scholars Program on College and Post-College Related Choices of Low-Income Minority Students

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Abstract

In this paper we analyze the impact of the Gates Millennium Scholarship program on several outcome variables using regression discontinuity techniques. We find that GMS recipients have lower college loan debt and parental contributions towards college expenses and work fewer hours during college than non-recipients. We find some empirical evidence suggesting that after graduation GMS recipients are more likely to work in the Educational Services industry, have lower average wages, and are more likely to apply for graduate school than non-recipients. However, we do not find statistically significant differences in four-year graduation rates between GMS recipeints and non-recipients.

Keywords: Regression Discontinuity, Local Polynomial Smoothing, College Graduation, Loan Debt Accumulation

I. Introduction

The costs of college in the United States have risen sharply over time. From 1997 to 2004 the average yearly increase in tuition rates for all four-year institutions was 5.1% (U.S. Department of Education, National Center for Education Statistics), over double the inflation rate for the same time period.[1] This raises the question of the affordability of a college education for high school graduates, especially those from low-income households who are disportionately ethnic minorities.

These high costs of education may dissuade some high school graduates from attending college. Moreover, those who decide to attend college may have to take out student loans and work while in school in order to pay tuition. This, in turn, may delay graduation, reduce performance in college, and saddle students with high debt loads upon graduation. Again, these effects may be particularly salient among low-income students.

In a world of perfect capital markets where an individual can borrow or lend as much as they wish at a single competitively determined interest rate, all individuals with positive net present discounted values of a college education (based on this interest rate) would attend college. However, with imperfect capital markets individuals may be effectively constrained in the amount they can borrow. In order to attend college, they may have to finance a college education (at least partially) through alternative means such as receiving gifts (or loans) from parents, or by working. Under such circumstances individuals (especially those from low-income households) may choose to forego a college education even when it is expected to substantially increase future earnings. The receipt of a scholarship, such as a Gates Millennium Scholarship (henceforth GMS), may

[1] Inflation rates are based on the consumer price index for all urban consumers.

reduce financial constraints and in so doing induce some individuals to attend college who would not otherwise have done so.

In addition to improving access to college, imperfect capital markets and/or debt aversion may alter an individual's behavior during college and after they complete their college education (see Millet, 2003, and Rothstein and Rouse, 2007). For example, individuals who expect a high debt levels after graduation, all else equal, may alter their college major choice and early career plans.

While some researchers have found evidence consistent with credit constraints (e.g., Ellwood and Kane, 2000) others have found that these short-term borrowing constraints have little impact on educational attainment (Keane and Wolpin, 2001). Most of the attempts to measure the impact of these constraints are indirect.[2] Thus, an additional purpose of this paper is to determine how an award, that eliminates the need to borrow money to finance a college education, affects college enrollment, persistence, and graduation, providing a more direct assessment of the impact of short-term credit constraints.

Another important aspect of college financing is whether the method of financing alters future behavior. In particular, does the amount of debt that a student accumulates while in college influence how they behave either during or after leaving college. For example, do they alter their choice of career in order to more quickly pay back their loans or behave differently with respect to their decision of whether or not to attend graduate school? By essentially eliminating such debt, a scholarship like the GMS may change the type of careers that individuals choose.

[2] For a critique of the evidence on short-term credit constraints see Carniero and Heckman (2002).

For example, Rothstein and Rouse (2007) found that when grants were substituted for loans individuals were more likely to choose low paying "public interest" jobs such as working in the education industry. Based on an experiment with law students, Field (2008) found evidence that the way in which monetary equivalent financial aid packages are structured may have psychological impacts on career choice. In particuar, Field found evidence that students who were offered a scholarship which must be paid back if they don't work in a public interest job after graduating law school are much more likely to be placed in public interest jobs than students offered an equivalent loan package that the law school agrees to pay off if the student accepts a public interest job after graduation.

In this paper we examine the impact of receipt of a GMS on several outcome variables including college enrollment, student debt, working while in college, choice of college major and four-year college graduation rates, as well as graduate school attendance, occupation choice, and earnings upon college completion. Given non-random assignment into the program, it is difficult to make valid inferences about the effects of programs such as the GMS. One advantage of the GMS program design is that the awards are allocated among applicants on the basis of a test score where the "cutoff" score is not known in advance. Thus, we employ regression discontinuity (RD) methods (see Imbens and Lemiuex, 2008, for example) to estimate the impact of a GMS award on the aforementioned outcomes.

We find evidence that the GMS receipt improves a number of important student outcomes for low income, high ability, minority students that are served by the program. In particular, we find evidence that GMS receipt lowers student debt and their parents' financial contributions toward their college education, and reduces the number of hours students work while in college. Additionally we find no empirical evidence that GMS affects four-year college graduation rates or the likelihood of attending graduate school immediately following graduation. There is limited evidence that among those who graduate college and enter the labor market, GMS recipients have lower average starting salaries than non-recipients and are more likely to work in the Educational Services industry and in Professional Specialty occupations. Also, we find that among those who work immediately after leaving college, GMS receipt has a positive effect on the probability of applying for graduate school in their second year out of college. For some racial/ethnic groups we also find that GMS receipt affects whether an individual enrolls in a private versus public college and that it affects their choice of college major.

This paper is organized in the following way: In the next section we discuss in more detail the structure of the selection mechanism by which students are chosen for the GMS program. In Section III we discuss the data while Section IV discusses the RD estimation techniques used in this article. Section V presents the RD results and Section VI concludes the article.

II. The Gates Millennium Scholars Program

The Gates Millennium Scholars program is a $1 billion, 20-year project designed to promote academic excellence by providing higher education opportunities for low-income, high-achieving minority students. High school students who apply for the program have to meet a number of eligibility criteria before being accepted. Cognitive assessment measures are used to judge the academic potential of applicants (e.g., the academic rigor of their high school course work and their high school grades), but non-cognitive measures are also used in the selection process. Applicants must provide evidence that their high school grade point average is at least 3.33 (on a 4.00 scale). In keeping with the goal of the program to fund needy students, applicants also have to demonstrate financial need by documenting that they are eligible for the federal Pell grant program. Applicants need to be citizens or legal residents of the United States and have to complete all the required application materials to be eligible for the scholarship.

Regarding the non-cognitive component of selection into the program, students applying for admission are required to answer a series of questions developed mostly to measure an applicant's non-cognitive abilities.[3] The answers to each of these questions are scored by trained raters and a total non-cognitive test score (henceforth, "test score") is assigned to each applicant.[4] Thresholds on these test scores are established and they vary by racial/ethnic group and by matriculating cohort and are used to allocate the scholarships within race/ethnic group. Within each racial/ethnic group, qualified applicants are rank ordered from highest to lowest test score and scholarships are offered according to those rankings until the number of scholarships allocated for that group are exhausted. Applicants are unlikely to be aware of the thresholds because they are unaware of the number of applicants at the time they take the test. The raters are also unlikely to know the thresholds because they are unaware of the number of qualified applicants. However, even if raters are aware of the number of applicants at the time they score the

[3] The eight areas measured by these non-cognitive variables are positive self-concept, realistic self-appraisal, successfully handling the system, preference for long-term goals, availability of strong support person, leadership experience, community involvement, and knowledge acquired in a field. For additional information on the development and use of the non-cognitive measures see Sedlacek (1998, 2003, 2004).

tests, many applicants are later disqualified because they do not meet other program criteria including whether they are Pell eligible, have at least a 3.33 high school grade point average, or whether they fully completed the application process.[5] Of the 3,000 to 4,000 students who apply for the program in a given year, about 1,000 of them are eventually selected for the program.

Once in the program the students receive a scholarship that is a "last dollar" award meaning that it covers the unmet need remaining after the Pell and any other scholarships or grants are awarded. The GMS scholarship is portable to any institution of higher education of the student's choice in the United States and can be used to pay tuition and fees, books, and living expenses. The average award to freshman is about $8,000 and the average award to upper division students (juniors and seniors) is about $10,000-$11,000. The average award also differs by institution type, with students attending public institutions of higher education receiving about $8,000 and private school attendees receiving slightly more than $11,000 in financial support. As undergraduates, students are eligible for the GMS financial support for up to five years and they can apply for additional support if they decide to attend graduate school in engineering, mathematics, science, education, or library science.

III. Data

In this study we analyze data from Cohorts II and III of the GMS program. These are two of the cohorts that the National Opinion Research Center (NORC) tracks over

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[4] For a more detailed explanation of the non-cognitive test scores, see Appendix A.
[5] Individuals applyto GMS before a determination of Pell eligibility has been made using the Free Application for Federal Student Aid (FASFA).

time. At the time of this study NORC had collect three waves of information from both cohorts. The baseline survey was administered in the spring of the applicants' freshman year in college and the 1st follow-up survey was administered in the spring of the applicants' junior year of college. The 2nd follow-up survey was administered approximately two years after the 1st follow-up survey. So, those students who graduate college within four years after starting will have graduated by the time of the 2nd follow-up survey.

Table 1 presents the distribution of outcomes for applicants in Cohorts II and III (the fall 2001 matriculants are known as Cohort II and the fall 2002 matriculants are known as Cohort III). As noted in panel a) of the table, the vast majority of applicants who do not receive a scholarship are disqualified due to a test score that is lower than the "cut" score.

Of the approximately 4,000 Cohort II and 3,000 Cohort III applicants, NORC asked 2,340 and 2,333 (respectively) to participate in its longitudinal surveys (see Table 1, panel b). For both cohorts all 1,000 GMS recipients were asked to participate in the survey, whereas a random sample of non-recipients was also asked to participate. We have obtained applicant data for GMS scholars and the random samples of non-scholars for both cohorts. This data includes the applicants test score as well as scores on 11 sub-components, race, family income and family size.

As noted in panel b) of Table 1, the survey response rates were 69% for Cohort II and 81% for Cohort III, and higher for GMS recipients than for non-recipients in both cohorts (83% versus 58% in Cohort II and 90% versus 75% in Cohort III). Among the non-recipient responders in Cohort II only 25% were applicants who were disqualified because of low test score while 74% of non-scholars in Cohort III were disqualified because their test score was below the cut- point.[6]

The baseline survey asked individuals to provide information about their backgrounds, enrollment status, academic and community engagement, college finances and work, self-concept and attitudes, and future plans. The survey also asked the respondent the name of the college that they were attending. Using this information we merged additional data about school characteristics (e.g. public versus private) from the Integrated Postsecondary Education Data System (IPEDS) survey conducted by the National Center for Education Statistics (NCES). The follow-up surveys asked additional information for those who had obtained their undergraduate degree about any post-graduate study and/or labor market experience including job information for those currently working.

Cohorts II and III were combined and after removing a few (less than 20) inaccurate cases, the effective sample used in the analysis contains about 3,500 (see Table 2) respondents to the baseline survey, nearly evenly divided between GMS recipients and non-recipients. For these two cohorts, the initial award notification was made after most individuals with multiple college acceptances would have had to make a decision about which school to attend (i.e., the notification was made after May 1). Thus, the receipt of the Gates scholarship may only have a limited effect on (at least) the initial college choice.[7]

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[6] For Cohort II, NORC drew only 25% of the random sample of non-scholars from individuals below the cut-point while for Cohort III NORC drew 75% of its random sample from non-scholars below the cut point.
[7] In later cohorts notifications were made before May 1 for a substantial fraction of applicants.

There are observable differences in the overall sample including more (fewer) Latino/a (Asian American) students receiving (not receiving) scholarships than in the non-recipient group. Given the selection criteria, the parents of GMS recipients tend to have lower incomes and lower levels of education compared to their non-recipient counterparts. The SAT scores and percent of students who have less than four years of mathematics in high school are roughly equivalent between program participants and non-participants. For the sample used in this study, nearly all GMS recpients and non-recipeints are still enrolled in college at the time of the 1st follow-up survey (see Table 3). The enrollment rate for GMS recipeints at the time of the 1st follow-up survey, however, is 3 percentage points larger than for non-recipients (98 percent versus 95 percent).

The dollar amount of loans borrowed in the freshman year is about $2,140 for the full sample. Not surprisingly, GMS recipients borrow much less than non-recipients, the former borrowing about $975 in their freshman year compared to about $3,200 for non-participants. Using National Postsecondary Student Aid Study (NPSAS) 1999-2000 data, we calculated freshman loan levels for high ability (high school GPA of B+ or better), non-white Pell eligible students and the average was slightly lower (at about $2,800) than the overall average in Cohorts II and III of non-participants. Average cumulative loan levels though the junior year of college for the full sample are about $6,800, with GMS recipients borrowing about $3,300 and their non-recipient counterparts borrowing about $10,000. NPSAS data indicates cumulative borrowing for similar non-white students (high ability, low income) to be about $6,100 on average.

The NPSAS data also contains information on hours worked while students are enrolled in college. In 1999-2000, high-ability, low income students worked about 19 hours per week in their freshman and 19.5 hours per week in their junior year of college. The average number of hours worked in the Cohort II and III sample during the freshman year was substantially smaller (at 13.5 hours) than national averages during the freshman year. GMS participants worked about 11 hours during an average academic year work-week, whereas the non-recipient group reported working 15 hours (difference significant at p=.0000). During their junior year, students in the sample reported increasing their work effort to about 16 hours, with the difference between GMS recipients and non-recipients being about four hours (significant at p=.0000).

IV. The Estimation Strategy

In the early 1960s Thistlewaite and Campbell (1960) used the regression discontinuity design (RDD) technique to study the effects of the National Merit Scholarship program on career choice. Since then the method has also been used to examine the effects of compensatory education programs, especially Title I programs (Trochim, 1984) and in recent years RDD has been used to examine school district and housing prices (Black, 1999), the effect of class size on student achievement (Angrist and Lavy, 1999), the effect of school funding on pupil performance (Guryan, 2001), how student financial aid affects student enrollment behavior (van der Klauuw, 2002; Kane, 2003), how teacher training impacts student achievement (Jacob and Lefgren, 2004), the incentive effects of social assistance programs (Lemieux and Milligan, 2008) and the relationship between failing the high school exit exam and graduation from high school and/or subsequent postsecondary education outcomes (Martorell, 2004).

RD design is a non-experimental design (see Cook and Campbell, 1979) where subjects are assigned to the treatment (e.g., GMS recipients) and control groups (e.g., non-recipients) based on a score on some pre-specified criterion (or criteria). In a sharp design all individuals at or above a specific score (the "cut point") receive the treatment and those below the score are controls. In a fuzzy design, the probability of treatment increases discontinuously at the cut-point. For our data, no individual below the cut point received a scholarship while some individuals above the cut-point didn't receive a scholarship. This is sometimes referred to as a partially-fuzzy design (see Battistin and Retorre, 2008).

More formally, suppose that the mean value of an outcome variable for individual i, y_i, depends on whether or not a treatment is received which is represented by the indicator variable D_i. Thus,

(1) $$y_i = \beta_0 + D_i\alpha + \varepsilon_i$$

where α measures the impact of the treatment (D_i) on the $E(y_i)$ and ε_i is a zero mean random error.[8] In a "sharp" RDD there is a variable, x_i, such that $D_i = 1$ if $x_i \geq \tilde{x}$, where the value $\tilde{x}$ is the threshold or cut point, and D_i equals zero otherwise. Taking expectations of both sides of (1) with respect to x yields

(2) $$E(y_i \mid x_i) = \beta_0 + \alpha + E(\varepsilon_i \mid x_i)$$

when $x_i \geq \tilde{x}$ and

(3) $$E(y_i \mid x_i) = \beta_0 + E(\varepsilon \mid x_i) .$$

when $x_i < \tilde{x}$.

[8] For simplicity, in the discussion we assume homogeneity in the treatment effect.

One estimation strategy, which we use, is a parametric approach to RDD. For sharp design this approach assumes that $E(\varepsilon_i \mid x_i)$ is some parametric function of x_i (usually a polynomial of some known order, r, and estimates

(4) $$y_i = \beta_0 + \alpha I(x_i \geq \tilde{x}) + \beta_1 x_i + \beta_2 x_i^2 + \cdots + \beta_r x_i^r + \nu_i$$

where $E(\varepsilon_i \mid \nu_i) = 0$. Selection of Gates scholars is, however, a partially-fuzzy rather than a sharp design. This is the case because not all students with scores above the cut point receive scholarships due to Pell ineligibility, low high school GPAs, and in rare cases, incomplete information on the application. In this situation, $D_i \neq I(x_i \geq \tilde{x})$, so (4) no longer yields consistent estimates of the treatment effect.[9] However, using $I(x_i \geq \tilde{x})$ as an instrument for D_i and estimating

(5) $$y_i = \beta_0 + \alpha D_i + \beta_1 x_i + \beta_2 x_i^2 + \cdots + \beta_r x_i^r + \nu_i$$

by two-stage least squares estimation produces a consistent estimate of α. Because our running variable (test score) is discrete we compute standard errrors that are clustered by the test score as suggested by Lee and Card (2008).

One shortcoming with this parametric technique is that if the order of the polynomial is misspecified then estimates will be inconsistent. To estimate α without imposing parametric assumptions on $E(\varepsilon_i \mid x_i)$, we also employ a non-parametric approach. Here we assume only that $E(\varepsilon_i \mid x_i)$ is a smooth function of x_i so that $\lim_{x_i \downarrow \tilde{x}} E(\varepsilon_i \mid x_i) = \lim_{x \uparrow \tilde{x}} E(\varepsilon_i \mid x_i)$.[10] In a partially fuzzy design similar smoothness conditions are imposed on $E(\theta_i|x_i)$ where we assume

[9] Instead it is a measure of the intent to treat.
[10] For more details on non-parametric estimation of regression discontinuity models see Porter (2003).

(6) $$E(D_i = 1 | x_i) = \delta I(x_i \geq \tilde{x}) + E(\theta_i | x_i)$$

and θ_i represents the Pr(D_i =1) when an individual receives the treatment.

Under these assumptions it is demonstrable that

(7) $$\alpha = \frac{\lim_{x_i \downarrow \tilde{x}} E(y_i | x_i) - \lim_{x_i \uparrow \tilde{x}} E(y_i | x_i)}{\lim_{x_i \downarrow \tilde{x}} E(D_i | x_i)}$$

We estimate the three limit terms in (7) using local linear regression (see Fan and Gijbels, 1996). With non-paramteric methods, the choice of the bandwidth becomes an issue. The results reported below are based on choosing three separate data driven or "plug-in" optimal bandwidths for each of the limits in (7) where the bandwidths are chosen by minimizing the mean squared error using data only from the same side of the cut point (see the Appendix B for details).[11]

One requirement that must be satisfied in order for RDD to yield valid causal inferences is that subjects must be randomly distributed around the cut point (Lee, 2008). To explore this, we tested for statisically significant differences in the average values of observed characterieristics between those at the cutoff score and those one-point below the cutoff score and found no evidence of statistically significant differences (see Table 2). Recall that the test score is composed of 11 subscores. While we would expect some differences in the average subscore measures between those just below the cut-point and those at the cut-point (since the sum of subscores for the latter is by definition 1 greater than the former), we shouldn't expect the subscores measured as a fraction of the total test score to be substantially different between the two groups if they are randomly

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[11] Results were similar when a single bandwidth was used for the limits in the numerator of (7).

distributed. When we tested this hypothesis (see Table A1 of appendix) we could not reject the null hypothesis of no differences at conventional levels of statistical significance (p-value = 0. 14).

Of course, of greatest concern are the non-random differences around the cut-point that are related to the dependent variables of interest. To examine this we also estimated regression models for several of our dependent variables which included several predictor variables other than our total non-cognitive score running variable.[12] Plotting the average predicted values of the dependent variable by total non-cognitive test score, we would expect to see substantial jumps in the average predicted values of these dependent variables at the cut point if these non-random differences are important (see Card, Chetty, and Weber, 2007). Figures 5 and 6 present plots of the average predicted value by total non-cognitive score, as well as polynomial regression estimates of the predicted value on the total non-cognitive score. As these figures indicate, there appear to be no substantial jumps at the cut points. These findings further bolster our confidence that, at least for observable variables, individuals are approximately randomly distributed around the cut point.

While the manner in which the cut-point was determined suggests that manipulation of the test score should not pose a serious threat, we explored this possibility more formally. While partial manipulation poses no problem, as long as the test score contains a random error component (Lee, 2008), there may be a possiblity of full manipulation of the test score variable. To assess this, Figures 1 and 2 provide weighted estimates of the test score densities by race/ethnicity group for Cohorts II and III for all those in the random sample (regardless of whether they responded to the any of the NORC survey waves) and test scores being measured relative to the cut-point. If there was full manipulation of the test score then one would expect to see a “jump” in the density around the cut point (McCrary, 2008). Inspection of the smoothed densities in Figures 1 and 2 fail to reveal any unusual behavior in the density estimates near the cut points.

Because test scores are integer valued, we further tested for substantial relative jumps in the estimated probability distribution at the cut point by analyzing changes in the probability distribution between consecutive test scores.[13] We also standardized these distribution differences by dividing them by the standard deviation of the estimated difference. Full manipulation would present itself as a large positive estimated difference at the cut point relative to the estimated differences at other test scores. The smoothed empirical distributions of the standardized differences are presented in Figures 3 and 4 for Cohorts II and III, respectively. The vertical lines indicate the standardized difference at the cut point, and none of the standardized differences at the cut-point lie in the right-hand tail of the empirical distribution implying that full manipulation of the test score is unlikely.

V. The Results

[12] Models are estimated separately by ethnic group and control for type of high school (public, private, religious), composite SAT score, number of years of science, number of years of math, family size, whether the family owns a home, parents’ education, gender and immigrant status.

[13] More formally if we let p_t be the probability distribution for test score t, the difference is defined as $d_t = p_t - p_{t-1}$. The estimates of p_t are derived using sample weights.

Regression discontinuity estimates for the full sample are presented in Table 4.[14] Results from the baseline survey are presented in columns (1) and (2), estimates from the 1st follow-up survey are presented in columns (3) and (4), and estimates from the 2nd follow-up survey are presented in columns (5) and (6). The estimates are based on a parametric model where the sample is limited to those whose test scores are within ten points of the cut-score. To allow for cut-score differences between the racial/ethnic groups, the estimates reported in columns (1), (3) and (5) include race and cohort controls, two-way interactions of race and cohort, polyomial functions of the test score, as well as the interactions of this polynomial with race, cohort, and race by cohort. The estimates in columns (2), (4) and (6) include additional controls for gender, mother's and father's education, family size, parental income, type of high school attended (private, public or religious), SAT score, number of years of math taken in high school, and number of years of science taken in high school. We also estimated models with linear, quadratic, and cubic polynomial specifications. The results reported in the tables are from the quadratic specification for test score. Estimates from a cubic specification tended to yield similar results while the linear specification was mostly rejected by the data.

The first row presents estimates of the effect of GMS on the total amount of scholarships received by an individual. While the GMS program is a last dollar award, there is some possibility that other scholarships may be reduced in anticipation of receiving a Gates scholarship. These estimates measure the net impact of GMS on the total amount of scholarship money received. The estimates are positive and statistically

[14] For all dichotomous dependent variables we report the linear probability estimates with robust standard errors. Probit estimates yielded similar results.

significant for all waves of the survey, although the estimates are appreciably larger for the first and second follow-up surveys than for the baseline survey.

The estimated impact of GMS on college enrollment is small and not statistically significant in the baseline, 1st follow-up survey, and 2nd follow-up surveys. One reason for the small effect may be that the applicant pool consists of higher ability minority students (recall that applicants need a high school g.p.a. of 3.33 or higher to qualify and few were disqualified because they didn't satisfy this requirement).

Whereas the estimated impact of GMS on the probability of enrollment in private (as oppossed to public) college is positive for all waves except the 1st follow-up when only the base set of controls are included, it is always imprecisely estimated and never statistically significant.

The impact of GMS on the cumulative loan amounts of students is negative and statistically significant for all three survey waves (although the significance is weak for the baseline survey estimates when additional controls are included). Measured as a percent of the estimated increase in scholarship money, yearly loans are reduced by 69%, 61%, and 44% of the estimated increase in scholarship money in the baseline, 1st follow-up and 2nd followup surveys, respectively.[15]

Not only did GMS reduce the amount of loans that students took out but there is also some empirical evidence that parental support was reduced, at least for the junior year of college (1st follow-up survey) equalling 27% of the estimated increase in scholarship money for that year. GMS also leads to a statistically significant reduction in

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[15] The cumulative scholarship amount is computed by taking the average value of the point estimates for the model with only the base set of controls up to the current survey wave and then multplying this average by

hours worked per week and average weekly earnings for the baseline and 1st follow-up surveys. The estimated effect of GMS on hours worked and earnings in the 2nd follow-up, while negative, are not statistically significant. Assuming a 30 week school year, the estimates translate into a yearly reduction in earnings equal to 57% and 35% of the estimated increase in the scholarship amount in the baseline and 1st follow-up surveys, respectively. The evidence in Table 4 suggests that, for the most part, the increased scholarship money from GMS leads to reductions in student debt and reductions in weekly hours of work.

Table 5 investigates whether the GMS changed the students' college major, the likelihood of having received an undergraduate degree by the time of the 2nd follow-up survey, the likelihood of attending graduate school by the 2nd follow-up survey, the average earnings, industry and occupation of job of those who completed their BA but chose not to go to graduate school immediately after completing their undergraduate degree, and the likelihood of applying to graduate school among those who completed their undergraduate degree but were not currently attending graduate school at the time of the 2nd follow-up survey.[16]

To explore whether receipt of GMS affected choice of major, we estimated separate RD models for whether GMS changed the probability of being a social science, STEM (Science, Technology, Engineering, or Mathematics), humanities, education, or professional school (e.g., business or journalism) major (measured at the time of the 1st follow-up survey). For these college majors, the estimated effect of GMS on type of

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the number of years in school. This cumulative scholarship amount is then compared to the cumulative amount of loans for that survey wave.

college major was not statistically significant in models with or without additional controls.[17]

Whereas receiving a GMS may alter students' incentives to finish their undergraduate degree in four years, the estimated impact of GMS on probability of having completed college at the time of the 2nd follow-up, while negative, was not statistically significant. Among those who had completed their undergraduate education by the second follow-up survey, the estimated effect of the GMS on the probability of attending graduate school at the time of the survey was also negative but not statitsically significant.[18] However, among those who had completed an undergraduate degree but were not currently enrolled in graduate school at the time of the 2nd follow-up, the estimated impact of the GMS on the probability of applying to graduate school was positive and statistically significant both in models with and without additional controls. The point estimates imply that the GMS increases this probability by about 30 percentage points or by over 150%.[19]

Among those who finished college but were working instead of attending graduate school, the estimated impact of the GMS on yearly earnings was negative and weakly significant (p-value = 0.073) at least for the estimates with just the baseline set of

[16] Unfortunately the survey did not ask those currently enrolled as undergraduates whether they applied to graduate school for the following year.

[17] GMS continues to provide scholarship money to recipients who attend graduate school if they are in either the STEM or education fields. It appears, however, that such an added incentive did not lead to significantly higher rates of STEM or education majors among GMS recipients.

[18] Since we are conditioning on completing an undergraduate degree the estimated effect does not have a causal interpretation. The estimated effect of receiving a Gates scholarship on the probability of attending graduate school among all respondents, however, while negative is also not statistically significant.

[19] When we analyzed the combined event of either being in graduate school or having applied to graduate school, the estimates effect of receiving a Gates scholarship was positive and statistically significant for models with and without additional control variables.

control variables. The point estimate indicates that GMS reduced annual earnings by about $7,000.[20]

The estimated effect of GMS on the probability of working in the Educational Services industry was positive and statistically significant. This finding is similar to results by Rothstein and Rouse (2007) for reductions in student debt. There was also some weak statisical evidence (p-value = 0.060 for estimates with the base set of controls) that, among those who completed their undergraduate degree but were not enrolled in graduate school, GMS increased the probability that the individual was working in a professional specialty occupation.[21]

To check the robustenss of the findings we performed several additional sets of estimations. First, because many students in the sample (both GMS scholars and non-recipients) attend the same institution, we re-estimated the models (where applicable) controlling for school (at the time of the baseline survey) fixed effects. The results are presented in Table 6 and are qualitatively similar to those presented in Tables 4 and 5.

Second, we added 10 of the 11 subscores and included them as additional control variables. Third, we re-estimated the models reported in Tables 4 and 5 when the sample was restricted to those whose test score were within 6 points of the cut-off point. Fourth, we further restricted the sample to those within 4 points of the cut-off and estimated a model with a linear term for test score. Fifth, we confined the sample to those within 2 points of the cut-off and estimated a model with no further controls for test score. Sixth, we estimated models that excluded those with test scores within 10 points of the cut-point

[20] While the estimate with additional control variables was not statistically significant at the 10% level, with 95% confidence we rule out GMS effects of greater than $1,825 or 6%.

and also excluded those who were ineligible for GMS because they never completed their application, were not Pell eligible, had no record of financial aid or did not have at least a 3.33 high school GPA. The only individuals with test scores about the cut-point who didn't receive GMS in this sample were six individuals who explicitly turned GMS down.

For all the alternative specifications noted above the estimated effects are qualitatively similar.[22] For many of these specifications, however, the estimated effect of GMS on the probability of working in a professional specialty occupation was statistically significant at the 5% significance level.[23] One of the more narrowly defined occupations contained in the professional occupation category is school teacher. When we estimated the effect of GMS on the probability of being employed as a teacher, it was positive and statistically significant in several of the alternative specifications.[24] At the time of the 2nd follow-up survey, among employed college graduates who were education majors 60% of the GMS recipients and 62% of the non-recipients were working as teachers. However, among employed college graduates who had not majored in education, 10% of GMS recipients and only 5.7% of non-recipients were teachers, a statistically significant difference. This evidence suggests that GMS may induce some non-education majors to become teachers.

As a further check of the robusteness of our results we estimated non-parametric models using local linear regression as described in Section IV. These estimates used the

[21] In the model estimates with additional controls the estimate was not statistically significant at conventional levels but with 95% confidence we rule out decreases of more than 0.035 or 10 percent.
[22] The results are available from the authors upon request.
[23] For example, in estimates that restrict the sample to those with test scores within 2 points of the cut point, the point estimate for GMS when additional regressors are included is 0.217 with a standard error of 0.091.
[24] For example in the specification where the sample is restricted to those with test scores within 2 points of the cut point the estimated coefficient for the Gates scholarship variable is 0.097 with a standard error of 0.040.

relative score as the running variable and combined all three racial/ethnic groups and we restricted the sample to students with test scores within six points of the cut-point. The results of the non-parametric estimations using the optimal bandwidth are presented in Tables 7 and 8. Although more imprecise, the local linear regression estimates are generally consistent with the findings from the parametric specifications which further bolster our confidence in our results.[25]

To determine whether the estimated effects differed by student characteristics, we also estimated separate models by racial/ethnic group, gender, whether or not at least one parent had some college education, and whether the student was above or below the median SAT score.[26] The results for the dependent variables of Table 4 are presented in Table C1 of the appendix and the results for the dependent variables of Table 5 are presented in Table C2 of Appendix C. Statistically significant differences by race were found for the effect of GMS on the probability of attending a private college for all waves, although the significance was weak for the 1st follow-up survey. The main difference was that the effect of GMS on the probability of attending a private college was positive and statistically significant for African Americans and larger than the estimated effects for other racial groups for the baseline and 1st follow-up surveys, with the point estimates implying that GMS increased the probability of an African Americn attending a private college by 71% for the baseline survey and 45% for the 1st follow-up survey.

[25] The optimals bandwidths varied from 0.5 to 5.0 for the limits in the numerator of (7) and from between 0.25 and .75 for the denominator limit in (7). So, to check the robustness of the non-parametric estimates, we comptued estimates with fixed bandwidths from between 0.5 and 5.0 with increments of .5 for the numerator terms and from between 0.25 and 1.25 for the denominator term with increments of 0.25. The

For the 1st follow-up survey, statistically significant racial differences were also found for the effect of GMS on parental contributions and total amount of loans, with GMS receipt leading to substantially larger drops in both parental contributions and total loan amounts for Asian Americans relative to their African Americans and Latinos. Finally, there was some evidence of racial/ethnic differences in the effect of GMS on the probability of being a humanities major (p-value = 0.070) with GMS increasing the likelihood of being a humanities major for Asian Americans (p-value = 0.068).

When analyzed separately by gender, there was some weak statistical evidence (p-value =0.058) that GMS reduced hours of work more for males than females in the baseline survey with the average number of hours worked lower by 58% for males and 21% for females. We also found statistically significant differences between males and females in the effect of GMS on total scholarship money for the 1st follow-up survey with males receiving approximately $7,000 more in scholarship money than females. The lower total amount of loans caused by the GMS for the 1st follow-up survey was also larger for males than for females, but the difference was significant only at the p-value=0.057 level. GMS had a statistically signifcant differential effect on the probability of being a STEM major between males and females, with the estimated coefficient associated with GMS being positive and statisically significant for males and negative and statistically significant for females.

When models were estimated separately by whether or not the individual had a parent who attended college, there were statistically significant differences in the

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results, which are available upon request, were similar to those produced by both the parametric models and those produced using the optimal bandwidths.

[26] For these latter two categories those with missing values were excluded from the estimations.

estimated impact of the GMS on weekly hours worked with those having a parent that attended college showing a larger reduction than those who had no parent that attended college. There were also statistically significant differences by parental education in the effect of GMS on the probability of having applied for graduate school at the time of the 2nd follow-up. The estimated effect of GMS on the probability of applying for graduate school, among those who had completed their undergraduate degree but were not currently attending graduate school, was positive and not statistically significant for individuals whose parents had not attended college. The estimated coefficient for those with a parent that attended college was over four times as large as the estimate for those without a parent that attended college.

As noted in Table 1 there were differences in the reposnse rates between GMS recipients and non-recipients. However, what is important for our analyses is whether or not there are differential response rates around the cut point. To investigate this we estimated paramteric RD models similar to those reported above where the sample consisted of all individuals asked to participate in the survey. The dependent variable is an indicator variable for whether an individual was a non-respondent. The results of the parametric RD estimations are presented in column (1) of Table 9. The first row presents estimates for the non-response rate for the baseline survey while the second and third rows present the estimates for the non-response rate of the 1st and 2nd follow-up surveys, respectively. The point estimates associated with the GMS variable, while negative, are not statistically significant for any of the survey waves. Column (2) presents analogous estimates for the non-parametric RD model. The point estimates are also negative but not statistically significant.

To check the robusteness of the non-response findings we estimated several additional specifications, some of which found that GMS receipt leads to a statistically significant decrease in non-response rates. We therefore calculated sharp upper and lower bounds of the treatment effect using the techniques reported in Lee (2009). For simplicity, we computed the bounds using only the sample of individuals with test scores within two points of the cut point who were not disqualified due to Pell ineligibility, missing information, or low grades. For this sample of 296 individuals, only 1 individual with a test score above the cut-point declined the GMS. We, therefore, discarded this individual and estimated the effect of GMS using a sharp RD model which simply compared mean differences for the two groups. The estimated impact as well as the estimated lower and upper bounds are presented in Table 10. While in many cases the point estimates of the lower and upper bounds for the effect of GMS are either both positive or both negative, in general, the estimates are imprecise and the 95% confidence intervals in most instances contain 0. The exceptions are for the estimated bounds for the impact of GMS on total scholarship amount and the total amount of student loans for the 1st follow-up survey and the estimated effect of GMS on the probability of applying for graduate school among those who have completed an undergraduate degree but are not currently attending graduate school at the time of the 2nd follow-up survey.

V. Discussion and Conclusions

This paper has analyzed the impact of the receipt of a Gates Millennium Scholarship on several outcome variables. We found evidence that GMS reduces hours worked while in college, the amount of debt that a student incurs while in college, and the amount of parental contributions towards the students' college education. The impact of GMS on college enrollment and four-year college graduation chances was generally not statistically significant. One factor that may have influenced the enrollment and graduation findings was that GMS applicants were already self-selected to be high achieving students. Regarding the impact of GMS on four-year graduation chances, it appears that some students continue to receive the GMS in their fifth year of college. Realization that this funding is available may change student incentives about timely (four-year) completion, lengthening their time to degree.[27]

Among those who completed college in four-years, the GMS also increased the probability that an individual was either currently enrolled or was in the pocess of applying for graduate school. Among college graduates who were not attending graduate school at the time of the 2nd follow-up survey, there was also some evidence that the GMS increased the likelihood that an individual was working in a professional specialty occupation and also increased the probability that an individual worked in the Educational Services industry

We also found evidence that for African Americans, receipt of a GMS increased the likelihood of attending a private college and reduced the likelihood of being a STEM major. We also found that GMS receipt had a larger impact on the probability of applying for graduate school if the individual had a parent with some college education.

One limination of this study is that the currently available data covers a period spanning only about four and one-half years. Thus, the longer term impacts of the GMS on college graduation, graduate school attendance, and occupation choice cannot be fully

[27] In some instances students in their fifth year of college appear to still be receiving GMS funding.

assessed. Future research will be able to remedy this shortcoming as additional waves of survey data become available.

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Appendix A

Non-Cognitive Essay Questions

In Cohorts II and III of the Gates Millennium Scholarship Program applicants (nominees) were asked a series questions in order to assess the following eight non-cognitive variables (See Sedlacek, 2004).

1. **Positive self-concept**: Demonstrates confidence, strength of character, determination, and independence.
2. **Realistic self-appraisal:** Recognizes and accepts any strengths and deficiencies, especially academic, and works hard at self-development; recognizes need to broaden his or her individuality.
3. **Successfully handling the system (racism):** Exhibits a realistic view of the system on the basis of personal experience of racism; committed to improving the existing system; takes an assertive approach to dealing with existing wrongs, but is not hostile to society.
4. **Preference for long-term goals:** Able to respond to deferred gratification; plans ahead and sets goals.
5. **Availability of strong support person:** Seeks and takes advantage of a strong support network or has someone to turn to in a crisis for encouragement.
6. **Leadership experience:** Demonstrates strong leadership in any are of his or her background.
7. **Community involvement:** Participates and is involved with his or her community.
8. **Knowledge acquired in a particular field:** Acquires knowledge in a sustained or culturally related way in any field.

In addition to scoring applicants on these eight dimensions based on their answers to a series of short essay questions, applicants were also assessed on the rigor of their course work, number of math, science and langauge courses, and the scholarly quality of their essay(s). Scores were computed by trained raters. Each dimension was given a score between 1 and 8. The total score across all 11 subscales was then used to allocate scholarships. This is referred to as the Total Non-Cognitive Test Score (or simply test score) in the text.

Table A1 reports average subscores for all 11 subscales. It also shows average subscores measured as a fraction of the total test score for those whose total score was equal to or greater than the cut-off score and for those whose total score was less than the cut-off score. In addition, Table A1 shows differences in subscale scores measure as a fraction of the total subscore between those whose total score was equal to or 1 point above the cut-off score and those whose total score was 1 or 2 points below the cut-off score. Once we look only at individuals close to the cut-off score, a joint significance test for differences in the average subscores (measured as a fraction of the total score) between those above or below the cut-off score is not rejected at conventional significance levels.

Table A1

Average Test Sub-scores

		Sub-score	Sub-scores as a fraction of total score			Sub-score	Sub-scores as a fraction of total score		
	Sub-score	**All**	**Total score at or above Cut Point**	**Total score below Cut Point**	**t-test of difference**	**Total Score at or 1 below Cut Point**	**Total score = Cut Point**	**Total score = Cut Point -1**	**t-test of difference**
1	Positive Self-concept	6.84	0.096	0.096	-0.10	6.79	0.095	0.094	0.22
2	Realistic Self Appraisal	6.67	0.093	0.092	3.91	6.62	0.092	0.092	-0.17
3	Understand and Navigate Social System	6.26	0.090	0.084	16.03	6.24	0.088	0.088	-0.84
4	Prefer Long Range Goals over Short Term Needs	6.68	0.094	0.092	5.33	6.67	0.094	0.093	1.37
5	Strong Support Person	5.62	0.075	0.083	-26.61	5.68	0.077	0.079	-3.13
6	Leadership	6.53	0.093	0.089	9.99	6.52	0.092	0.092	0.53
7	Community Service/Involvement	6.30	0.089	0.087	7.18	6.29	0.088	0.088	0.51
8	Ability to Aqcuire Knowledge in Non-Traditional Ways	6.43	0.090	0.088	6.19	6.42	0.088	0.089	-1.09
9	Rigor of Course Work	7.08	0.097	0.103	-13.48	7.06	0.101	0.099	1.45
10	Math/Science/Language Courses	6.92	0.097	0.101	-9.51	6.93	0.099	0.098	0.94
11	Scholarly Essay Score	6.21	0.087	0.085	4.32	6.21	0.086	0.087	-0.72
13	Overall F-test for Mean Differences			p-value=	0.000			p-value=	0.136
14	Total Non-Cognitive Component: Sub-scores (1)-(8)	51.34	0.719	0.711	11.54	51.23	0.713	0.716	-1.070
15	Total Cognitive Components: Sub-scores (9)-(11)	20.22	0.281	0.289	-	20.21	0.287	0.284	-

Source: Cohorts II and Cohort III Gates Millenium Scholarship Program
Notes: All subscores on 8 point scale.

Appendix B
Local Polynomial Regression Estimates and Optimal Bandwidth Determination

The RD estimate is given by $\alpha = \dfrac{\lim_{x_i \downarrow \tilde{x}} E(y_i \mid x_i \geq \tilde{x}) - \lim_{x_i \uparrow \tilde{x}} E(y_i \mid x_i < \tilde{x})}{\lim_{x_i \downarrow \tilde{x}} E(D_i \mid x_i \geq \tilde{x})}$.

To derive a consistent estimator of α we need to consistently estimate $E(y_i \mid x_i \geq \tilde{x})$, $E(y_i \mid x_i < \tilde{x})$ and $E(D_i \mid x_i \geq \tilde{x})$ in a neighborhood of $\tilde{x}$. To obtain consistent estimates of these three terms we apply local polynomial regression.

Consider the regression model

$$y_i = m(x_i) + \varepsilon_i$$

Local polynomial regression estimates of m(x) at a point x_0 by estimating a weighted polynomial regression where points near x_0 receive larger weights. Suppose that a local polynomial regression of order p is estimated. Let **X** be the matrix defined by

$$\mathbf{X} = \begin{pmatrix} 1 & (X_1 - x_0) & \cdots & (X_1 - x_0)^p \\ \vdots & \vdots & & \vdots \\ 1 & (X_n - x_0) & \cdots & (X_n - x_0)^p \end{pmatrix}$$

and let **y** be the vector

$$\mathbf{y} = \begin{pmatrix} Y_1 \\ Y_2 \\ \vdots \\ Y_N \end{pmatrix}$$

Finally, define a diagonal weighting matrix **W** by

$$\mathbf{W} = \mathrm{diag}\{K_h(X_i - x_0)\}$$

where K_h is a kernel weighting function with bandwidth h and is defined by

$$K_h(\cdot) = K(\cdot / h)/h$$

for some kernel function. Throughout we use the Epanechnikov kernel function defined by $K(u) = \tfrac{3}{4}(1-u^2)$ for -1 < u < 1. The estimated local polynomial coefficients at x_0,

$$\hat{\boldsymbol{\beta}} = \begin{pmatrix} \beta_0 \\ \beta_1 \\ \vdots \\ \beta_p \end{pmatrix}$$

are then obtained from

$$\min_{\boldsymbol{\beta}} (\mathbf{y} - \mathbf{X}\boldsymbol{\beta})'\mathbf{W}(\mathbf{y} - \mathbf{X}\boldsymbol{\beta}) \text{ .}$$

For theoretical reasons (See Fan and Gijbels, 1995 and Porter, 2003) it is preferable to estimate odd ordered polynomial models. In our estimates α we simply estimate a local linear regression (p = 1). For our estimate of α we then estimate three local linear regressions for $E(y_i \mid x_i \geq \tilde{x})$ and $E(D_i \mid x_i \geq \tilde{x})$ and use data from the right of the cut point only, and for $E(y_i \mid x_i < \tilde{x})$ we use data to the left of the cut point. Letting x_- be the closet integer to the "left" of $\tilde{x}$ (which in out case equals $\tilde{x}$ - 1) and x_+ be the point closest interger to the "right" of $\tilde{x}$ (which in out case equals $\tilde{x}$), the estimated value of α equals

$$\hat{\alpha} = \frac{\hat{E}(y \mid x_+) - \hat{E}(y \mid x_-)}{\hat{E}(D \mid x_+)} \text{ .}$$

To implement this technique it is necessary to choose a bandwidth. We choose the bandwidth that minimizes the asymptotic mean squared error.

Let, $s_{r,0} = \int_0^\infty K(u)u^r du$.

Then, it can be shown that for a local linear regression model the optimal bandwidth, to the left of the cut-point equals, (see Fan and Gijbels, 1992, 1995) equals

$$h_{opt}(x_o) = C(K)\left[\frac{\sigma^2(x_0)}{\{m''(x_0)\}^2 f(x_o)}\right]^{1/5} n^{-1/5}$$

where

$$C(K) = \left[\frac{\int_0^\infty \left[s_{2,0} - ts_{1,0}\right] K^2(t)dt}{\left\{s_{2,0}^2 - s_{1,0}s_{3,0}\right\}^2}\right]^{1/5},$$

$\sigma^2(x_0)$ is the variance of ε at x_0, $m''(x_0)$ is the second derivative of m at x_0, and $f(x_0)$ is the density of x at x_0. For the Gaussian kernel function C(K) = 0.794. Several of these quantities are unknown and so we employ a two step method to obtain the optimal bandwidth.

In the first step we compute what is termed the "Rule of Thumb" (ROT) bandwidth which we denote h_{ROT}. To compute h_{ROT} a forth order polynomial is estimated globally (i.e., with all data weighted equally). From these estimates we compute

$$\tilde{m}(x) = \hat{\beta}_0 + \cdots + \hat{\beta}_4 x^4$$

which results in

$$\tilde{m}''(x) = 2\hat{\beta}_2 + 6\hat{\beta}_3 x + 12\hat{\beta}_4 x^2$$

and $\hat{\sigma}^2$ where

$$\tilde{\sigma}^2 = \sum_{i=1}^{N} (y_i - \tilde{m}(x_i))^2 / (N-5) .$$

Then,

$$h_{ROT} = 0.794 \left[\frac{\tilde{\sigma}^2}{\sum_{i=1}^{n} \left\{ \tilde{m}''(x_i) \right\}^2} \right]^{1/5}$$

In the second step we estimate a 3rd order local polynomial regression using bandwidth h_{ROT} to compute

$$h_{opt}(x_0) = 0.794 \left[\frac{\hat{\sigma}^2(x_0)}{\sum_{i=1}^{n} \left\{ \hat{m}''(x_i) K_{h_{ROT}}(x_i - x_0) \right\}^2} \right]^{1/5}$$

where

$$\tilde{\sigma}^2 = \sum_{i=1}^{N} (y_i - \hat{m}(x_i))^2 / \operatorname{tr}\left\{ \mathbf{W} - \mathbf{WX}(\mathbf{X}'\mathbf{W}X)^{-1}\mathbf{X}'\mathbf{W} \right\} .$$

To calculate the standard errors of the estimate $\hat{\alpha}$ we employed bootstrapped techniques using 1000 replications where we recompute the optimal bandwidths for each replication.

Appendix C
Estimates for Different Subgroups
Table C1

IV Estimates of the Impact of GMS on Various Outcome Variables by Race, Gender, Parental Education, and SAT score

Outcome Variables	Baseline Survey (1)	1st Follow-up Survey (3)	2nd Follow-up Survey (5)
	African Americans		
Scholarships	$2,102.66	$6,499.00	$5,350.79
	($1,751.52)	($1,591.75)	($2,658.29)
Enrollment	-0.039	0.030	-0.010
	(0.033)	(0.032)	(0.058)
Private School Attendance	0.213	0.141	0.140
	(0.040)	(0.058)	(0.104)
Loans	-$975.25	-$6,740.79	-$16,125.42
	($466.56)	($1,240.12)	($3,003.96)
Parental Support	-$270.37	-$126.94	$812.86
	($457.31)	($336.26)	($678.33)
Weekly Hours Worked	-5.97	-5.17	-0.96
	(1.70)	(2.50)	(5.82)
Earnings	-$48.18	-$44.65	$31.73
	($15.89)	($27.53)	($81.17)
	Asian Americans		
Scholarships	$3,841.24	$4,670.39	$4,908.24
	($1,769.55)	($1,989.85)	($6,337.43)
Enrollment	-0.020	0.084	0.436
	(0.033)	(0.058)	(0.216)
Private School Attendance	-0.085	-0.198	-0.410
	(0.097)	(0.155)	(0.178)
Loans	-$2,094.39	-$10,497.86	$2,357.19
	($1,516.71)	($2,757.45)	($8,385.99)
Parental Support	-$1,610.63	-$5,947.79	-$274.62
	($1,166.03)	($1,599.35)	($3,005.46)
Weekly Hours Worked	-13.95	-15.13	-28.50
	(4.05)	(5.57)	(22.20)
Earnings	-$121.12	-$163.65	-$294.29
	($37.00)	($66.46)	($304.44)

	Latinos		
Scholarships	$1,560.06	$6,385.57	$9,132.04
	($884.48)	($3,249.20)	($2,537.99)
Enrollment	-0.039	-0.039	0.028
	(0.033)	(0.033)	(0.154)
Private School Attendance	-0.075	-0.066	0.210
	(0.085)	(0.104)	(0.087)
Loans	-$2,956.57	-$7,186.13	-$8,891.33
	($2,142.07)	($1,639.92)	($4,214.64)
Parental Support	-$462.52	-$898.58	-$18.19
	($593.14)	($498.40)	($432.87)
Weekly Hours Worked	0.43	-5.76	2.86
	(3.80)	(3.03)	(4.30)
Earnings	-$2.90	-$41.81	-$65.32
	($32.96)	($37.77)	($85.49)
	Females		
Scholarships	$2,110.00	$4,056.71	$4,980.25
	($1,280.30)	($1,549.84)	($2,327.40)
Enrollment	0.020	0.031	-0.004
	(0.014)	(0.030)	(0.093)
Private School Attendance	0.041	-0.025	0.087
	(0.063)	(0.062)	(0.094)
Loans	-$1,637.48	-$6,374.94	-$12,993.14
	($1,083.79)	($904.41)	($2,362.33)
Parental Support	-$660.04	-$1,519.39	$908.88
	($492.35)	($647.29)	($469.22)
Weekly Hours Worked	-3.03	-7.67	-4.12
	(2.58)	(2.49)	(4.50)
Earnings	-$33.97	-$70.61	-$77.94
	($20.87)	($30.24)	($55.49)
	Males		
Scholarships	$3,504.55	$11,172.90	$8,445.94
	($2,260.36)	($1,705.08)	($4,115.15)
Enrollment	0.020	-0.019	0.201
	(0.030)	(0.035)	(0.116)
Private School Attendance	0.105	0.031	0.098
	(0.113)	(0.100)	(0.153)
Loans	-$2,018.91	-$11,411.15	-$6,657.71
	($480.92)	($2,488.95)	($2,692.13)
Parental Support	-$752.26	-$1,883.32	-$731.61
	($574.37)	($835.96)	($1,031.20)
Weekly Hours Worked	-11.00	-7.63	-1.37
	(3.34)	(3.04)	(7.17)
Earnings	-$83.58	-$77.77	-$9.22
	($26.50)	($48.24)	($154.76)

Parents: No College			
Scholarships	$2,340.08	$5,135.47	$7,254.10
	($882.82)	($1,481.69)	($2,062.74)
Enrollment	0.023	0.020	0.016
	(0.017)	(0.034)	(0.089)
Private School Attendance	0.047	0.002	0.107
	(0.058)	(0.048)	(0.109)
Loans	-$2,254.03	-$7,886.01	-$10,573.02
	($1,162.60)	($1,312.26)	($2,570.20)
Parental Support	-$480.50	-$1,087.44	$274.20
	($343.61)	($354.10)	($417.71)
Weekly Hours Worked	-1.92	-7.42	-2.02
	(1.48)	(2.56)	(4.00)
Earnings	-$25.19	-$85.64	-$43.89
	($14.42)	($28.61)	($62.73)
Parents: College			
Scholarships	$3,022.44	$7,050.56	$6,762.76
	($1,877.59)	($2,377.66)	($3,689.12)
Enrollment	0.029	0.011	0.204
	(0.031)	(0.039)	(0.097)
Private School Attendance	0.099	-0.082	-0.049
	(0.114)	(0.117)	(0.164)
Loans	-$132.25	-$6,750.24	-$14,441.17
	($1,045.91)	($2,054.93)	($3,255.76)
Parental Support	-$627.50	-$2,406.64	$612.43
	($1,028.93)	($964.79)	($799.38)
Weekly Hours Worked	-12.06	-7.44	-3.72
	(4.31)	(3.39)	(6.31)
Earnings	-$86.99	-$49.21	-$3.41
	($35.75)	($45.74)	($106.50)
Below Median SAT score			
Scholarships	$2,154.35	$7,925.33	$5,519.90
	($1,545.26)	($1,541.88)	($2,224.01)
Enrollment	-0.007	-0.002	-0.034
	(0.015)	(0.027)	(0.070)
Private School Attendance	0.177	0.051	0.302
	(0.059)	(0.059)	(0.088)
Loans	-$1,652.11	-$5,114.73	-$8,612.53
	($1,336.12)	($1,433.44)	($3,258.77)
Parental Support	$237.85	-$650.67	$139.64
	($548.89)	($394.82)	($379.97)
Weekly Hours Worked	-5.83	-6.55	-0.75
	(2.96)	(2.31)	(5.08)
Earnings	-$58.25	-$60.11	$3.43
	($23.00)	($20.42)	($107.79)

Above Median SAT score			
Scholarships	$3,357.35	$7,134.11	$9,907.07
	($1,898.39)	($2,004.89)	($3,275.47)
Enrollment	0.024	0.041	0.235
	(0.021)	(0.021)	(0.111)
Private School Attendance	0.005	0.073	0.057
	(0.093)	(0.112)	(0.124)
Loans	-$1,902.30	-$9,742.06	-$13,188.53
	($727.43)	($1,980.42)	($4,988.86)
Parental Support	-$1,360.03	-$1,981.24	$446.30
	($796.75)	($1,135.64)	($1,274.40)
Weekly Hours Worked	-3.57	-8.48	-2.63
	(2.61)	(3.88)	(6.73)
Earnings	-$26.74	-$69.04	-$110.24
	($25.60)	($55.74)	($68.29)

Notes: Robust standard errors clustered by test score are in parentheses. Estimates are restricted to individuals whose test score within 10 points of the cutoff. The controls for estimtates by race are a cohort dummy, test score and its square, and the interaction of the cohort dummy with test score and its square. For estimates broken by gender, parental education or SAT score the controls for race, cohort, test score and its square, and all possible 2 and 3 way interactions between race and cohort and test score and its square are included.

Table C2

IV Estimates of the Impact of GMS on Additional Outcome Variables by Race, Gender, Parental Education, and SAT score

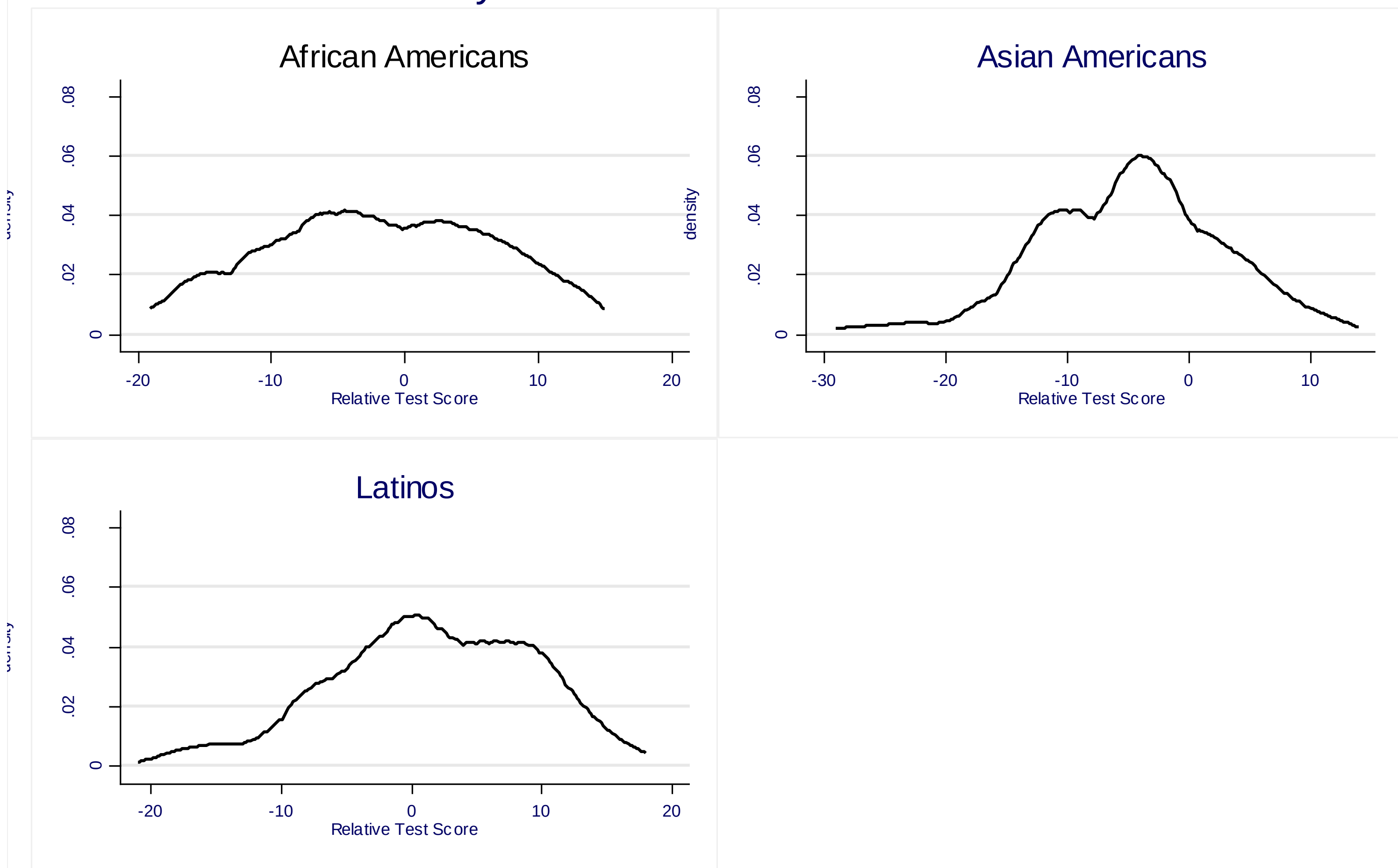
Outcome Variables	African American (1)	Asian American (2)	Latinos (3)	Females (4)	Males (5)
Social Sciences Major[a)]	0.027	0.093	0.015	0.080	-0.080
	(0.068)	(0.128)	(0.066)	(0.061)	(0.091)
STEM Major[a)]	-0.135	-0.084	0.026	-0.154	0.179
	(0.054)	(0.132)	(0.100)	(0.065)	(0.077)
Humanities Major[a)]	-0.046	0.159	-0.063	-0.011	0.179
	(0.056)	(0.083)	(0.042)	(0.040)	(0.077)
Education Major[a)]	-0.007	-0.005	0.025	0.023	0.010
	(0.037)	(0.036)	(0.051)	(0.030)	(0.050)
Professional School Major[a)]	0.151	-0.152	-0.042	0.024	-0.037
	(0.098)	(0.108)	(0.106)	(0.079)	(0.037)
Complete College	0.008	-0.250	-0.034	-0.068	-0.031
	(0.118)	(0.124)	(0.112)	(0.072)	(0.133)
Attending Graduate School	-0.098	-0.038	0.096	-0.068	0.102
	(0.126)	(0.190)	(0.158)	(0.086)	(0.159)
Applied to Graduate School/Not in School	0.346	0.298	0.301	0.396	0.089
	(0.132)	(0.208)	(0.126)	(0.108)	(0.190)
Earnings/ Not in School	$333.54	-$26,083.66	$661.54	-$7,250.45	-$557.84
	($8,167.26)	($6,159.90)	($4,142.64)	($5,103.34)	($3,955.96)
Educational Services/Not in School	0.067	0.410	0.169	0.180	0.247
	(0.091)	(0.145)	(0.192)	(0.131)	(0.133)
Professional Occupation/ Not in School	0.046	0.066	0.293	0.148	0.084
	(0.105)	(0.130)	(0.183)	(0.065)	(0.178)

Outcome Variables	Parents: No College (6)	Parents: College (7)	SAT: Below Median (8)	SAT : Above Median (9)
Social Sciences Major[a)]	0.081	0.013	0.032	0.011
	(0.056)	(0.089)	(0.091)	(0.061)
STEM Major[a)]	-0.106	-0.074	-0.094	-0.002
	(0.080)	(0.074)	(0.073)	(0.085)
Humanities Major[a)]	-0.027	0.044	0.021	-0.039
	(0.036)	(0.050)	(0.058)	(0.053)
Education Major[a)]	-0.006	0.008	-0.034	0.038
	(0.041)	(0.037)	(0.060)	(0.029)
Professional School Major[a)]	0.052	0.000	0.027	-0.006
	(0.083)	(0.080)	(0.102)	(0.085)
Complete College	-0.086	-0.074	-0.008	-0.102
	(0.069)	(0.074)	(0.075)	(0.086)
Attending Graduate School	0.087	-0.120	-0.204	0.031
	(0.096)	(0.096)	(0.109)	(0.090)
Applied to Graduate School/Not in School	0.084	0.426	0.211	0.439
	(0.056)	(0.138)	(0.121)	(0.166)
Earnings/ Not in School	-$7,929.34	-$8,326.62	-$598.55	-$10,289.94
	($3,874.90)	($7,599.06)	($4,050.84)	($6,086.47)
Educational Services/Not in School	0.134	0.297	0.174	0.201
	(0.133)	(0.143)	(0.146)	(0.139)
Professional Occupation/ Not in School	0.070	0.221	0.179	0.123
	(0.127)	0.137	0.093	(0.094)

Notes: Robust standard errors clustered by test score are in parentheses. Estimates are restricted to individuals whose test score within 10 points of the cutoff. The controls for estimtates by race are a cohort dummy, test score and its square, and the interaction of the cohort dummy with test score and its square. For estimates broken by gender, parental education or SAT score the controls for race, cohort, test score and its square, and all possible 2 and 3 way interactions between race and cohort and test score and its square are included.

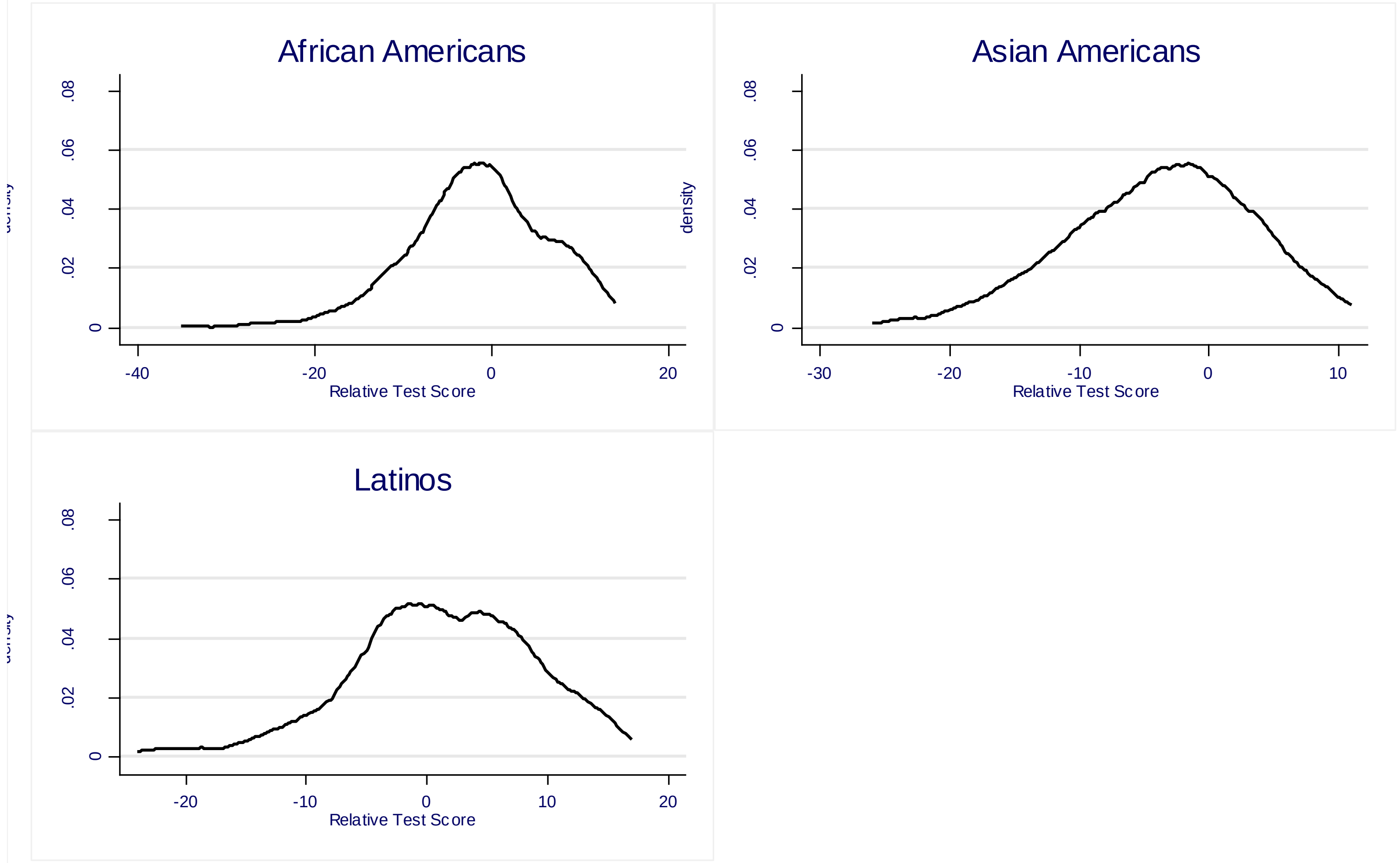
[a)] College major was determined in the 1st Follow-up survey. All other outcome variables were measured in the 2nd Follow-up survey

Figure 1
Smoothed Density Estimates of Relative Total Score: Cohort II



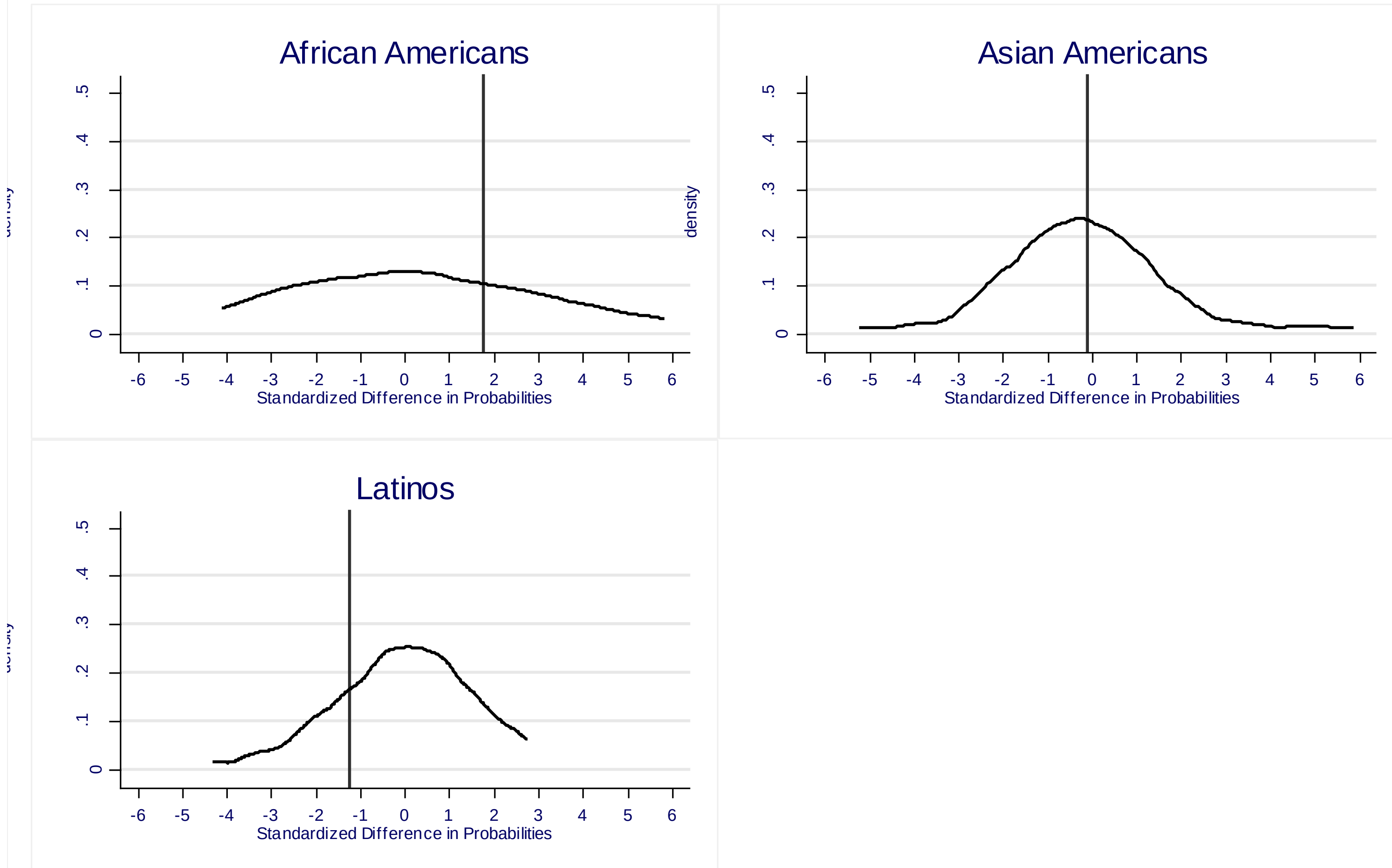
Source: Gates Millennium Scholar Surveys: Cohort II. The density function estimates are computed from the sample of 2340 applicants who were asked to complete the survey and are weighted to reflect the population of GMS applicants. Non-cognitve test scores are measured relative to the cut-point for each ethnic group

Figure 2
Smoothed Density Estimates of Relative Test Scores: Cohort III



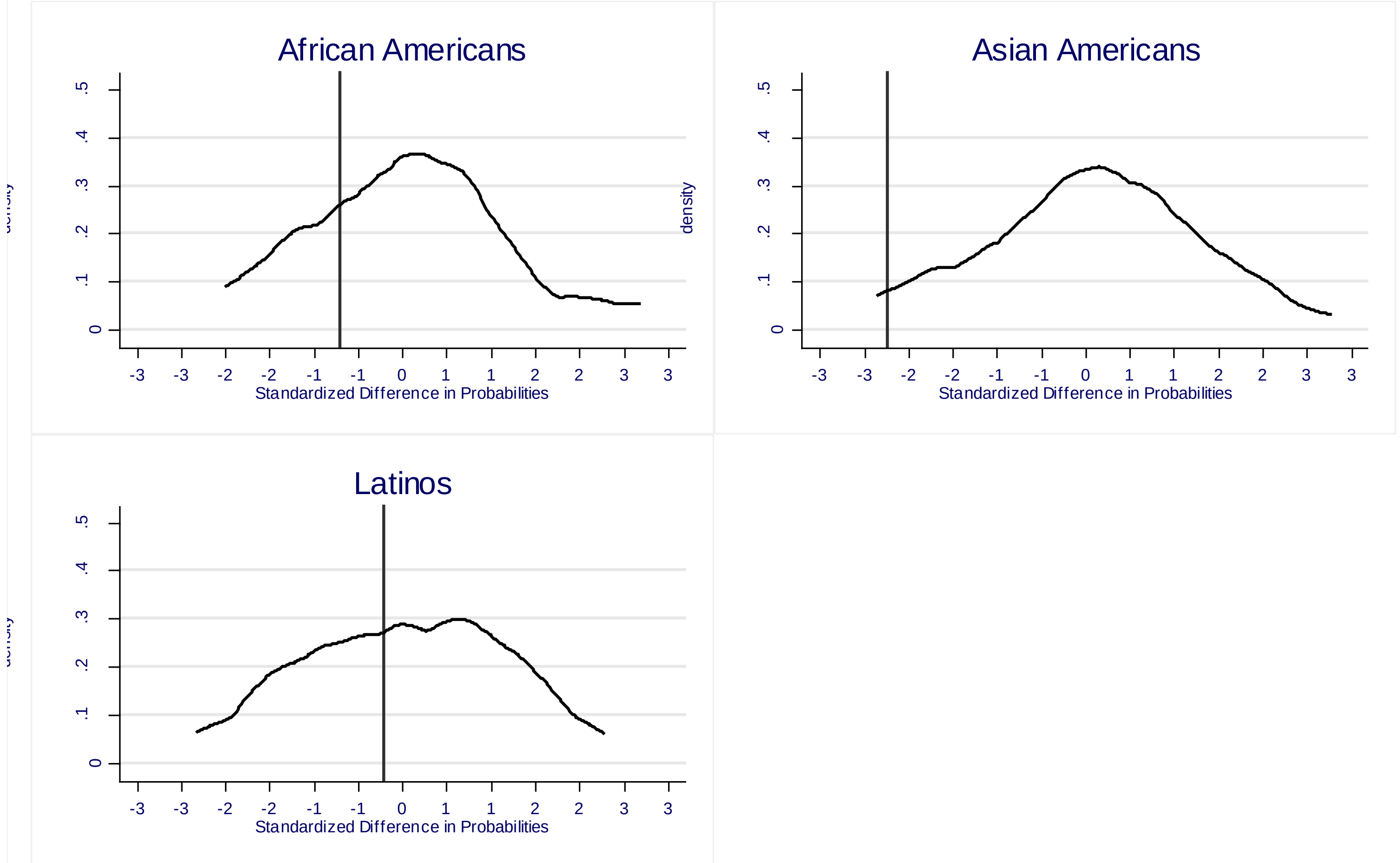
Source: Gates Millennium Scholar Surveys: Cohort III. The density function estimates are computed from the sample of 2340 applicants who were asked to complete the survey and are weighted to reflect the population of GMS applicants. Non-cognitve test scores are measured relative to the cut-point for each ethnic group

Figure 3
Smoothed Density Estimates of Standardized Difference in Probabilities: Cohort II



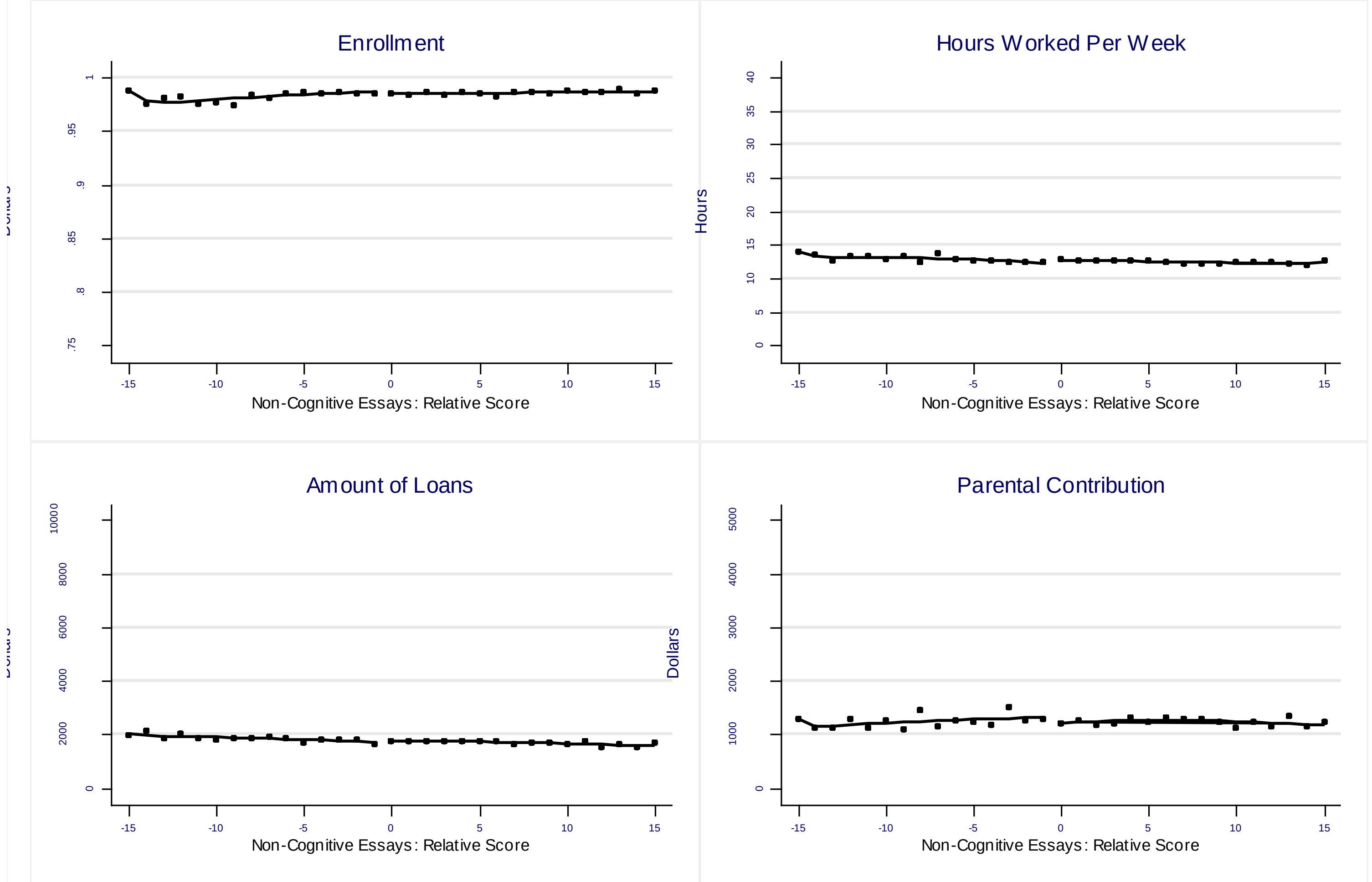
Source: Gates Millennium Scholar Surveys: Cohort II. The density function estimates are based on the sample of 2340 applicants who were asked to complete the survey and are weighted to reflect the population of GMS applicants. Vertical lines indicate the standardized change between cut-point+1 and cut-point

Figure 4
Smoothed Density Estimates of Standardized Difference in Probabilities: Cohort III



Source: Gates Millennium Scholar Surveys: Cohort III. The density function estimates are computed from the sample of 2340 applicants who were asked to complete the survey and are weighted to reflect the population of GMS applicants. Vertical lines indicate the standardized change between cut-point+1 and cut-point

Figure 5
Predicted Outcomes in Baseline Survey by Relative Non-Cognitive Score



Source: Gates Millennium Scholar Surveys: Cohort II & III.

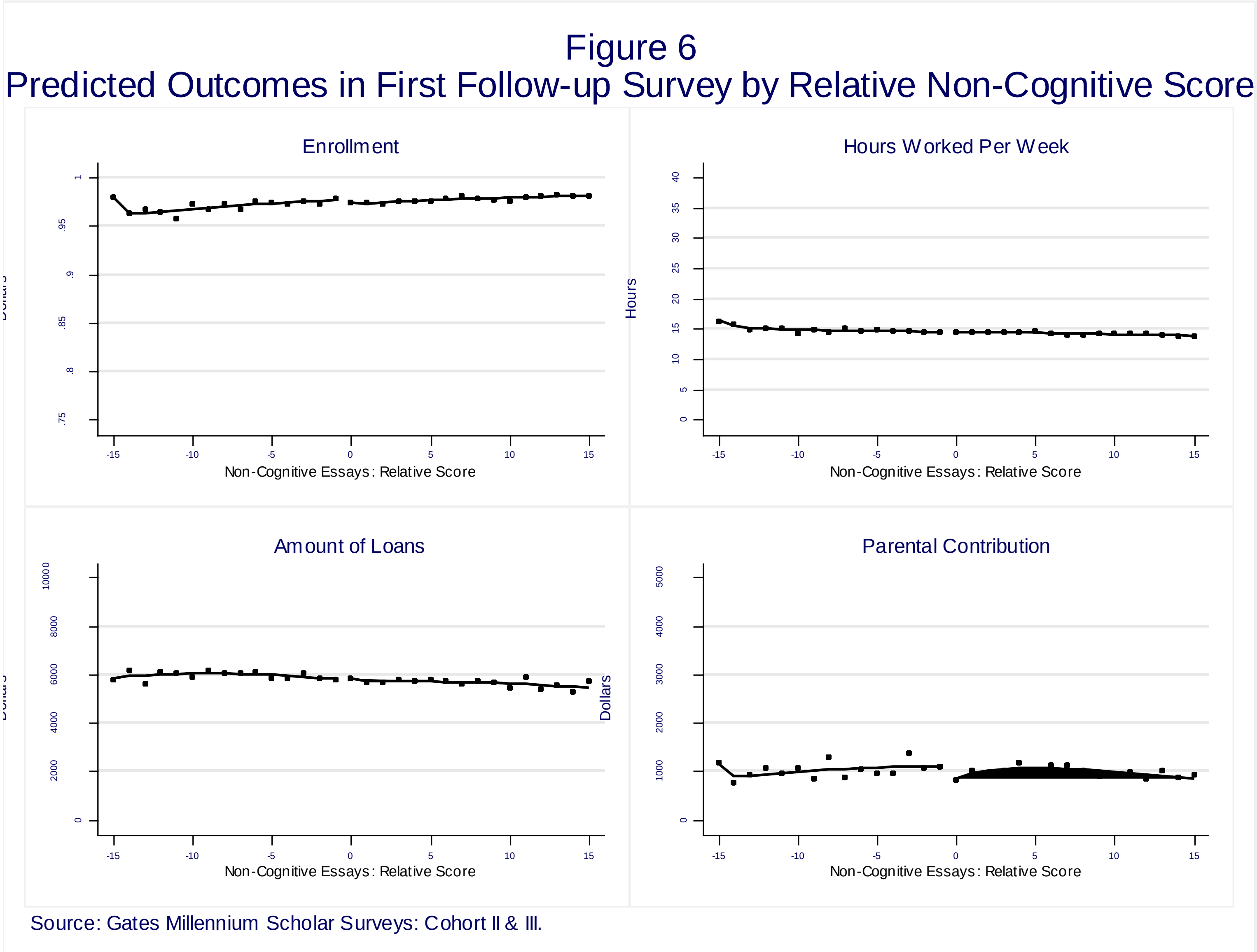
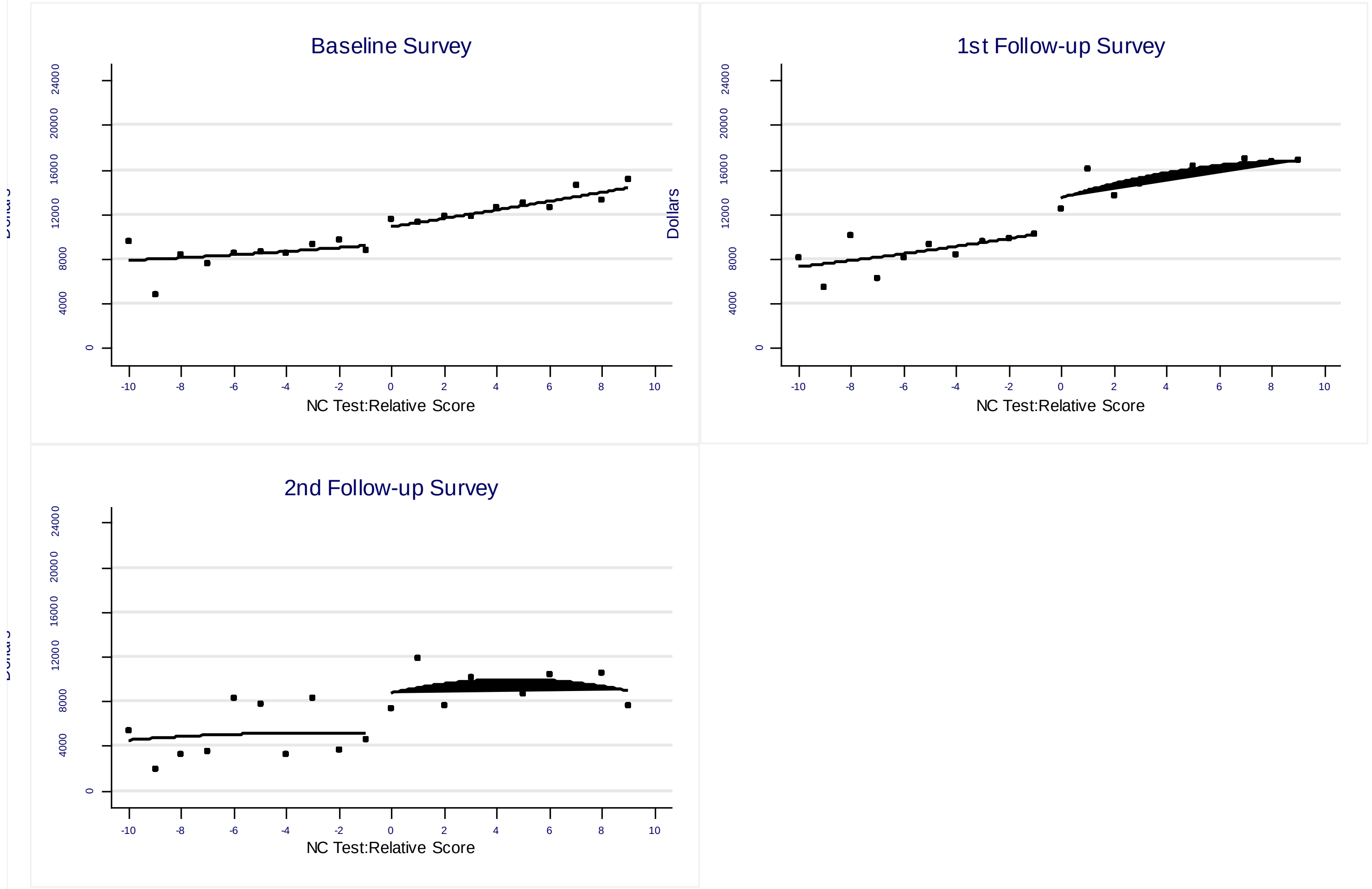


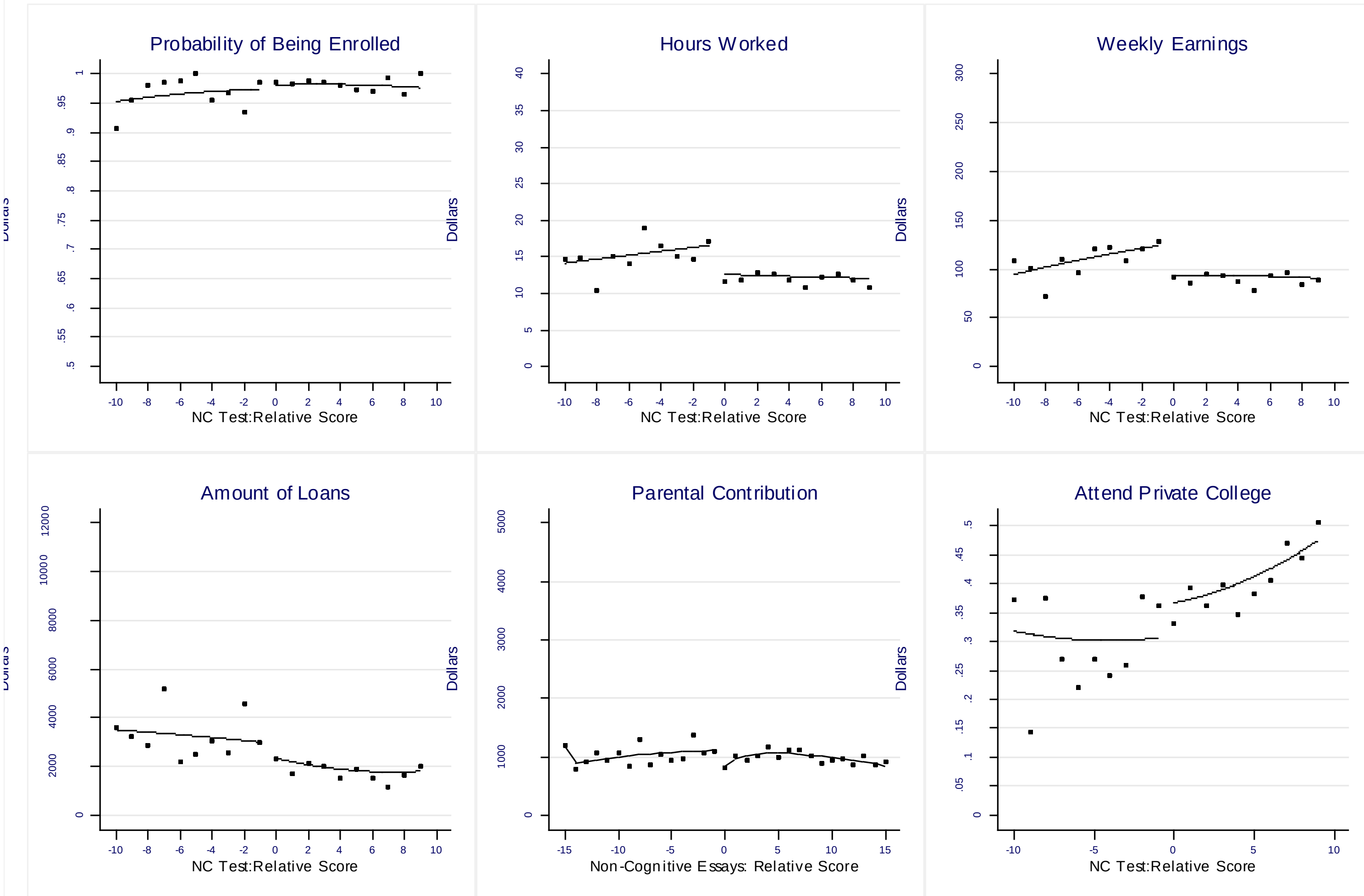
Figure 6
Predicted Outcomes in First Follow-up Survey by Relative Non-Cognitive Score
Enrollment
Hours Worked Per Week
Hours
Amount of Loans
Parental Contribution
Dollars
Non-Cognitive Essays: Relative Score
Non-Cognitive Essays: Relative Score
Non-Cognitive Essays: Relative Score
Non-Cognitive Essays: Relative Score
Source: Gates Millennium Scholar Surveys: Cohort II & III.

Figure 7
Regression Discontinuity Estimates: Total Scholarship Amounts



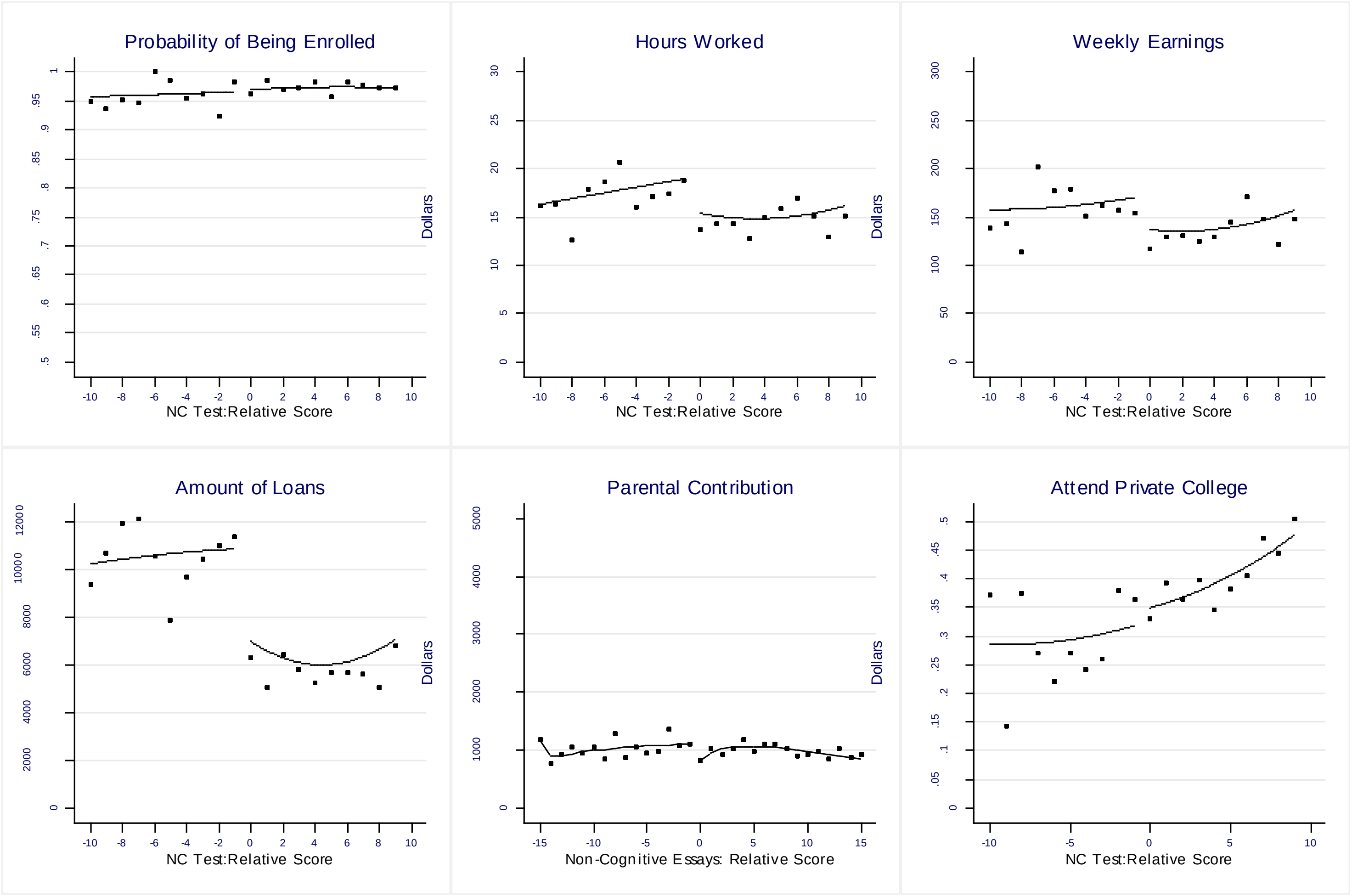
Source: Gates Millennium Scholar Surveys: Cohort II & III.

Figure 8
Regression Discontinuity Estimates: Baseline Interview



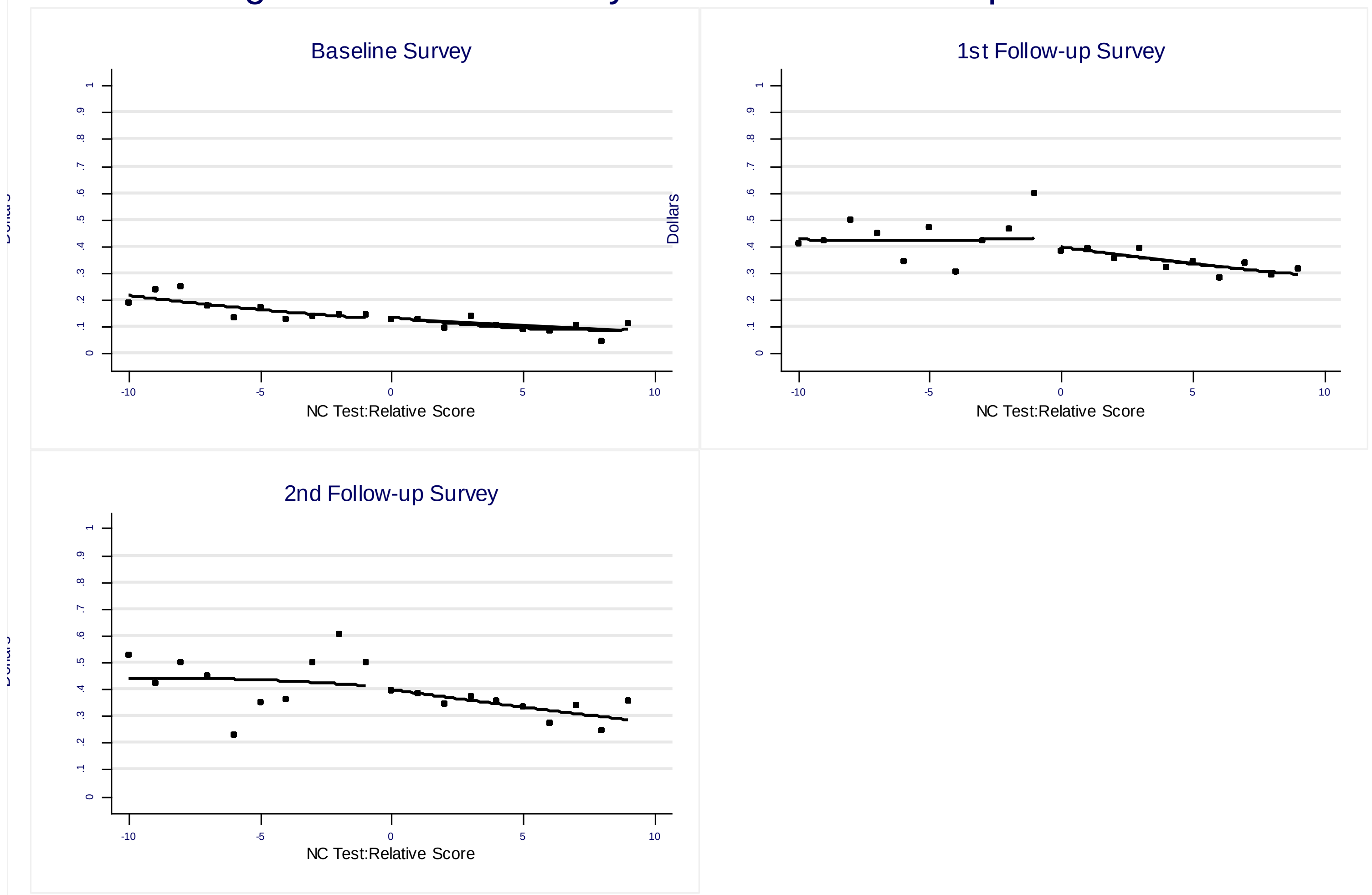
Source: Gates Millennium Scholar Surveys: Cohort II & III.

Figure 9
Regression Discontinuity Estimates: 1st Follow-up Interview



Source: Gates Millennium Scholar Surveys: Cohort II & III.

Figure 10
Regression Discontinuity Estimates: Non-Response Rates



Source: Gates Millennium Scholar Surveys: Cohort II & III.

Table 1

(a) Application Outcome by Cohort

Reason	Cohort II	Cohort III
Below Cut Score on Non-Cognitive Test	2,057	1513
Declined GMS Scholarship	3	8
GPA Ineligible	4	15
Incomplete Submissions	564	71
Institution Ineligible	4	0
No Record Of Financial Aid	13	8
Pell Ineligible	424	382
Scholar	1,000	1,000
Total	4069	2997

(b) Survey Response Rates

	Cohort II	Cohort III
Scholars		
Surveyed	1,000	1,000
Responding	830	897
Response Rate	83.00%	89.70%
Non-Scholars		
Surveyed	1340	1333
Responding	778	996
Response Rate	58.06%	74.72%
Responders Below Cut-Score	198	737
Percent Below Cut-Score	25.45%	74.00%

Source: Gates Millennium Scholarship Program Cohorts II & III.

Table 2
Sample Means and Means Just Above and Below the "Cut Points"
for Demographic and High School Background Variables

				All Applicants with Total Non-Cognitive Scores Equal to the…		
				Cut Score	Cut Score - 1	
	Full Sample	GMS Scholars	Non-Scholars			p-value
Variable Name	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
SAT Verbal+Math Score	1121.63	1130.40	1113.38	1110.85	1129.04	0.48
Attended Religious High School	0.06	0.06	0.06	0.06	0.03	0.20
Attended Private High School	0.07	0.07	0.07	0.08	0.03	0.09
Years of High School Math	3.87	3.89	3.85	3.89	3.84	0.58
Years of High School Science	3.65	3.65	3.66	3.63	3.66	0.41
Family Size	3.77	3.77	3.77	3.71	3.87	0.32
Born in U.S.	0.61	0.62	0.61	0.60	0.60	0.52
Family Owns Home	0.51	0.48	0.55	0.46	0.55	0.52
Male	0.29	0.30	0.29	0.29	0.29	0.41
Father's education						0.30
Less Than High school	0.20	0.24	0.17	0.16	0.19	
High School	0.27	0.27	0.26	0.28	0.25	
Some College	0.21	0.19	0.23	0.19	0.2	
BA/BS Degree	0.14	0.12	0.16	0.19	0.11	
Post BA/BS Degree	0.10	0.09	0.12	0.09	0.15	
Missing	0.08	0.09	0.07	0.09	0.1	
Mother's education						0.98
Less Than High School	0.19	0.23	0.16	0.19	0.18	
High School	0.25	0.26	0.24	0.27	0.19	
Some College	0.28	0.27	0.29	0.28	0.31	
BA/BS Degree	0.18	0.15	0.21	0.16	0.20	
Post BA/BS Degree	0.07	0.06	0.07	0.06	0.06	
Missing	0.02	0.02	0.02	0.03	0.05	
Sample Size	3181	1535	1646	172	131	

Notes: Cohorts II and III combined. Cut scores for total non-cognitive score were 71, 72 and 68 for African Americans, Asian Americans and Latinos, respectively in Cohort II and 72, 75 and 69 for African Americans, Asian Americans and Latinos, respectively for Cohort III. All tests of differences were Fisher exact tests for equality based on categorical data except for family size and SAT scores which were simple t-tests for differences in means.

Table 3
Sample Averages of Outcome Variables
by GMS receipt

Outcome Variable	GMS recipients (2)	Non-recipients (3)	p-value (6)
Baseline Survey			
Total Scholarships	$14,757.40	$8,501.70	0.000
Enrollment	0.99	0.96	0.000
Private School Attendance	0.42	0.34	0.000
Loans	$974.40	$3,198.18	0.000
Parental Support	$744.76	$2,690.89	0.000
Weekly Hours Worked	11.08	15.02	0.000
Weekly Earnings	$87.35	$123.88	0.000
1st Follow-up Survey			
Total Scholarships	$18,284.28	$8,895.68	0.000
Enrollment	0.98	0.95	0.000
Private School Attendance	0.42	0.32	0.000
Loans	$3,338.98	$9,969.69	0.000
Parental Support	$706.64	$2,117.40	0.000
Weekly Hours Worked	13.33	17.57	0.000
Weekly Earnings	$119.86	$162.75	0.000
STEM Major	0.41	0.43	0.129
Social Science Major	0.18	0.18	0.024
Humanities Major	0.21	0.10	0.162
Education Major	0.07	0.05	0.048
Professional Major	0.17	0.21	0.005
2nd Follow-up Survey			
Enrolled Undergraduate:	0.33	0.32	0.534
Total Scholarships	$11,849.06	$4,382.96	0.000
Private School Attendance	0.23	0.16	0.009
Loans	$6,623.32	$15,423.61	0.000
Parental Support	$397.43	$1,138.16	0.000
Weekly Hours Worked	18.45	22.80	0.000
Weekly Earnings	$221.80	$271.28	0.023
Graduated College:	0.63	0.60	0.090
Enrolled Graduate School	0.39	0.32	0.000
Professional Occupation if Working	0.38	0.30	0.013
Average Earnings if Working	$33,031.29	$30,645.18	0.029
Applied to Graduate school if Working	0.40	0.26	0.000

Notes: Cohorts II and III combined.

Table 4

IV Regression Estimates of the Impact of GMS on Different Outcome Variables

	Baseline Survey		1st Follow-up Survey		2nd Follow-up Survey	
	Base set of Control Variables	Additional Control Variables	Base set of Control Variables	Additional Control Variables	Base set of Control Variables	Additional Control Variables
Outcome Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Scholarships	$2,389.34	$3,059.95	$6,053.93	$6,900.23	$6,727.60	$6,978.11
	($956.88)	($1,086.20)	($1,301.41)	($1,206.19)	($1,610.84)	($1,811.74)
Enrollment	0.018	0.017	0.018	0.022	0.071	0.066
	(0.012)	(0.011)	(0.024)	(0.025)	(0.071)	(0.054)
Private School Attendance	0.061	0.073	-0.002	0.023	0.090	0.128
	(0.054)	(0.046)	(0.054)	(0.051)	(0.074)	(0.081)
Loans	-$1,653.02	-$1,507.38	-$7,688.16	-$7,406.68	-$11,163.89	-$12,004.19
	($838.13)	($852.68)	($1,021.27)	($1,171.24)	($2,214.26)	($2,628.68)
Parental Support	-$666.11	-$188.73	-$1,616.72	-$1,288.21	$366.80	$584.52
	($411.54)	($391.41)	($496.23)	($478.16)	($450.95)	($450.87)
Weekly Hours Worked	-5.16	-5.42	-7.55	-7.51	-3.29	-3.67
	(1.81)	(1.95)	(2.20)	(2.21)	(3.96)	(4.44)
Weekly Earnings	-$46.00	-$47.08	-$69.85	-$67.88	-$47.42	-$61.74
	($15.99)	($17.27)	($28.09)	($28.94)	($56.68)	($65.75)

Notes: Robust standard errors clustered by test score are in parentheses. Estimates are restricted to individuals whose test score within 10 points of the cutoff. The base set of controls include controls for race, cohort, test score and its square, and all possible 2 and 3 way interactions between race and cohort and test score and its square. Models with additional controls also include controls for gender, mother's and father's education, family size, whether an individual went to a public, private or religious high school, number of years of mathematics in high school, number of years of science in high school, SAT score and parental income as well as dummy variables indicating whether the value is missing for the particular variable is missing for the respondent.

Table 5

IV Regression Estimates of the Impact of GMS on Additional Outcome Variables

Outcome Variables	Base set of Control Variables (1)	Additional Control Variables (2)
Social Sciences Major[a)]	0.038	0.052
	(0.050)	(0.056)
STEM Major[a)]	-0.047	-0.035
	(0.058)	(0.053)
Humanities Major[a)]	-0.006	-0.002
	(0.050)	(0.031)
Education Major[a)]	0.004	-0.013
	(0.025)	(0.025)
Professional School Major[a)]	0.019	0.009
	(0.070)	(0.071)
Complete College	-0.065	-0.050
	(0.065)	(0.069)
Attending Graduate School	-0.023	-0.024
	(0.075)	(0.076)
Applied to Graduate School/Not in School	0.316	0.335
	(0.095)	(0.078)
Earnings/ Not in School	-$7,182.29	-$6,022.88
	($4,011.91)	($4,009.25)
Educational Services/ Not in School	0.201	0.214
	(0.103)	(0.114)
Professional Specialty Occupation/ Not in School	0.137	0.124
	(0.073)	(0.081)

Notes: Robust standard errors clustered by test score are in parentheses. Estimates are restricted to individuals whose test score within 10 points of the cutoff. The base set of controls include controls for race, cohort, test score and its square, and all possible 2 and 3 way interactions between race and cohort and test score and its square. Models with additional controls also include controls for gender, mother's and father's education, family size, whether an individual went to a public, private or religious high school, number of years of mathematics in high school, number of years of science in high school, SAT score and parental income as well as dummy variables indicating whether the value is missing for the particular variable is missing for the respondent.

[a)] College major was determined in the 1st Follow-up survey. All other outcome variables were measured in the 2nd Follow-up survey

Table 6

IV Regression Estimates of the Impact of GMS on Different Outcome Variables: College Fixed Effects

	Baseline Survey		1st Follow-up Survey		2nd Follow-up Survey	
	Base set of Control Variables	Additional Control Variables	Base set of Control Variables	Additional Control Variables	Base set of Control Variables	Additional Control Variables
Outcome Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Scholarships	$1,709.54	$2,112.58	$5,796.61	$5,721.12	$6,971.26	$7,387.86
	($1,186.54)	($1,200.15)	($1,466.46)	($1,488.44)	($3,665.78)	($4,169.02)
Loans	-$2,617.53	-$2,469.86	-$7,647.92	-$7,717.47	-$13,755.10	-$16,099.32
	($581.59)	($604.80)	($1,534.30)	($1,585.11)	($4,216.19)	($4,617.70)
Parental Support	-$705.31	-$228.13	-$1,716.99	-$1,499.34	$406.49	$975.57
	($456.52)	($441.53)	($547.81)	($548.63)	($847.18)	($918.38)
Weekly Hours Worked	-5.08	-5.36	-8.35	-8.41	-6.27	-5.81
	(1.92)	(2.00)	(2.22)	(2.30)	(5.82)	(6.43)
Weekly Earnings	-$40.52	-$41.71	-$73.20	-$71.23	-$73.90	-$109.77
	($16.32)	($17.00)	($29.08)	($30.03)	($148.44)	($165.20)
Social Sciences Major[a)]			0.011	0.015		
			(0.062)	(0.064)		
STEM Major[a)]			-0.007	-0.006		
			(0.076)	(0.078)		
Humanities Major[a)]			-0.024	-0.006		
			(0.047)	(0.062)		
Education Major[a)]			-0.017	-0.020		
			(0.036)	(0.049)		
Professional School Major[a)]			0.029	0.026		
			(0.061)	(0.063)		
Complete College	-	-	-	-	-0.042	-0.011
					(0.074)	(0.075)
Attending Graduate School	-	-	-	-	-0.110	-0.127
					(0.106)	(0.105)

Notes: Estimates are restricted to individuals with test scores within 10 points of the cutoff. The base set of controls include race, cohort, test score and its square, and all possible 2 and 3 way interactions between race and cohort and test score and its square. Models with additional controls also include gender, mother's and father's education, family size, whether an individual went to a public, private or religious high school, number of years of high school mathematics and number of years of science, SAT score and parental income as well as dummy variables indicating whether the value is missing for the particular variable is missing for the respondent.

Table 7
Estimated Impact of GMS on Selected Outcome Variables
RD estimates Based on Local Polynomial Regression with Optimal Bandwidth

Outcome	Baseline Survey (1)	1st Follow-up Survey (2)	2nd Follow-up Survey (3)
Scholarships	$3,859.77	$5,406.92	$6,519.40
	($1,521.24)	($1,831.02)	($2,402.10)
Enrollment	-0.012	-0.016	0.175
	(0.019)	(0.014)	(0.092)
Private School Attendance	0.034	-0.100	0.161
	(0.076)	(0.076)	(0.069)
Loans	-$115.00	-$8,886.35	-$16,185.48
	($712.41)	($1,774.71)	($4,437.91)
Parental Support	-$1,279.66	-$2,191.33	-$345.53
	($491.38)	($746.31)	($545.72)
Weekly Hours Worked	-6.23	-7.17	-1.50
	(2.42)	(2.06)	(4.01)
Weekly Earnings	$46.08	-$52.17	-$58.44
	($21.18)	($23.40)	($86.27)

Notes: Boostrapped standard errors based on 1000 replications are in parentheses. Only observations with test scores within 6 points of the cut-point are included. The estimates use the relative test score as the running variable.

Table 8
Estimated Impact of GMS on Outcome Variables
RD estimates based on Local Polynomial Regression with Optimal Bandwidth

Outcome Variables	Est. (B.S.E.)
Social Sciences Major[a)]	0.035
	(0.071)
STEM Major[a)]	-0.070
	(0.055)
Humanities Major[a)]	-0.007
	(0.045)
Education Major[a)]	-0.015
	(0.036)
Professional School Major[a)]	-0.101
	(0.079)
Complete College	-0.104
	(0.091)
Attending Graduate School	-0.015
	(0.081)
Applied to Graduate School/Not in School	0.147
	(0.057)
Earnings/ Not in School	-$4,189.71
	($4,098.18)
Educational Services/Not in School	0.115
	(0.102)
Professional Occupation/ Not in School	0.053
	(0.119)

Notes: Boostrapped standard errors based on 1000 replications are in parentheses. Bandwidth was set at the optimal bandwidth value which was recomputed for every repetition. Only observations with test scores within 6 points of the cut-point are included. The estimates use the relative test score as the running variable.

[a)] College major was determined in the 1st Follow-up survey. All other outcome variables were measured in the 2nd Follow-up survey

Table 9
Estimated Impact of GMS on the Probability of Non-Response

Survey	Parametric Estimates (1)	Non-Parametric Estimates (2)
Baseline	-0.075	-0.099
	(0.042)	(0.048)
1st Follow-up	-0.055	-0.111
	(0.061)	(0.063)
2nd Follow-up	-0.041	-0.095
	(0.040)	(0.064)

Notes: For the parametric estimates: robust standard errors clustered by test score are in parentheses, estimates are restricted to individuals whose test score within 10 points of the cutoff, and estimates include controls for race, cohort, test score and its square, and all possible 2 and 3 way interactions between race and cohort and test score and its square. Non-parametric estimates: boostrapped standard errors based on 1000 replications are in parentheses, only observations with test scores within 6 points of the cut-point are included and the estimates use the relative test score as the running variable.

Table 10
Estimated Impact of GMS on Selected Outcome Variables
Lee/Manski Upper and Lower Bounds

Outcome	RD Estimate (1)	Lower Bound (2)	Upper Bound (3)
	Baseline Survey		
Scholarships	$3,976.78	$119.25	$6,968.49
	($934.50)	($1,130.26)	($926.15)
Enrollment	0.026	0.025	0.033
	(0.013)	(0.013)	(0.012)
Private School Attendance	0.067	-0.010	0.120
	(0.046)	(0.054)	(0.051)
Loans	-$2,386.58	-$2,800.01	$312.06
	($560.07)	($547.18)	($656.39)
Parental Support	-$1,352.04	-$1,675.06	$42.63
	($276.20)	($255.53)	($350.78)
Weekly Hours Worked	-4.89	-5.65	6.98
	(1.42)	(1.68)	(1.59)
Weekly Earnings	-$40.61	-$63.81	$52.62
	($12.94)	($11.46)	($15.59)
	1st Follow-up Survey		
Scholarships	$7,276.95	$3,609.69	$8,552.91
	($1,298.04)	($1,740.20)	($1,346.06)
Enrollment	0.022	0.019	0.040
	(0.016)	(0.017)	(0.014)
Private School Attendance	0.025	-0.091	0.097
	(0.049)	(0.065)	(0.060)
Loans	-$7,952.48	-$8,624.25	-$5,011.05
	($959.02)	($896.74)	($1,046.66)
Parental Support	-$1,929.44	-$2,150.21	-$629.41
	($468.30)	($443.33)	($519.52)
Weekly Hours Worked	-5.97	-8.42	2.41
	(1.39)	(1.90)	(1.39)
Weekly Earnings	-$49.16	-$91.00	$27.11
	($15.68)	($14.22)	($18.10)
Social Sciences Major	-0.012	-0.165	0.019
	(0.038)	(0.068)	(0.043)
STEM Major	-0.012	-0.117	0.066
	(0.049)	(0.065)	(0.060)
Humanities Major	0.017	-0.145	0.040
	(0.032)	(0.065)	(0.036)
Education Major	0.002	-0.173	0.012
	(0.022)	(0.064)	(0.024)
Professional School Major	-0.002	-0.149	0.036
	(0.041)	(0.066)	(0.047)

2nd Follow-up Survey			
Scholarships	$8,717.30	$1,007.38	$14,298.24
	($2,046.15)	($3,634.57)	($2,488.82)
Enrollment	0.084	0.031	0.117
	(0.047)	(0.063)	(0.053)
Private School Attendance	0.143	-0.187	0.263
	(0.071)	(0.185)	(0.107)
Loans	-$8,046.63	-$10,610.42	-$4,462.64
	($1,839.67)	($1,878.62)	($2,234.23)
Parental Support	-$71.91	-$596.55	$2,694.36
	($330.55)	($266.74)	($968.58)
Weekly Hours Worked	-2.51	-5.64	9.41
	(3.34)	(5.00)	(3.30)
Weekly Earnings	-$23.83	-$133.74	$125.49
	($47.16)	($49.40)	($51.93)
Complete College	-0.063	-0.098	-0.012
	(0.048)	(0.055)	(0.063)
Attending Graduate School	0.043	-0.012	0.074
	(0.059)	(0.069)	(0.066)
Applied to Graduate School/Not in School	0.255	0.203	0.290
	(0.068)	(0.079)	(0.076)
Earnings/ Not in School	-$1,842.97	-$4,332.37	-$61.16
	($2,595.23)	($2,489.23)	($2,693.96)
Professional Occupation/ Not in School	0.126	0.080	0.166
	(0.081)	(0.089)	(0.090)

Notes: Estimates based on those with test scores within two points of cut-point and whose application was not rejected for any reason other than a test score below the cut-point.