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THE ECONOMIC IMPACT OF ACCESS TO PUBLIC FOUR-YEAR COLLEGES

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ABSTRACT

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Abstract

We estimate the economic impacts of students' access to an entire sector of U.S. public higher education. Approximately half of Georgia high school graduates who enroll in college do so in the state's public four-year sector, which requires minimum SAT scores for admission. Regression discontinuity estimates show enrollment in public four-year institutions boosts students' estimated household income around age 30 by about 17 percent and has even larger impacts for those from low-income high schools. Access to this sector has little clear impact on financial health or student loan balances. For the marginal student, and particularly for those from low-income high schools, enrollment in such institutions has large private returns in the short run and positive returns to state budgets in the long run.

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1. Introduction

To what extent do public colleges and universities improve economic mobility? Two-thirds of U.S. college students seeking bachelor's degrees enroll in public four-year institutions, which are partially subsidized by state appropriations.[1] Chetty et al. (2020) descriptively identify such colleges as catalysts for economic mobility, but we have relatively little causal evidence on whether access to this sector improves students' economic trajectories. Those who attend public four-year institutions differ from those who choose otherwise, making the question empirically challenging to answer. Rigorous evidence on the economic benefits of a college education would inform the discourse around public funding of these institutions.

We provide the first estimated economic impacts of access to an entire public sector of higher education in the U.S. We study Georgia, whose public higher education institutions have completion rates typical of the U.S. more broadly.[2] We specifically focus on the University System of Georgia (USG), which enrolls about half of Georgia's high school graduates who attend college. USG includes 17 public four-year universities that we call the University System of Georgia's universities (USGU), all of which require minimum SAT scores for admissions. This threshold generates exogenous variation in college access, allowing us to compare otherwise identical students who differ only in their college options. In Goodman, Hurwitz, and Smith (2017), we showed that students just above this SAT threshold for admission were more likely to attend USGU instead of two-year colleges or no college at all. Enrollment in these public four-year institutions substantially increased bachelor's and overall degree completion rates.

We show here that access to Georgia's public four-year sector leads to substantial economic benefits for the marginal student and for the state. We do so by linking all Georgia SAT takers from the high school classes of 2004-08 to credit bureau data on these individuals measured in November 2017, when they were in their late 20's and early 30's. The credit bureau data contain various measures of economic and financial well-being, including estimated household income, credit scores, outstanding debt, student loans, mortgages, and residential location. Many of these outcomes have not previously been measured in the literature on the economic returns to college

[1] See Table 3 of https://nscresearchcenter.org/wp-content/uploads/CurrentTermEnrollmentReport-Spring-2019.pdf.
[2] See Tables 5 and 15 of https://nscresearchcenter.org/signature-report-16-state-supplement-completing-college-a-state-level-view-of-student-completion-rates/.

in the U.S. or beyond. We can thus paint a fuller picture of the economic returns from attending public four-year colleges. We do so in four stages.

First, we replicate our earlier finding that access to and enrollment in the public four-year sector substantially boosts B.A. completion rates. To do so, we focus on the over 120,000 Georgia students who first took the SAT in senior year of high school, too late to retake the test in a potentially endogenous reaction to missing the publicly known admissions thresholds. Those just above the admissions threshold are five percentage points (18 percent) more likely to enroll in the USGU than those just below, who instead largely attend two-year colleges or no college at all. Instrumental variable estimates suggest access-driven enrollment in a public four-year institution relative to those alternatives increases B.A. completion rates by 37 percentage points, more than tripling the 14 percent B.A. completion rate among compliers denied access.

Second, and in our new results, we show that enrollment in such public four-year institutions substantially boosts estimated household income as measured around age 30, driven largely by students from low-income high schools. Enrollment in USGU increases estimated household income by around 17 percent, or $9,000 a year (in 2017 dollars). Although this estimate is marginally statistically insignificant, a series of alternative specifications yields similarly sized estimates, some of which are statistically significant. The increase in estimated household income is around 27 percent for students from low-income high schools. These results are robust to a variety of specification choices, suggesting that access to public four-year colleges yields large economic returns for the marginal student, particularly lower income ones.

Third, we use the credit bureau data to study the impact of public four-year college enrollment on other financial and residential outcomes. The overall impact of such enrollment on a financial health index (based on credit scores, payment delinquency and bankruptcy status) is positive but imprecisely estimated. The effect size is consistent with observed increases in estimated household income but wide confidence intervals leave substantial uncertainty. Estimated effects on home ownership rates, as proxied by mortgage debt, are always positive but statistically insignificant. Student loan balances at age 30 generally increase, and moreso for non-URM students and those from middle- and high-income high schools, but most estimates again have wide confidence intervals and magnitudes of these increases are somewhat sensitive to bandwidth. That the large impact on estimated household income does not translate into clear impacts on financial health is partly driven by wide confidence intervals but may also be due to the marginal student having higher student loan debt and lower income earlier in their career while enrolled in college. Longer-term impacts on financial health may be larger than those we can observe around age 30. Finally, we see little clear overall impact on Georgia residency, though URM students appear 15-20 percentage points more likely to remain in state by age 30. These residency results imply the marginal student is largely choosing between in-state college options and this lack of out-migration increases the state's returns on subsidies of its public four-year sector.

Fourth, we show that enrollment in the public four-year sector has positive private and public returns. For private returns to the marginal student, we show that short-run increases in tuition costs relative to the two-year sector or non-enrollment are rapidly offset by increased income. The private return to enrollment in a public four-year institution becomes positive and large early in a student's career. The marginal student's enrollment in a public four-year institution is a break-even proposition 10 years after initial enrollment, with a net present value over $80,000 after 20 years and over $130,000 after 30 years.

We compute public returns by comparing Georgia's increased expenditures on college subsidies for an additional student against the increased state income tax revenue from increased earnings. Between 10 and 20 years later, the state roughly breaks even on its initial investment and after 30 years the net present value of that investment is about $2,000. For students from low-income high schools, the state's NPV turns positive within 10 years and jumps to $26,000 after 30 years. The large increase in income tax revenue more than offsets the cost of subsidizing one additional student at a four-year campus. Accounting for additional effects of college education on co-worker productivity, sales tax revenue, and health would likely make this calculation even more favorable for the state, with even higher social returns once we account for increased federal income tax revenue. These estimated private and social returns for the marginal student implicitly hold current total college enrollment fixed. Expanding overall enrollment could generate different returns if there were general equilibrium effects on the wages of college and non-college students.

Our work makes two contributions to the literature. First, we extend prior work on college returns to generate among the first estimated economic impacts of American students' access to an entire public system of higher education. Recent well-identified work on returns to college access in the U.S. has largely exploited thresholds generated by one institution's admissions process (Hoekstra, 2009; Zimmerman, 2014). Well-identified research showing substantial returns to access to broader sets of institutions or degree programs has largely been conducted outside of the U.S., in countries with more centralized admissions systems, such as France (Canaan and Mouganie, 2018) or Chile (Hastings, Neilson and Zimmerman, 2014). Closer to our work is Mountjoy (2022), which uses distance instruments to estimate earnings impacts of community college access relative to no college or four-year colleges. Our work complements recent work on earnings by Bleemer (2021), studying access to a few University of California campuses, and Kozakowski (2023), studying access to Massachusetts' four-year state colleges, and Mountjoy (2024), studying marginal enrollees to public colleges in Texas. Results such as ours, based on a broad set of students and institutions, are critical both to understanding public and private returns to college and to providing the broader research community estimates to serve as benchmarks or parameters in a larger model (e.g., Chetty et al., 2020; Hendren and Sprung-Keyser, 2020).

Our estimated 15 percent increase in household income from public four-year college enrollment is fairly similar to estimates from other papers. Hoekstra (2009) finds that enrollment in a state's most selective public university access increases earnings by 20 percent (for white men) and Zimmerman (2014) finds that access to a state's least selective public four-year college increases earnings by 22 percent. Our results are consistent with the negative returns to community college enrollment that Mountjoy (2022) observes for students diverted from four-year colleges and somewhat larger than the 5-10 percent return estimated by Mountjoy (2024) for those on the margin of two-year and four-year enrollment. Kozakowski (2023) estimates a 26 percent return from access to the public four-year sector, or a nearly 45 percent return from enrollment in such colleges. Her low-income sample is comparable to our sub-sample from low-income high schools, for whom we observe returns on the order of 30 percent. Our work, combined with Zimmerman (2014), Kozakowski (2023) and Mountjoy (2024), helps establish something close to a consensus that returns to college access for students on the admissions margin are very high. This is true both for students and for governments, given that the investment eventually pays for itself and thus makes the marginal value of public funds here and in prior papers infinite (Hendren and Sprung-Keyser, 2020).

Our second main contribution is to expand the set of economic outcomes considered by most literature on returns to college. The U.S. papers discussed above measure outcomes using administrative data on earnings reported to state unemployment insurance agencies. Credit bureau data contains estimated household income, which includes the student's own wage earnings, spousal earnings and non-wage earnings, an arguably more complete measure of economic well-being than individual earnings might capture. We also observe a much wider range of financial outcomes than the typical state administrative data set. The only other papers in the higher education space to consider such outcomes are Scott-Clayton and Zafar (2019), on the impact of one state's merit scholarship program, and Boatman, Hurwitz, Lee and Smith (2019), on the impact of test-based college credit. These outcomes give a more complete picture of students' economic well-being than considering only income and allow us to study policy questions not focused solely on earnings.

2. Data and Summary Statistics

We combine data from three sources: College Board's SAT data, the National Student Clearinghouse (NSC), and TransUnion credit bureau data. We begin with the College Board's data on the nearly 300,000 SAT takers residing in Georgia and in the high school graduating cohorts of 2004-2008. We observe each student's full history of SAT scores. For these cohorts, the SAT had a math and verbal/critical reading section, each scored between 200 and 800 in increments of 10. The exam was offered six times per year, typically at high schools, and was most frequently taken in 11th and 12th grades. Students could retake the SAT as often as they liked. The SAT data include basic self-reported demographic information, including: sex, race/ethnicity, parental education and income, home zip code, and high school.

We merge the data on Georgia SAT takers to the National Student Clearinghouse, which tracks college enrollment and completion across the U.S. As of 2015, over 3,600 colleges and universities participate in the NSC, comprising over 98% of all students enrolled in American postsecondary institutions.[3] We use the NSC data to track college enrollment spells of SAT takers up to six years after high school graduation. We do not observe graduate school enrollment. We observe which college a student is enrolled in at any moment, as well as the timing and type of any degree completed. We supplement this with data from the Integrated Postsecondary Education Data System (IPEDS) on college type (two-year or four-year, public or private, for-profit or non-profit).

Credit Bureau Data and Estimated Household Income

[3] See Dynarski, Hemelt and Hyman (2015).

Financial outcome data come from TransUnion, one of three main credit bureaus that collect and generate financial metrics for most U.S. residents. The College Board acquired TransUnion data after a match on available student information. We merge a cross-section of these data from November 2017 to our SAT and NSC data, giving us a snapshot in time of students' financial well-being. The merging process yields a 97 percent match rate in Georgia. A potential empirical concern is that access to public four-year colleges might increase the probability of opening a credit card and thus having a credit report at all. Formal tests for of potentially endogenous matching show no statistically significant differences in match rates across the threshold, ruling out differences of greater than about one percentage point overall and for the primary subgroups of interest.[4] We take this as evidence that our identification strategy is not threatened by the matching process.

The outcome of greatest interest is estimated household income. To generate this estimate, the credit bureau merges its data with actual joint income data taken from a large sample of IRS 1040 tax forms. The credit bureau uses the hundreds of pieces of information it observes contemporaneously and historically (such as monthly credit card expenditures and payments, as well as historical debt balances and credit use) to estimate a consumer's joint gross adjusted income (line 37 of the 1040 federal tax form). This covers not only wage income that appears in state unemployment insurance databases but also income from any other sources, such as investment income and business income. Importantly for our purposes, the estimation does not account directly for college enrollment.

Two types of evidence suggest that the estimated household income generated by this credit bureau algorithm is strongly related to actual income. First, other researchers linking credit bureau data to external data sets with income measures have found that the credit bureau's estimates are good predictors of actual income. Blattner and Nelson (2021) link credit bureau data to mortgage application data with applicants' self-reported income. They find a correlation of 0.44 between the credit bureau's estimated household income and applicants' self-reported income, with similar distributions and a median difference between the two measures of $2,000.[5] Mello (2021) links

[4] See Table A1.
[5] See Appendix E of Blattner and Nelson (2021).

credit bureau data to payroll earnings data. He finds a correlation of 0.62 between estimated household income and payroll earnings.[6]

Second, we compare the TransUnion income estimator to median earnings from the College Scorecard, federal data containing the average earnings by college of all students who received federal financial aid. To do so, we construct college-specific incomes for different cohorts using the full College Board sample of approximately 17 million observations matched to the National Student Clearinghouse and TransUnion data. Across all colleges, we find a correlation of about 0.75 between median estimated household income from TransUnion and the College Scorecard's college-level median earnings for students 10 years after initial enrollment. That correlation is even higher (0.83) when we restrict the sample to colleges where at least 80 percent of students appear in the College Board data.

The TransUnion income estimator is generally higher than the Scorecard earnings, which makes sense because the former estimates household income from all sources and not just individual earnings. This is, however largely a level shift, as the line of best fit through these points is nearly parallel to the 45-degree line.[7] This pattern of a level difference in income measures but not a slope shift holds when we use the TransUnion subsample of students who attended high school in Georgia or when we look at only Georgia colleges, including separately by sector. Overall, we view this as compelling evidence that TransUnion's income estimate contains substantial information about individual earnings.

In addition to estimated household income, the credit bureau data allow us to observe debt, including student loans, non-student loans, and mortgages. We use principal components analysis to generate a standardized financial health index based on four components: credit scores;[8] whether any payments are delinquent; the amount of delinquent payments; and whether the individual has ever declared bankruptcy. We know students' state of residence in 2017, allowing us to measure out-of-state mobility. The credit bureau data does not allow us to observe marriage or family formation.

Analytical Sample and Summary Statistics

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[6] See Appendix F of Mello (2021).

[7] See Figure A1.

[8] The credit score, known by TransUnion as VantageScore 3.0, was developed jointly by TransUnion, Equifax, and Experian, and is used by many major U.S. lenders.

Our full analytical sample consists of all Georgia SAT takers in the high school graduating cohorts of 2004-2008 who took the SAT for the first time during their senior year of high school and who are matched to the credit bureau data. We focus on students who first took the SAT during their senior year to minimize endogenous retaking in reaction to low first scores. These students have relatively few opportunities to retake the exam so that their first SAT scores closely resemble the final scores sent to colleges. The public nature of the USGU thresholds means students who take the exam earlier in high school and miss the cutoffs have clear incentives and opportunities to retake the exam (Goodman, Hurwitz, and Smith, 2017). Nationally, relative to all SAT takers, senior year SAT takers have lower SAT scores, have lower parental income, and are more likely to be Black (Goodman, Gurantz and Smith, 2020). Throughout this paper, we refer to students who first take the SAT during senior year as "late takers."

The national pattern that late takers tend to be lower scoring and lower income than the average SAT taker holds true in Georgia as well. Table 1 shows mean characteristics for three sets of Georgia students: all SAT takers; all late SAT takers; and our primary regression discontinuity sample of late SAT takers, those whose first SAT scores fall within 60 points of the USG admissions threshold. Compared to the full sample of Georgia SAT takers, late takers: are 13 percentage points more likely to be Black, Hispanic, or Native American; are 12 percentage points more likely to come from low-income high schools (defined as those in the lowest tercile statewide based on students' self-reported income)[9]; have substantially lower first SAT scores; and have substantially lower estimated household income around age 30. The regression discontinuity sample of late takers looks nearly identical on average to the full sample of late takers, suggesting that those near the USG admissions threshold are typical of late SAT takers more broadly. In this regression discontinuity sample, 43 percent of students are under-represented minorities (nearly all of whom are Black) and 46 percent come from low-income high schools.

Average first SAT scores in this sample are about 900, which represents roughly the 30th percentile of the national score distribution during this time. Slightly more than half of students achieve the minimum scores needed for admission to USG. Despite the modest academic preparation of these students, the NSC data shows that 48 percent enrolled in a four-year college

[9] Because self-reported income is missing for roughly one-third of students in the College Board data, we categorize students by the average of non-missing self-reported income among their high school peers, which can be computed for all students. Low-income high schools are defined as those in the bottom tercile statewide, where students' average self-reported family income is below about $59,000.

within one year of high school graduation, and over 60 percent of those enrolled in one of the four-year USG universities. Another 29 percent enrolled in a two-year college. Only 26 percent of the sample finished a B.A. within six years and only nine percent completed an associate degree in that time.

Students are nearly 29 years old on average when we observe them in the credit bureau data in November 2017. Their average estimated household income at that point is about $62,000. They have total student loan balances of $21,000, of which nearly all are government loans. Nearly 11 percent of the sample missed student loan payments and nearly 20 percent have been delinquent on some loan payment within the past year. About three percent have ever declared bankruptcy. As a result, senior year SAT takers have a financial health index about 0.13 standard deviations below the mean of all of Georgia's SAT test-takers during this period. Over 80 percent still live in Georgia in 2017.

3. Methodology

To estimate the causal impact of access to and enrollment in public four-year institutions, we exploit the USGU admissions thresholds. In Georgia, a student must score at least 430 in verbal/critical reading and at least 400 in math to be eligible for admission to USGU.[10] A regression discontinuity design that compares outcomes of the nearly identical students just above and below these thresholds helps eliminate bias driven by students' non-random college choices. Because Georgia's admissions thresholds are publicly known, we define each student's distance from the threshold using that student's first SAT scores to avoid potential endogeneity driven by any retaking of the SAT upon failure to meet the thresholds.

We collapse the two-dimensional threshold into one dimension by defining distance from USGU access as:

$$Distance = \min(SAT_R - 430,\ SAT_M - 400) \qquad (1)$$

This running variable is negative when a student has missed at least one threshold and is zero or positive when a student has met or exceeded both thresholds. This method of collapsing a multi-

[10] Each university can (and some do) set thresholds above the minimums.

dimensional boundary into a single dimension is discussed in Reardon and Robinson (2012) and has been used in papers such as Cohodes and Goodman (2014) and Papay, Murnane, and Willet (2014). The resulting estimates are local average treatment effects averaged across students near either component of the admissions threshold.

We first show that the admissions thresholds induce variation in college choice, generating first stage estimates with local linear regressions (with a uniform kernel) of the form:

$$USGU_{isc} = \alpha_0 + \alpha_1 Access_i + \alpha_2 Distance_i + \alpha_3 Access_i * Distance_i + \lambda_s + \gamma_c + \mu_{isc} \quad (2)$$

For student *i* from high school *s* and cohort *c*, the outcome *USGU* indicates initial enrollment (within one year of high school graduation) in a Georgia public four-year university. *Distance* measures SAT points from the admissions threshold, as defined in equation 1. *Access* indicates whether the students' scores met or exceeded that threshold ($Distance \geq 0$). High school and cohort fixed effects (λ_s and γ_c) control for school- and cohort-specific factors, so that impacts are identified off of comparisons between students in the same school and cohort whose SAT scores were just above or below the threshold. Because students on either side of the threshold are nearly identical in academic skill and other characteristics, the coefficient of interest, α_1, estimates the causal effect of earning the minimum admissible SAT score on enrollment in Georgia's public four-year sector.

We then generate instrumental variable estimates of the impact of enrollment in the public four-year sector by fitting the model below:

$$Y_{isc} = \beta_0 + \beta_1 \widehat{USGU}_i + \beta_2 Distance_i + \beta_3 Access_i * Distance_i + \rho_s + \delta_c + \varepsilon_{isc} \quad (3)$$

where $\widehat{USGU}$ is instrumented by *Access* according to equation 2. This model implies we are estimating the impact of enrollment in the public four-year sector relative to forgone alternatives, including two-year colleges, non-USGU four-year colleges, and no college at all. The dependent variable *Y* includes various college completion and economic outcomes.

Our default specification uses a bandwidth of 60 SAT points, the optimal bandwidth for our primary outcomes according to the criteria described in Imbens and Kalyanaraman (2012). We then test the sensitivity of our estimates to the choice of both smaller and larger bandwidths. We compute heteroskedasticity robust standard errors as suggested by Kolesár and Rothe (2018) for the case of a discrete running variable.

For the regression discontinuity approach above to produce valid causal estimates, SAT takers should not be able to manipulate their scores with respect to the threshold. Three pieces of evidence suggest such manipulation is unlikely. First, the SAT is scored centrally and outside of Georgia, with scaled scores generated from more than 100 multiple choice questions by an algorithm unknown to students and teachers. Second, the density of the running variable (Figure A2) shows no evidence of heaping at the threshold itself, which would be expected if scores were being manipulated to just meet the admissions criteria. The only two values of the running variable for which the density in Georgia appears discontinuous (-40 and +10) also appear discontinuous in the rest of the country, suggesting they come from idiosyncratic features of the national raw-to-scaled score conversion algorithm and are not driven by Georgia-specific manipulation.

Third and finally, we see little evidence of differences in observable student characteristics across the threshold. Table 2 tests for such differences by estimating equation 2 but using student characteristics as the dependent variable. Across the full regression discontinuity sample, as well as sub-samples divided by high school income and race/ethnicity, we see nearly no economically or statistically significant discontinuities in student characteristics, including PSAT scores, race/ethnicity, and gender.[11] As a further test, we use these covariates to predict various outcomes within our regression discontinuity sample, testing for differences in predicted values of those outcomes.[12] Across our full sample and most sub-samples, we see no evidence of discontinuities in outcomes predicted by student characteristics, with only estimated household income and financial health showing small but statistically significant discontinuities in the low-income high school sub-sample. To measure the extent to which this might generate bias in our estimates, we run our regression discontinuity models both with and without such covariates.

4. Results

College Enrollment

[11] The PSAT is a College Board standardized exam taken by many students prior to the SAT.
[12] See Table A2.

Consistent with our earlier research, access to public four-year institutions substantially changes where students enroll. Figure 1 shows a clear discontinuity at the USGU admission threshold in students' probability of enrolling at an in-state public four-year university. Students missing the USGU thresholds on their first SAT attempt can enroll in USGU by meeting criteria through other means, including retaking the SAT or taking the ACT, explaining the USGU enrollment below the threshold. The discontinuity is fuzzy above the threshold because such students do not all: apply to USGU; gain admission upon applying; or enroll upon admission. The first column of Table 3 estimates the magnitude of this first stage discontinuity, showing that SAT-based admissibility increased the probability of enrolling in USGU by 5.1 percentage points. That result is highly statistically significant and precise enough to easily pass weak instrument tests with an F-statistic of 56. The magnitude of the overall first stage effect does not vary much by high school income and is somewhat larger for URM students, though still large and statistically significant for non-URM students.

Because the admissions thresholds apply system-wide, the first stage impact comes from a combination of increased enrollment probabilities across multiple Georgia institutions, not just a single institution as in some prior studies.[13] Two-thirds of the first stage comes from increased enrollment in the somewhat selective "Comprehensive Universities" (Georgia Southern University, Kennesaw State University, University of West Georgia, Valdosta State University), which tend to be relatively large and broad-access institutions geographically spread across the state. One-fourth of the first stage comes from increased enrollment in the more selective "Research Universities" (Augusta University, Georgia Institute of Technology, Georgia State University, University of Georgia), some of which are so selective to render admission thresholds irrelevant. We observe little effect of the thresholds on enrollment in the least selective "State Universities." Subgroups by high school income and URM status exhibit similar patterns.

The remaining columns of Table 3 show instrumental variable estimates of the change in college characteristics for the marginal student whose enrollment in USG colleges is affected by the threshold. For the marginal student, threshold-driven enrollment in USG increased four-year college enrollment by 69 percentage points, implying that most of these marginal students would not otherwise have enrolled in a four-year institution at all. Had they not been admissible, a little more than half would have otherwise enrolled in a two-year college. USG enrollment increased by

[13] See Table A3

23 percentage points the probability of attending any public college and by 34 percentage points the probability of attending college in-state.

Perhaps most strikingly, access to these in-state public four-year institutions substantially increased the quality of college attended, as measured by competitiveness and average income of prior college attendees. Enrollment in USG colleges increased by 90 percentage points the probability of attending a college ranked by Barron's as "competitive", suggesting that in the absence of access to the public four-year sector, nearly none of these students would have attended a competitive college. We also characterize colleges with data from Opportunity Insights' College Mobility Report Cards, which measure the average income in 2014 (around age 30) of all students who attended a given college in the early 2000s (Chetty et al., 2020). Conditional on enrolling in college and having such an income measure available, the marginal student's enrollment in USG colleges increased by over $4,000 the annual income earned by students at that college. This is a roughly 20 percent boost relative to the control complier mean college-level income of $23,000.

The treatment here can thus be thought of as inducing students to choose a moderately selective public four-year institution with better historical student outcomes relative to the counterfactual options, the most common of which are two-year colleges. This pattern is seen both in the full regression discontinuity sample and in each of the sub-samples split by high school income and student race/ethnicity. The remaining results focus on whether this change in college type and quality affects students' later outcomes, such as college completion, estimated household income, and finances.

College Completion

Access to the public four-year sector thus substantially increases educational attainment, consistent with our earlier findings in Goodman, Hurwitz, and Smith (2017). Enrollment in public four-year institutions, instead of these counterfactual options, substantially increased the probability of completing a B.A. Figure 2 shows the reduced form version of this, with a clear discontinuity at the USG admission threshold in students' probability of earning a B.A. within six years of graduating from high school. Table 4 shows instrumental variable estimates of B.A. and A.A. completion effects, along with sensitivity of B.A. effects to bandwidth choice. For the marginal student, enrolling in a USG college increased B.A. completion rates by 37 percentage points, more than tripling B.A. completion relative to the control complier mean completion rate of 14 percent. A.A. completion rates dropped only by (a statistically insignificant) 12 percentage points, suggesting that most of the increased degree completion comes from students who would not otherwise have earned any type of college degree. Narrowing or widening the bandwidth does not alter the conclusion that the enrollment shift induced by access to public four-year institutions at least tripled the B.A. completion rate of the marginal student. The magnitude of B.A. completion effects is also quite robust to inclusion of demographic and PSAT controls.[14]

We observe potential heterogeneity by income and race/ethnicity in the impact on college completion rates. Students from low-income high schools had increased B.A. completion rates of around 27 percentage points, a result that is statistically insignificant for our default bandwidth but similar in magnitude and statistically significant for slightly larger bandwidths. Students from middle- and high-income high schools had B.A. completion rates increase by 44 percentage points, though we cannot reject equality of the estimates across income groups and the point estimates are nearly identical for larger bandwidths. Non-URM students had larger B.A. completion rates across our default and larger bandwidths but also exhibited a larger decrease in A.A. completion, suggesting that access to the public four-year sector had a larger impact on the overall college degree completion rates for URM students than for non-URM students. As with the overall sample, none of these conclusions are affected by inclusion of demographic and PSAT controls.

Estimated Household Income

Access to and enrollment in the public four-year sector substantially increased estimated household income, particularly for students from low-income high schools. Figure 2 shows across the full sample a fairly clear discontinuity at the USG admission threshold in mean estimated household income. Tables 5 and 6 show, for multiple bandwidths and with and without controls, instrumental variable estimates of the impact of USG college enrollment on estimated household income and its logarithm. The second column of Table 5 suggests that, for the full sample, USG college enrollment increased estimated household income by 17 percent ($e^{0.157} - 1 = 0.169$). This coefficient generated using the default bandwidth with no controls (mirroring earlier tables) is statistically insignificant but using slightly larger bandwidths yields estimates that are similar in magnitude and statistically significant. Inclusion of demographic and PSAT controls only slightly

[14] See Table A4.

shrinks these estimates, leaving those from the larger bandwidths still marginally statistically significant. Across all of these specifications, the estimated effect ranges from 10-18 percent.

The top row of Table 6 tells a roughly similar story for estimated household income, which increased nearly $10,000 from a baseline among control compliers of about $50,000. That estimated effect is marginally significant only for bandwidth slightly larger than the default bandwidth of 60. Inclusion of additional controls again slightly shrinks these estimates, none of which are statistically significant. Across all of these specifications, the estimated effect ranges from $6,000 to $10,000, which represents 11-20 percent of the control compliers' mean estimated household income.

The observed effects on estimated household income are heavily concentrated among students from low-income high schools. As seen in panel B of Table 5, estimated impacts on such students are almost all around 27-28 percent ($e^{0.24} - 1 = 0.271$), with the default bandwidth yielding a particularly large estimate of 37 percent ($e^{0.314} - 1 = 0.368$). Inclusion of controls only slightly shrinks those point estimates and all but those using the narrowest bandwidth are statistically significant. Estimated impacts on students from middle- and high-income high schools are always positive but statistically insignificant and substantially smaller than those from low-income high schools.

Similar heterogeneity is observed in the levels of estimated household income in Table 6, where impacts on students from low-income high schools range from $12,000 to $23,000 and are again statistically significant across all specifications but those using the smallest bandwidth. These are relative to control complier mean incomes ranging from $39,000 to $48,000 depending on specification. Impacts on students from middle- and high-income high schools range from negative $5,000 to positive $4,000 and are never statistically significant.

Though the standard errors do not allow us to rule out meaningfully large impacts on the estimated household income of students from higher income high schools, it is intriguing that the large degree completion effects observed for such students do not appear to translate into income gains. One hypothesis is that the counterfactual education and labor market options available to students from higher income high schools are sufficiently good as to make public four-year college access less crucial. Control compliers from higher income high schools have much higher estimated household incomes than do their counterparts from low-income high schools ($57,000 versus $45,000).

Across most specifications, income effects for URMs are only somewhat larger in magnitude than non-URMs, suggesting that here socioeconomic status is a stronger predictor of the returns to college access than is race or ethnicity. In panel C of Table 5, URM students' returns to USG enrollment range from roughly 10-20 percent and are statistically significant or marginally so across all but the narrowest bandwidths. Effects for non-URM students are similar in magnitude or somewhat smaller, but never statistically significant in part because of a weaker first stage. Panel C of Table 6 shows similar impacts across URM and non-URM students, which are only marginally statistically significant for URM students across some specifications.

Financial Health and Residential Choice

The overall impact of enrollment in public four-year institutions on financial health is positive but imprecisely estimated. We measure financial health as an index based on credit score, payment delinquency and bankruptcy status. Visual evidence shows no obvious large discontinuity in financial health.[15] As seen in the first coefficient of Table 7, the estimated impact on financial health is about 0.1 standard deviations and is always positive.[16] This is roughly what we would expect from an increase of $9,000 in estimated household income, given the cross-sectional relationship between estimated household income and financial health. None of these financial health estimates is, however, statistically significant, given wide confidence intervals. Effects on student sub-groups are even more imprecise and point estimates do not particularly line up with impacts on estimated household income. Though not shown here, closer examination of the components of our financial health index shows little clear impact on the distribution of credit scores, on payment delinquency or on personal bankruptcy status.

We see suggestive evidence that access to the public four-year sector increased student loan balances around age 30. In the full sample, no clear discontinuity is visible in the graph of student loan balances by distance to the admissions threshold.[17] Point estimates imply USGU increases student loan balances by about $12,000, due to increased government-sponsored loans. That overall effect is, however, imprecise. URM students and those from lower income high schools see little increase in loan balances, perhaps because they are more frequently eligible for

[15] See Figure A3.
[16] See Table A5.
[17] See Figure A4.

grant aid. Student loan balances rise more for those from higher income high schools and for non-URM students, whose balances increase by about $27,000. Though the magnitude of the increase in student loan balances for non-URM students is somewhat sensitive to specification, that increase is at least marginally statistically significant across the majority of specifications.[18]

We have three hypotheses as to why the large, clear impact on estimated household income around age 30 does not translate into statistically clear impacts on financial health. First, though the overall positive coefficient is of the magnitude we would expect given the full sample's increase in estimated household income, the financial health impacts are sufficiently imprecisely estimated that we cannot rule out clear positive (or negative) effects, particularly for student sub-samples. Second, given that the marginal student here spent more time in college and thus had lower income earlier in their career, their overall financial health may not yet have improved at the age where observe them. Third, early in their careers, marginal students' increased student loan balances may offset some of the benefits of their increased income, again making early impacts on financial health less clear.

We see weak evidence that college access affected home ownership rates, as proxied by the presence of mortgage debt on credit reports. There is a somewhat identifiable visual discontinuity in the graph of the probability of having a mortgage[19] but the point estimates in Table 7 are never statistically significant, though they are positive and large compared to control complier means across all samples. The estimated impact of public four-year college enrollment on this proxy for wealth thus points in the positive direction but is fairly imprecise, much like the financial health index.

For the overall sample, there is little clear evidence of an impact on Georgia residency around age 30. No clear discontinuity is visible in the graphs of that outcome across all students.[20] As the rightmost columns of Table 7 show, we cannot reject the null of no effect on Georgia residency. Across a wider set of specifications, we do see some evidence that USG enrollment increased the rates of Georgia residency for URM students, by between 15 and 20 percentage points.[21] The null or relatively small residency results imply the marginal student is largely choosing between in-state two-year colleges and four-year institutions, not leaving the state for

[18] See Table A6.
[19] See Figure A5.
[20] See Figure A6.
[21] See Table A7.

education or work. This lack of out-migration increases the state's returns on investment to its subsidies of the public four-year sector.[22]

5. Returns on Investment

We compute both the private and public returns to the marginal student enrolling in an in-state public four-year institution, relative to the counterfactual mixture that consists largely of in-state public two-year colleges and to a lesser extent private or out-of-state four-year colleges and universities, as well as no college. To compute private returns, we compare students' increased future income streams to the increased costs they pay in tuition and other fees as a result of USGU enrollment. To compute public returns, we compare Georgia's increased income tax revenue from higher-earning residents to the state's increased expenditures on the subsidies required for each student enrolling in the public four-year sector. All calculations are in 2017 dollars and assume a discount rate of three percent. We again note that these returns for the marginal student implicitly hold total enrollment fixed but could differ if expanding enrollment created general equilibrium wage effects.

Computing both the private and public returns requires estimating the increase in income at every age for the marginal student, even though we only directly observe their income in 2017. To impute students' incomes in other years, we use the 2017 American Community Survey (ACS) to estimate quadratic income-age profiles in Georgia separately for people with bachelor's degrees, associate's degrees, some college, and no college. Using these estimated income-age profiles and each student's observed 2017 income, we assign a predicted income to every year prior to 2017 that students are not in college and every year after 2017. To compute after-tax income, we assume a federal tax rate based on the average tax rate by income quintiles plus Georgia's state income tax rate of 6 percent.[23] We discount each year's after-tax income relative to students' high school cohorts, then aggregate the discounted predicted net incomes over various time horizons to get the present discounted value (PDV) of net income for each student.

To complete the private return calculation, we compute the PDV of the tuition costs each student likely faced given their college enrollment choices. To do so, we use sticker price and

[22] See Tables A8 and A9 for estimated effects by gender on enrollment, attainment, and economic outcomes. We cannot rule out equality of effects by gender.
[23] We use the CBO's 2016 figures from https://www.cbo.gov/publication/55413.

average grant aid for each institution in each year as reported to IPEDS. We assume students pay private colleges the sticker price less average grant aid, out-of-state public colleges the sticker price minus average federal aid, and in-state public colleges our best approximation of the average tuition for in-state students. The NSC data allow us to construct each student's complete history of college enrollment, which we combine with these estimated costs to compute for each student a PDV of their college tuition costs. The difference between the PDVs of each student's after-tax income stream and tuition cost stream yields the net present value (NPV) of that student's college choice.

The private return to public four-year sector enrollment becomes positive and large early in students' careers. To show this, we generate fuzzy RD estimates using each student's NPV at 10, 20 and 30 years as our model's outcome. For the marginal student, enrollment in a public four-year institution is a break-even proposition after 10 years but has an NPV of about $82,000 after 20 years and over $132,000 after 30 years.[24] The substantial increase in income due in part to increased B.A. completion rates thus rapidly outweighs both increased tuition costs relative to cheaper counterfactual college options and delayed earnings due to increased time spent enrolled. For the marginal student, enrollment in the public four-year sector thus pays off fairly rapidly. The private return turns positive more quickly and is much larger for students from low-income high schools because of their larger increases in estimated household income. After 30 years, the NPV for the marginal student from a low-income high school's is over $300,000.

We use a similar approach to computing the public return to Georgia of the marginal student's enrollment in the USGU. We use the imputed income-age profiles and the state's six percent income tax rate to compute for each student their PDV of state income tax payments. Students who migrate out of state do not pay Georgia income taxes. We estimate the state's expenditures on each student's college education by assigning the average per capita state expenditure at each in-state public college in years when the student is enrolled at such a college, and zero state expenditures when they are enrolled at private, out-of-state or no college.[25] These expenditures are discounted to each student's high school graduation year and then added to a

[24] These estimates use our default specification with a bandwidth of 60 and excluding PSAT and demographic controls. Changing the bandwidth or adding controls does not meaningfully change the results.
[25] College-specific state expenditure data come from the Delta Cost Project.

PDV of state expenditures. The state's NPV is the difference between the PDV of income tax payments and expenditures on college subsidies.

For the marginal enrollee in a public four-year institution, the state appears to break even after 10 to 20 years and its NPV turns slightly positive in the longer run. We show this by generating fuzzy RD estimates using the state's NPV at 10, 20 and 30 years as our model's outcome. We observe little impact on the state's NPV after 10 years but the marginal student's enrollment in a public four-year institution increases the state's NPV by over $2,000 after 30 years. For students from low-income high schools, the state's NPV turns positive within 10 years and jumps to $26,000 after 30 years. The large increase in income tax revenue generated by additional enrollment thus completely offsets the cost to the state of subsidizing one additional student at a four-year campus. This likely represents a lower bound on the state's budgetary impacts of expanding college access given that our calculations ignore spillovers to the productivity of co-workers (Moretti, 2004) and potentially reduced state expenditures on health care given the impact of college education on the health of students and their children (Buckles et al., 2016; Currie and Moretti, 2003).

In the language of Hendren and Sprung-Keyser (2020), the marginal value of public funds (MVPF) in this context is infinite because the state's investment pays for itself. Similar estimates from Florida and Texas also suggest infinite MVPFs of such investments. Hendren and Sprung-Keyser (2020) calculate the MVPF using Zimmerman's (2014) estimates in Florida, while Mountjoy (2024) shows an infinite MVPF for marginal enrollees in Texas' public colleges. Our paper, along with these other ones, helps establish something close a consensus that the returns to college access for students on the admissions margin are very high.

Finally, we show that federal tax revenues substantially increase when these marginal students begin at the USGU. The marginal increase in PDV of federal tax revenue is approximately $11,000, $29,000, and $46,000, after 10, 20, and 30 years respectively. For marginal students from low-income high schools, the increase in PDV of federal tax revenue is approximately $42,000, $84,000, and $119,000, after 10, 20, and 30 years. These magnitudes suggest that federal policies encouraging enrollment in USGU-like colleges over the typical alternatives may pay for themselves, especially for students from low-income high schools whose income appears to benefit the most from such enrollment.

6. Conclusion

This paper presents some of the first clear evidence that access to entire public systems of four-year colleges substantially improves students' incomes, particularly for those from low-income high schools. For many students, state subsidies of in-state public four-year institutions mean that such institutions are the only relatively low-cost options that also have reasonable degree completion rates. We show that enrollment in such universities increases students' B.A. completion rates and raises their incomes around age 30 by about 17 percent on average. Students from low-income high schools see increases roughly twice as large from such enrollment. We see little clear positive or negative impact on other measures of economic well-being, including student loan balances, financial health, mortgage status and residential location. We estimate that the marginal student sees a positive return on investment to enrollment in a public four-year institution in the relatively short run.

Our estimates also suggest that expanding access to the public four-year sector might be revenue-neutral or even revenue-enhancing to states in the long run. Many states explicitly ration access to the public four-year sector through required academic qualifications such as minimum SAT scores and GPAs. Others implicitly ration such access through processes that rely on the judgment of individual colleges' admissions officers. Our estimates suggest that, though such rationing is understandable given short-run budget constraints, allocating state tax dollars to increase the number of college enrollees might improve states' budget outlooks in the long run. Increasing access to public four-year institutions of higher education likely has positive social returns.

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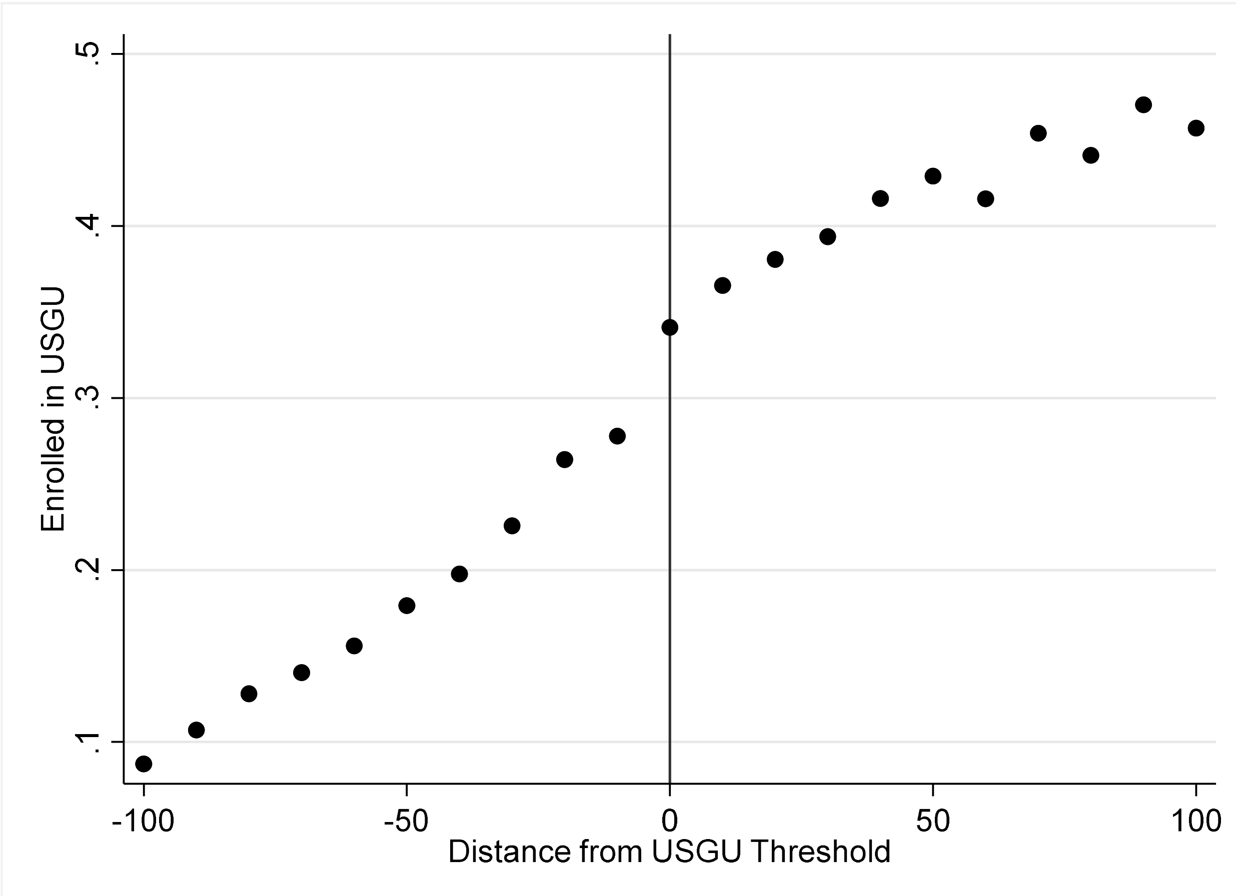
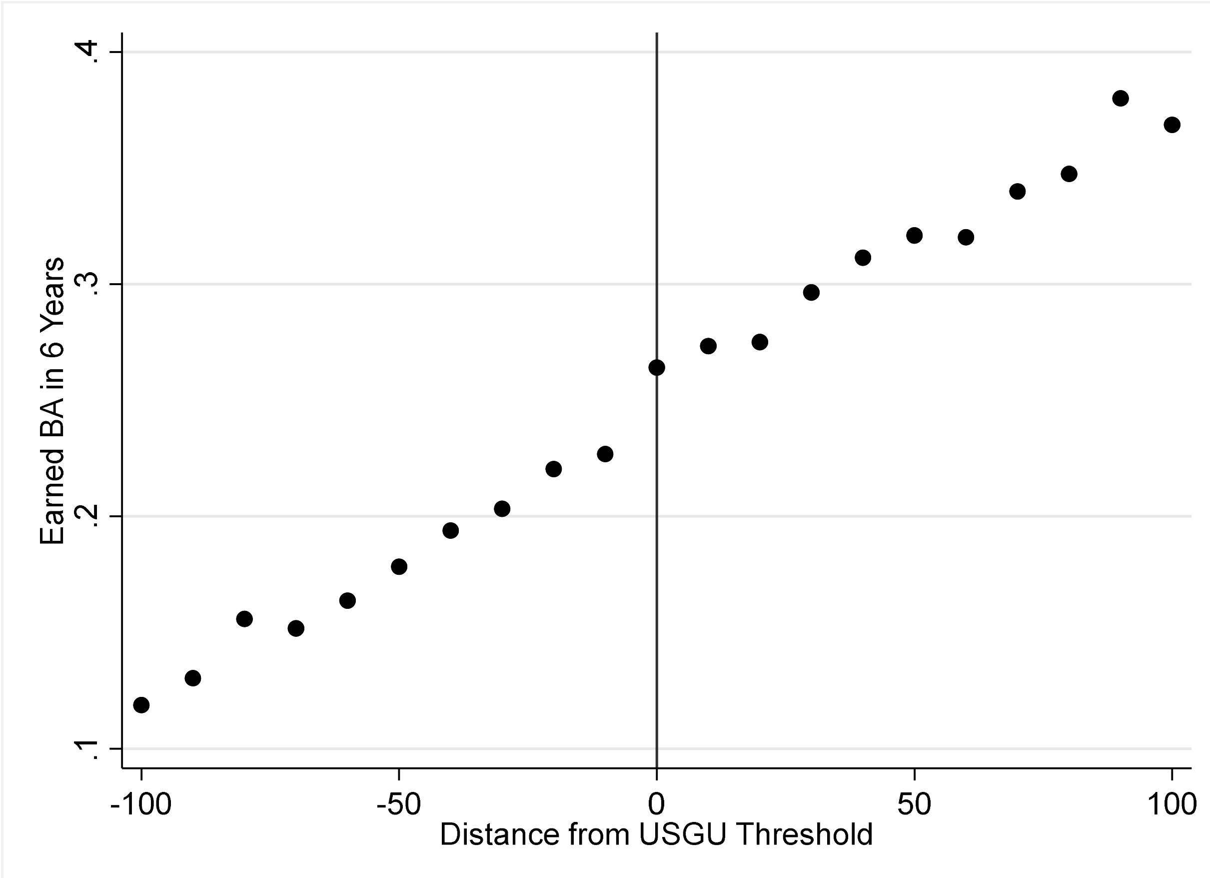
Figure 1 – Public Four-Year College Access and College Enrollment



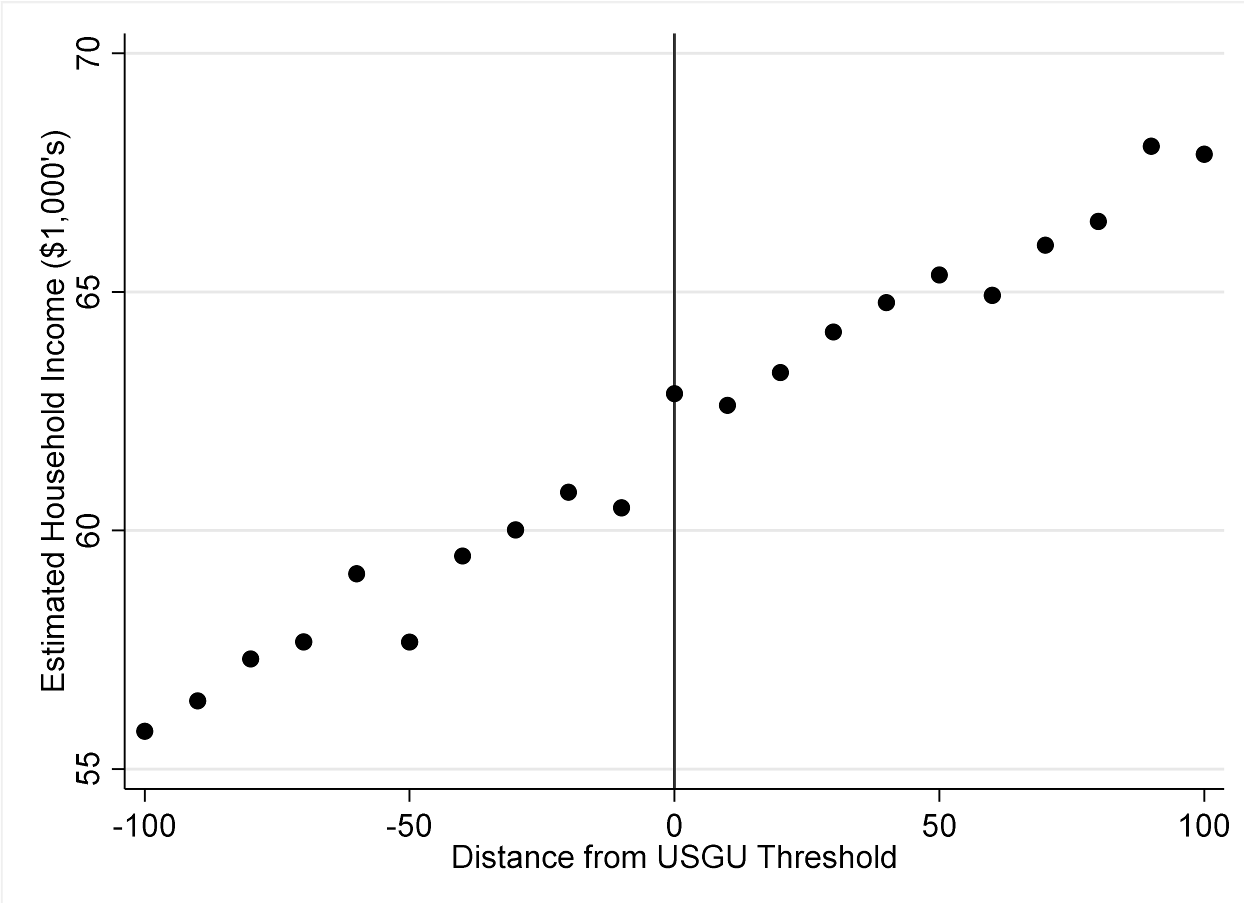
Notes: Shown above is the fraction of students attending an in-state public four-year college, by distance from the University System of Georgia's admissions threshold. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Figure 2 – Public Four-Year College Access and College Completion



Notes: Shown above is the fraction of students earning a B.A. from any college within six years of high school graduation, by distance from the University System of Georgia's admissions threshold. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Figure 3 – Public Four-Year College Access and Estimated Household Income



Notes: Shown above is students' average estimated household income around age 30, by distance from the University System of Georgia's admissions threshold. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Table 1 - Summary Statistics

	(1) All SAT takers	(2) Late SAT takers	(3) RD sample
(A) Demographics			
Female	0.55	0.55	0.57
White	0.61	0.49	0.49
Black	0.27	0.38	0.38
URM (Black/Hispanic/Native American)	0.30	0.43	0.43
Low income high school	0.34	0.46	0.46
(B) College enrollment and completion			
First SAT score	968	901	890
Met or exceeded USG minimum	0.66	0.54	0.55
Enrolled in USG	0.40	0.31	0.32
Enrolled in 4-year college	0.63	0.48	0.48
Enrolled in 2-year college	0.21	0.28	0.29
Earned B.A. within 6 years	0.44	0.27	0.26
Earned A.A. within 6 years	0.06	0.08	0.09
(C) Income			
Age (as of November 2017)	29.0	28.9	28.9
Estimated household income ($1000's)	71.6	62.2	62.0
(D) Student loans			
Outstanding student loans ($1000's)	22.9	21.3	21.4
Government student Loans ($1000's)	20.6	19.4	19.5
Any student loans past due last year	0.08	0.11	0.11
(E) Other financial outcomes			
Financial health index	0.00	-0.13	-0.13
Credit score above 700	0.46	0.32	0.31
Delinquent on any payments last year	0.16	0.20	0.20
Total past due in last year ($1000's)	0.12	0.16	0.16
Ever bankrupt	0.02	0.03	0.03
Any mortgage	0.23	0.18	0.19
Lives in Georgia	0.76	0.80	0.81
N	275,870	120,373	66,356

Notes: The full sample includes all Georgia students from the graduating high school cohorts of 2004-2008 and who were matched to credit bureau data. Late SAT takers limit that sample to those who first took the SAT in senior year. The regression discontinuity further limits that sample to those whose first SAT scores are within 60 points of the USG admissions threshold.

Table 2 - Covariate Balance

	Took PSAT (1)	PSAT Verbal (2)	PSAT Math (3)	Black (4)	Hispanic (5)	Asian (6)	Other race (7)	Female (8)
(A) Full RD sample								
USG access	-0.001	0.128	0.084	-0.007	0.002	0.003	0.002	0.007
	(0.005)	(0.227)	(0.229)	(0.006)	(0.003)	(0.003)	(0.003)	(0.008)
Control mean	0.84	33.57	34.05	0.42	0.04	0.03	0.05	0.58
N	66,356	66,356	66,356	66,356	66,356	66,356	66,356	66,356
(B) By high school income								
Low income	-0.009	-0.213	-0.147	-0.014	0.002	0.004	-0.003	-0.005
	(0.008)	(0.335)	(0.320)	(0.009)	(0.005)	(0.003)	(0.005)	(0.010)
Control mean	0.84	33.30	33.57	0.57	0.04	0.03	0.05	0.59
N	30,629	30,629	30,629	30,629	30,629	30,629	30,629	30,629
Middle/high income	0.007	0.412	0.297	-0.001	0.002	0.002	0.005	0.017
	(0.008)	(0.324)	(0.338)	(0.008)	(0.005)	(0.004)	(0.005)	(0.012)
Control mean	0.84	33.82	34.50	0.27	0.05	0.04	0.04	0.56
N	35,727	35,727	35,727	35,727	35,727	35,727	35,727	35,727
(C) By student race/ethnicity								
URM	-0.009	-0.223	-0.109	-0.010	0.008	0.000	0.002	0.005
	(0.007)	(0.307)	(0.309)	(0.007)	(0.007)	(0.000)	(0.002)	(0.011)
Control mean	0.88	34.73	34.89	0.90	0.09	0.00	0.01	0.60
N	28,569	28,569	28,569	28,569	28,569	28,569	28,569	28,569
Non-URM	0.009	0.561*	0.418	0.000	0.000	0.003	0.003	0.010
	(0.008)	(0.335)	(0.340)	(0.000)	(0.000)	(0.005)	(0.006)	(0.011)
Control mean	0.81	32.57	33.32	0.00	0.00	0.06	0.08	0.56
N	37,787	37,787	37,787	37,787	37,787	37,787	37,787	37,787

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is a reduced form estimate of being above the threshold on the listed covariate. All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. The sample includes all Georgia students from the graduating high school cohorts of 2004-2008 who first took the SAT in senior year and who were matched to credit bureau data. Control means are for observations 10 SAT points below the threshold.

Table 3 - Four-Year Public College Access and College Enrollment

	First stage	Instrumental variable estimate					
College type	USG	4-year	2-year	Public	In-state	Competitive	OI Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(A) All students							
USG access / enrollment	0.051***	0.685***	-0.523***	0.233*	0.339**	0.896***	4.369**
	(0.007)	(0.090)	(0.137)	(0.125)	(0.132)	(0.108)	(1.711)
Control mean / CCM	0.288	0.315	0.523	0.767	0.661	0.138	22.938
N	66,356	66,356	66,356	66,356	66,356	66,356	53,442
(B) By high school income							
Low income	0.054***	0.554***	-0.475**	0.272	0.370**	0.972***	4.021*
	(0.009)	(0.143)	(0.189)	(0.182)	(0.169)	(0.152)	(2.335)
Control mean / CCM	0.306	0.446	0.475	0.728	0.630	0.070	22.241
N	30,629	30,629	30,629	30,629	30,629	30,629	25,270
Middle/high income	0.052***	0.808***	-0.564***	0.199	0.320*	0.805***	4.742**
	(0.010)	(0.121)	(0.191)	(0.168)	(0.193)	(0.145)	(2.371)
Control mean / CCM	0.270	0.192	0.564	0.801	0.680	0.200	23.288
N	35,727	35,727	35,727	35,727	35,727	35,727	28,172
(C) By student race/ethnicity							
URM	0.070***	0.592***	-0.461***	0.254**	0.453***	0.802***	3.301*
	(0.011)	(0.112)	(0.125)	(0.125)	(0.125)	(0.132)	(1.842)
Control mean / CCM	0.361	0.408	0.461	0.746	0.547	0.216	23.234
N	28,569	28,569	28,569	28,569	28,569	28,569	24,831
Non-URM	0.046***	0.808***	-0.638***	0.151	0.159	0.968***	5.543*
	(0.009)	(0.130)	(0.221)	(0.189)	(0.213)	(0.134)	(2.929)
Control mean / CCM	0.226	0.192	0.638	0.849	0.841	-0.010	22.279
N	37,787	37,787	37,787	37,789	37,788	37,791	28,611

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Column 1 shows first stage estimates of being above the threshold on enrollment in a USG college (with mean USG enrollment just below the threshold listed at bottom). Columns 2-7 show instrumental variable estimates of the impact of USG college enrollment on the listed outcome (with control complier outcome means listed at bottom). All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. Panel A includes all late SAT takers, panel B splits students by their enrollment in a high school in the lowest tercile of statewide income, and panel C splits students by underpresented minority status. College enrollment is defined within one year of high school graduation. Household income is measured in November 2017, when respondents are about 30 years old. The outcome in column 6 is an indicator for a college categorized as at all competitive by Barron's. The outcome in column 7 is mean college-level income as computed by Opportunity Insights.

Table 4 - Four-Year Public College Access and Degree Completion

College degree	B.A.	A.A.	B.A.			
Bandwidth	60	60	40	60	80	100
	(1)	(2)	(3)	(4)	(5)	(6)
(A) All students						
USG access / enrollment	0.372***	-0.118	0.527**	0.372***	0.369***	0.291***
	(0.136)	(0.082)	(0.213)	(0.136)	(0.099)	(0.082)
CCM	0.141	0.145	0.072	0.141	0.142	0.158
N	66,356	66,356	48,145	66,356	80,480	92,409
(B) By high school income						
Low income	0.270	-0.178	0.459*	0.270	0.280**	0.261**
	(0.185)	(0.111)	(0.270)	(0.185)	(0.129)	(0.107)
CCM	0.291	0.156	0.238	0.291	0.228	0.215
N	30,629	30,629	22,119	30,629	37,213	42,690
Middle/high income	0.438**	-0.073	0.557*	0.438**	0.428***	0.272**
	(0.193)	(0.114)	(0.330)	(0.193)	(0.143)	(0.113)
CCM	0.015	0.138	-0.069	0.015	0.071	0.144
N	35,727	35,727	26,026	35,727	43,267	49,719
(C) By student race/ethnicity						
URM	0.317**	-0.000	0.634***	0.317**	0.280**	0.189**
	(0.149)	(0.073)	(0.228)	(0.149)	(0.112)	(0.089)
CCM	0.156	0.018	0.010	0.156	0.192	0.240
N	28,569	28,569	20,621	28,569	34,738	39,807
Non-URM	0.460**	-0.280*	0.256	0.460**	0.464***	0.394***
	(0.206)	(0.147)	(0.331)	(0.206)	(0.155)	(0.119)
CCM	0.102	0.312	0.200	0.102	0.071	0.065
N	37,787	37,787	27,524	37,787	45,742	52,602

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All columns show instrumental variable estimates of the impact of USG college enrollment on degree completion within six years of high school graduation (with control complier outcome means listed at bottom). The local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects. Panel A includes all late SAT takers, panel B splits students by their enrollment in a high school in the lowest tercile of statewide income, and panel C splits students by underpresented minority status.

Table 5 - Four-Year Public College Access and Log(Estimated Household Income)

	No controls				With controls			
Bandwidth	40	**60**	80	100	40	60	80	100
	(1)	**(2)**	(3)	(4)	(5)	(6)	(7)	(8)
(A) All students								
USG enrollment	0.120	0.157	0.167**	0.136**	0.098	0.145	0.146*	0.114*
	(0.155)	(0.104)	(0.081)	(0.066)	(0.158)	(0.104)	(0.081)	(0.065)
CCM	10.818	10.798	10.798	10.844	10.828	10.803	10.811	10.858
N	48,145	66,356	80,480	92,409	48,145	66,356	80,480	92,409
(B) By high school income								
Low income	0.237	0.314**	0.242**	0.246***	0.212	0.276**	0.206**	0.213**
	(0.187)	(0.142)	(0.102)	(0.092)	(0.184)	(0.133)	(0.097)	(0.086)
CCM	10.706	10.640	10.703	10.709	10.720	10.668	10.729	10.733
N	22,119	30,629	37,213	42,690	22,119	30,629	37,213	42,690
Middle/high income	0.030	0.025	0.096	0.035	0.013	0.030	0.089	0.025
	(0.247)	(0.145)	(0.119)	(0.085)	(0.251)	(0.152)	(0.123)	(0.088)
CCM	10.899	10.923	10.876	10.942	10.907	10.916	10.878	10.947
N	26,026	35,727	43,267	49,719	26,026	35,727	43,267	49,719
(C) By student race/ethnicity								
URM	0.220	0.193**	0.166**	0.108*	0.226	0.192**	0.159**	0.104*
	(0.145)	(0.093)	(0.074)	(0.057)	(0.156)	(0.095)	(0.075)	(0.057)
CCM	10.724	10.681	10.700	10.750	10.716	10.678	10.704	10.751
N	20,621	28,569	34,738	39,807	20,621	28,569	34,738	39,807
Non-URM	-0.046	0.083	0.128	0.104	-0.038	0.083	0.128	0.110
	(0.269)	(0.170)	(0.127)	(0.097)	(0.277)	(0.178)	(0.132)	(0.100)
CCM	10.942	10.943	10.907	10.952	10.931	10.941	10.904	10.946
N	27,524	37,787	45,742	52,602	27,524	37,787	45,742	52,602

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variable estimate of the impact of USG college enrollment on the logarithm of estimated household income. All local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects. Column 2 is our preferred specification. Columns 5-8 adds controls for PSAT scores and indicators for sex, race, and PSAT-taking status.

Table 6 - Four-Year Public College Access and Estimated Household Income ($1000's)

	Without controls				With controls			
Bandwidth	40	**60**	80	100	40	60	80	100
	(1)	**(2)**	(3)	(4)	(2)	(4)	(6)	(8)
(A) All students								
USG enrollment	7.364	9.984	9.982*	8.042*	5.711	9.128	8.479	6.456
	(11.341)	(7.815)	(5.848)	(4.750)	(11.515)	(7.847)	(5.866)	(4.667)
CCM	51.449	50.398	51.390	54.501	52.139	50.794	52.278	55.500
N	48,145	66,356	80,480	92,409	48,145	66,356	80,480	92,409
(B) By high school income								
Low income	19.822	23.299**	15.723**	14.730**	18.170	20.675**	13.338**	12.474**
	(12.601)	(10.239)	(7.048)	(6.340)	(12.195)	(9.421)	(6.666)	(5.813)
CCM	40.752	38.570	44.811	46.417	41.639	40.426	46.482	48.022
N	22,119	30,629	37,213	42,690	22,119	30,629	37,213	42,690
Middle/high income	-3.509	-1.646	4.350	1.907	-4.933	-1.408	3.742	1.170
	(18.946)	(11.278)	(8.867)	(6.410)	(19.315)	(11.865)	(9.252)	(6.612)
CCM	60.517	60.271	56.916	60.202	61.090	59.824	57.084	60.588
N	26,026	35,727	43,267	49,719	26,026	35,727	43,267	49,719
(C) By student race/ethnicity								
URM	12.912	9.765*	7.607*	3.930	13.306	9.665*	7.038	3.622
	(9.049)	(5.720)	(4.531)	(3.596)	(9.688)	(5.846)	(4.585)	(3.620)
CCM	47.299	45.272	46.346	49.314	46.854	45.189	46.633	49.462
N	20,621	28,569	34,738	39,807	20,621	28,569	34,738	39,807
Non-URM	-3.507	7.333	9.342	7.866	-2.923	7.401	9.317	8.245
	(20.577)	(13.617)	(9.875)	(7.309)	(21.202)	(14.244)	(10.249)	(7.585)
CCM	57.457	57.035	56.854	60.166	56.598	56.705	56.663	59.825
N	27,524	37,787	45,742	52,602	27,524	37,787	45,742	52,602

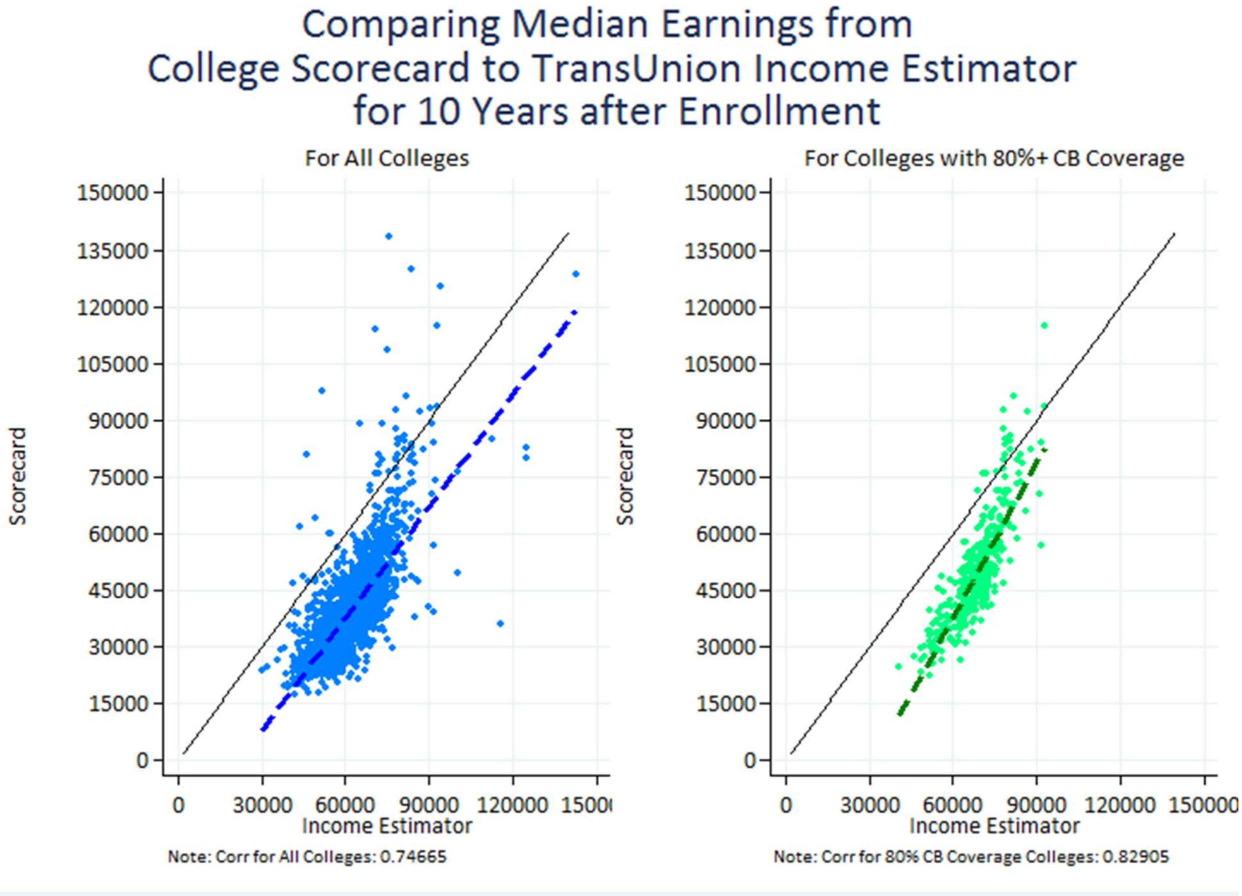
Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variable estimate of the impact of USG college enrollment on estimated household income. All local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects. Column 2 is our preferred specification. Columns 5-8 adds controls for PSAT scores and indicators for sex, race, and PSAT-taking status.

Table 7 - Four-Year Public College Enrollment and Financial and Residential Outcomes

	Financial health index (1)	Total student loan balance ($1000's) (2)	Government student loan balance ($1000's) (3)	Any mortgage (4)	Still lives in Georgia (5)
(A) All students					
USG enrollment	0.100	11.913	11.801	0.060	0.003
	(0.365)	(11.254)	(10.444)	(0.112)	(0.118)
CCM	-0.144	13.718	10.965	0.127	0.845
N	65,995	66,356	66,356	66,356	66,356
(B) By high school income					
Low income	-0.276	5.977	4.080	0.082	0.104
	(0.512)	(15.765)	(15.217)	(0.128)	(0.144)
CCM	0.019	27.519	26.343	0.065	0.766
N	30,464	30,629	30,629	30,629	30,629
Middle/high income	0.473	16.634	17.995	0.043	-0.077
	(0.476)	(15.075)	(13.554)	(0.172)	(0.179)
CCM	-0.350	3.007	-1.340	0.168	0.913
N	35,531	35,727	35,727	35,727	35,727
(C) By student race/ethnicity					
URM	-0.131	3.897	5.617	0.071	0.170
	(0.407)	(15.249)	(14.216)	(0.093)	(0.122)
CCM	-0.262	34.305	29.612	0.035	0.773
N	28,444	28,569	28,569	28,569	28,569
Non-URM	0.418	27.122**	26.317**	0.002	-0.129
	(0.562)	(13.265)	(12.436)	(0.209)	(0.198)
CCM	-0.140	-9.616	-11.385	0.250	0.905
N	37,551	37,787	37,787	37,787	37,787

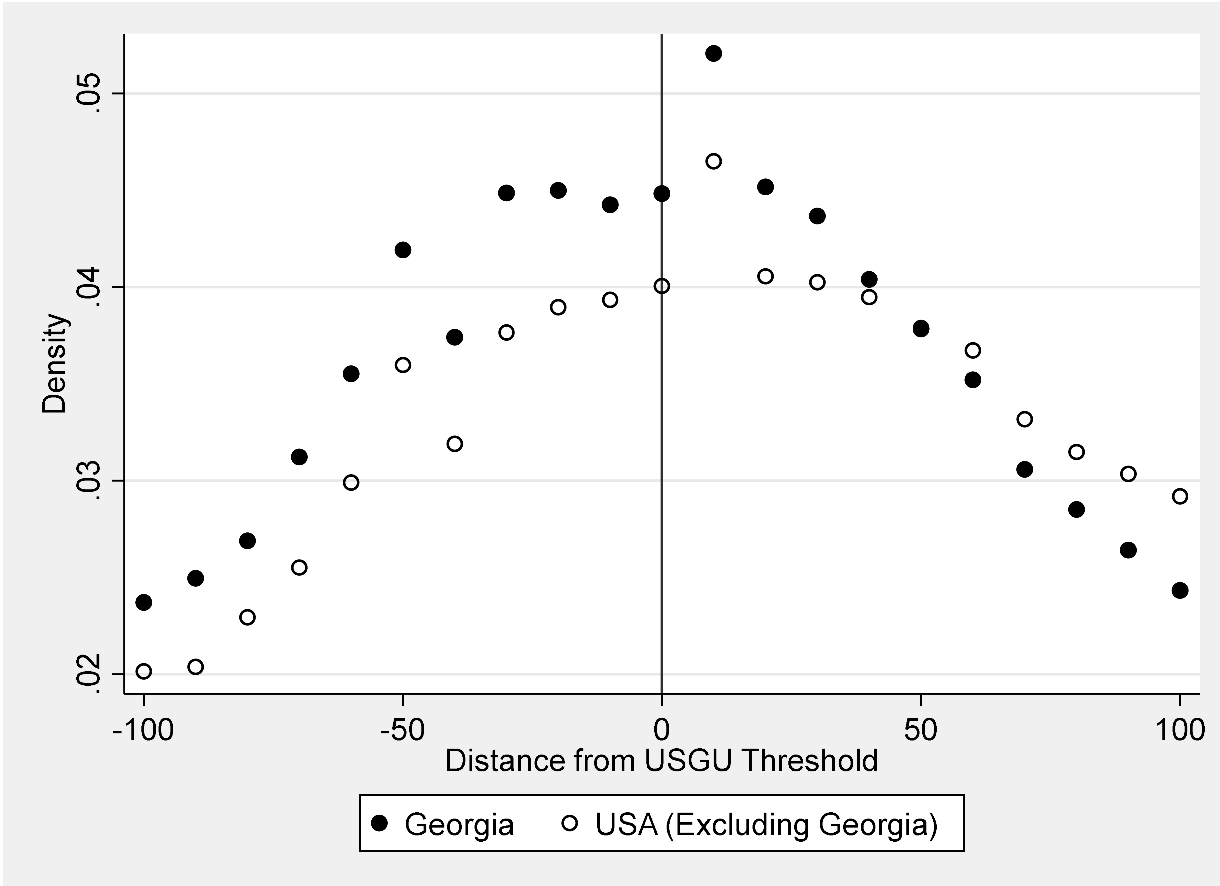
Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variables estimate of the impact of USG college enrollment on the listed outcome (with control complier outcome means listed at bottom). All local linear regression discontinuity models use a bandwidth of 60 SAT points and control for high school and cohort fixed effects. Panel A includes all late SAT takers, panel B splits students by their enrollment in a high school in the lowest tercile of statewide income, and panel C splits students by underpresented minority status. Outcomes are measured in November 2017, when respondents are about 30 years old. The financial health index is the first principal component of standardized versions of an individual's credit score, past year payment delinquency status, past year amount past due, and an indicator for ever having declared bankruptcy.

Figure A1 – College Scorecard Earning vs. TransUnion Income Estimator



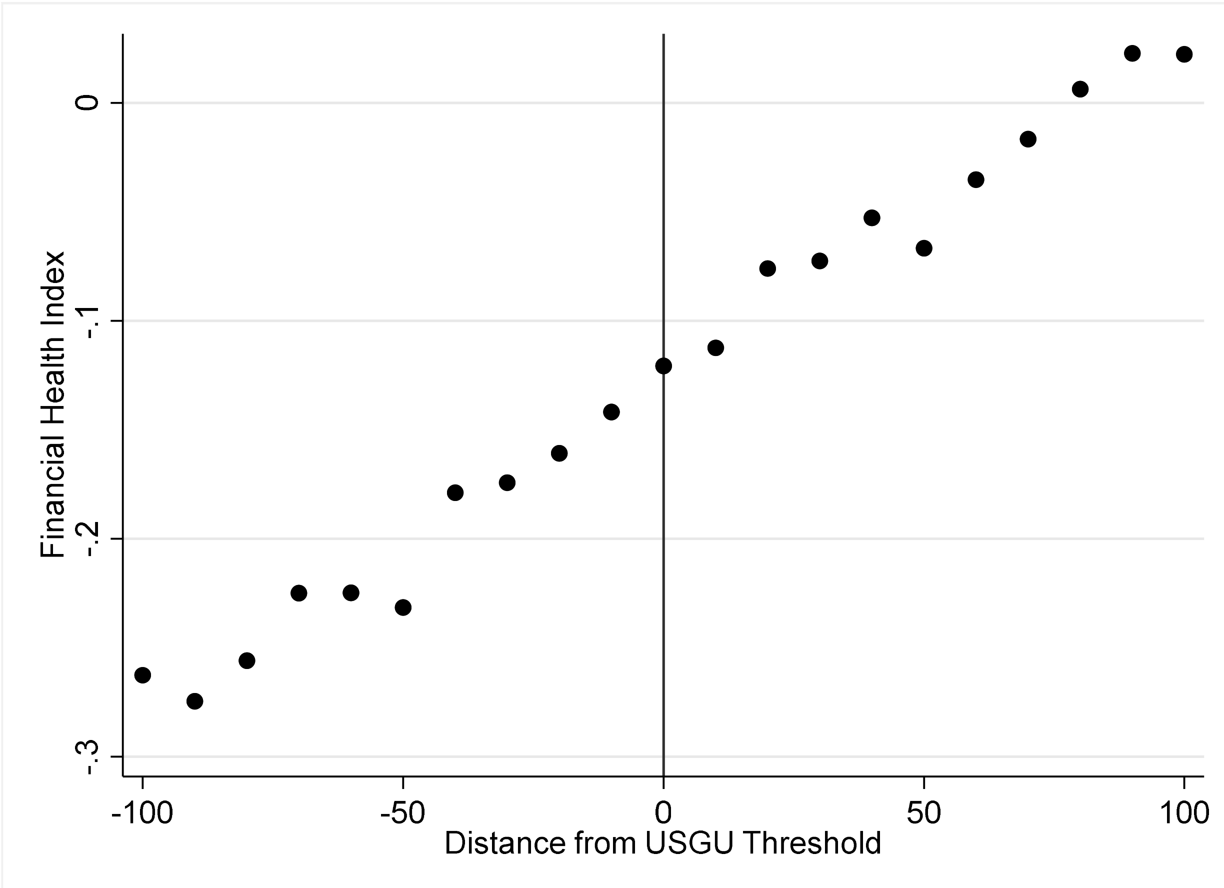
Notes: Scorecard data are from the College Scorecard aggregated data at the college-year level, which is freely available online. CreditVision Income Estimator comes from TransUnion credit bureau. It is merged to individual-level data and aggregated to the college-year level.

Figure A2 – Density of Running Variable



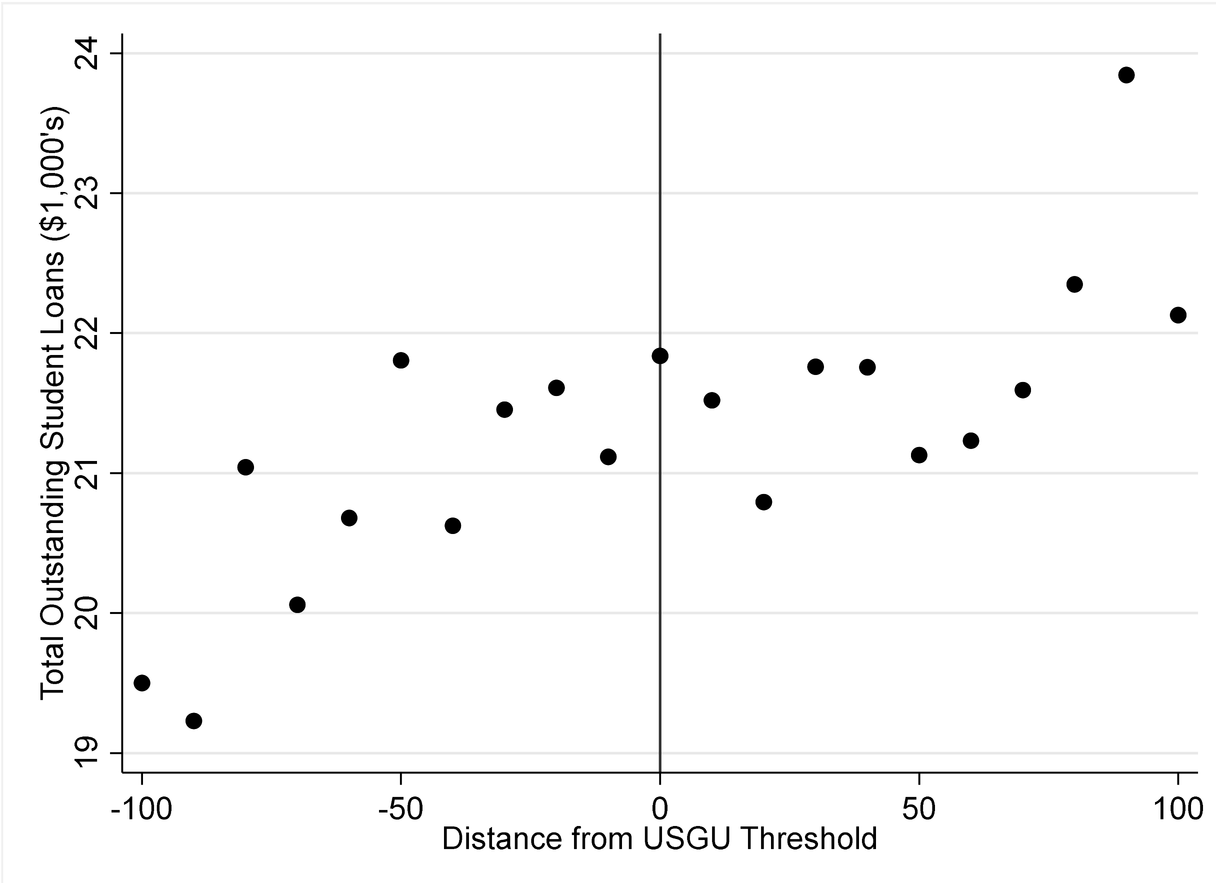
Notes: Sample includes all Georgia high school graduates between 2004 and 2008 who took the SAT for the first time in their senior year and matched to financial data. University System of Georgia's (USG) university admission threshold is 400 math and 430 verbal and the distance is the minimum between a student's scores and the thresholds for each section. The small jump in density at a distance of 10 results from lumpiness in the underlying SAT scores, as explained in further detail in Goodman, Hurwitz and Smith (2017).

Figure A3 – Financial Health Index



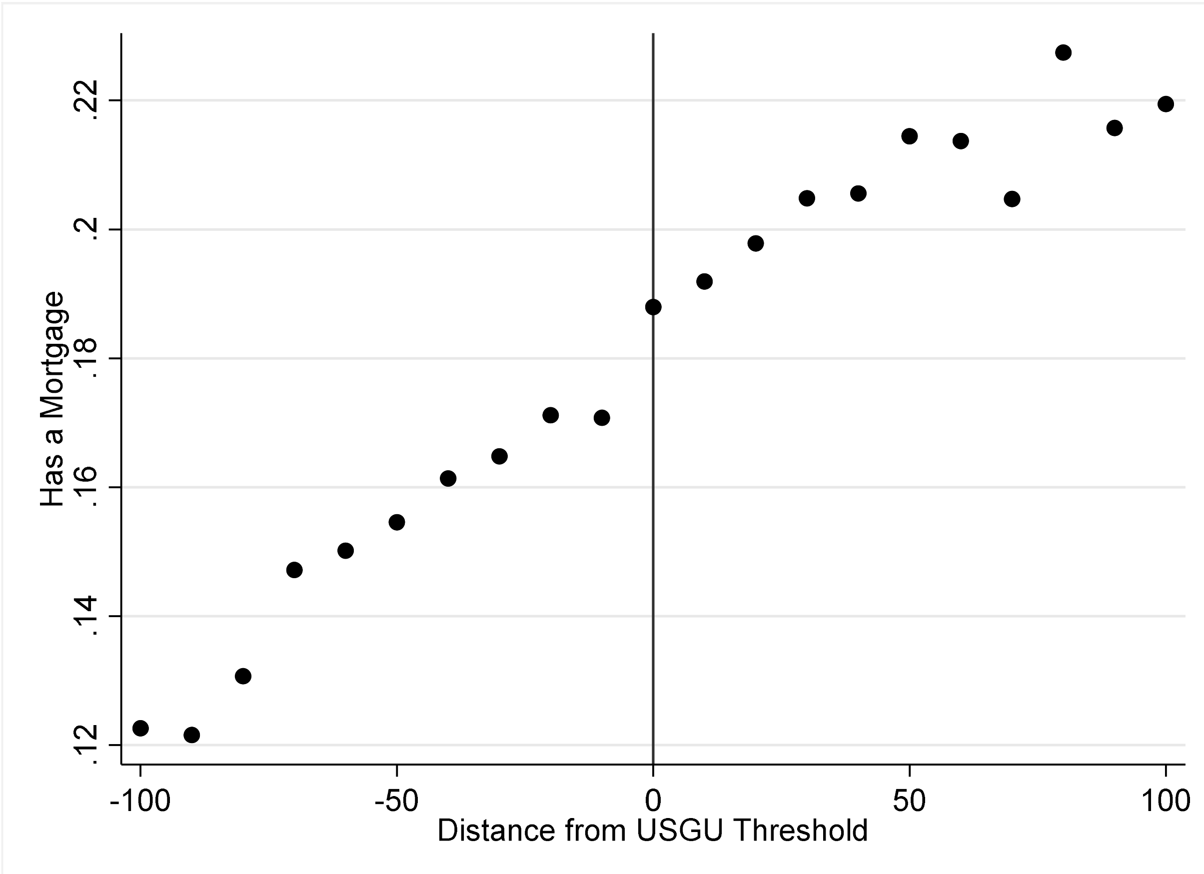
Notes: Shown above is the average financial health of students, by distance from the University System of Georgia's admissions threshold. The financial health index is the first principal component of standardized versions of an individual's credit score, past year payment delinquency status, past year amount past due, and an indicator for ever having declared bankruptcy. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Figure A4 – Total Student Loan Balances



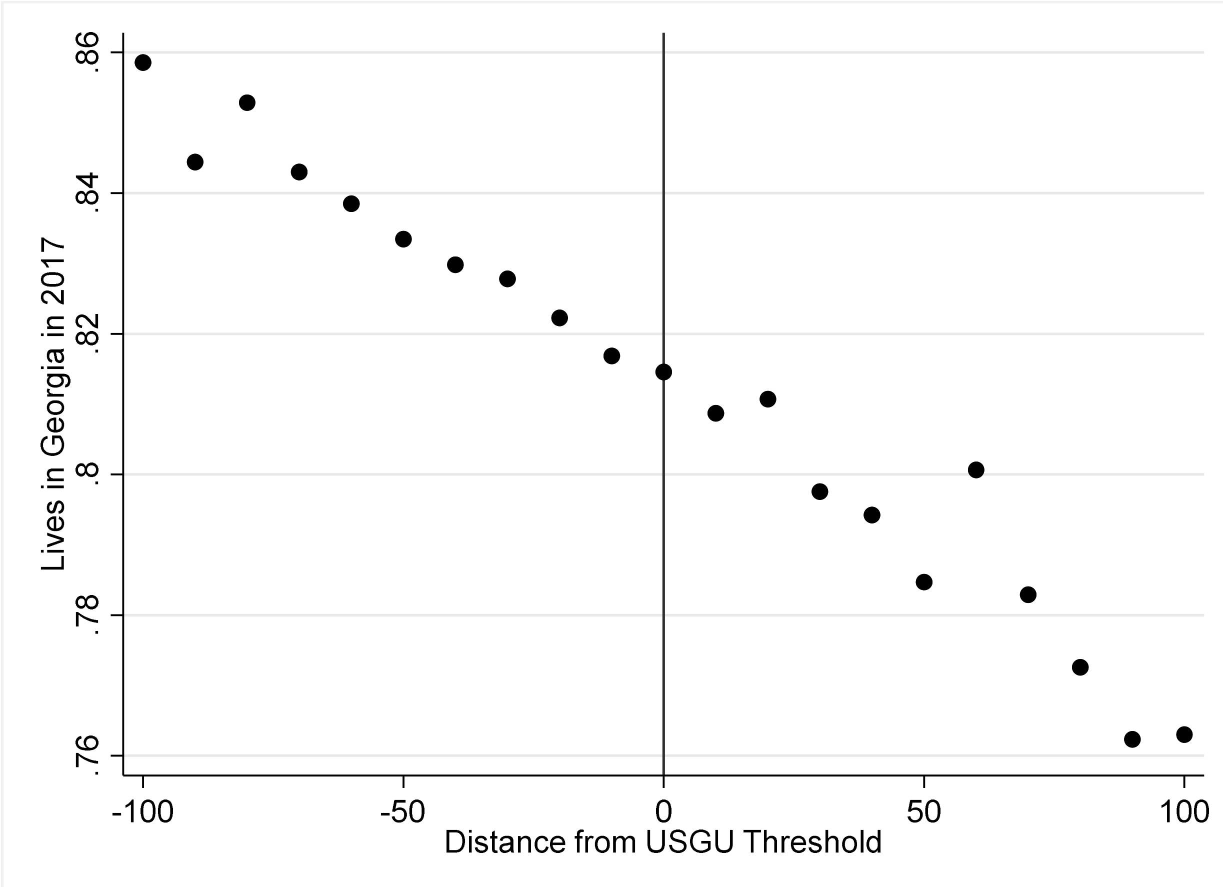
Notes: Shown above is the average total student loan balance around age 30, by distance from the University System of Georgia's admissions threshold. The financial health index is the first principal component of standardized versions of an individual's credit score, past year payment delinquency status, past year amount past due, and an indicator for ever having declared bankruptcy. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Figure A5 – Probability of Having a Mortgage



Notes: Shown above is the probability of having a mortgage around age 30, by distance from the University System of Georgia's admissions threshold. The financial health index is the first principal component of standardized versions of an individual's credit score, past year payment delinquency status, past year amount past due, and an indicator for ever having declared bankruptcy. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Figure A6 – Probability of Living in Georgia



Notes: Shown above is the probability of living in Georgia around age 30, by distance from the University System of Georgia's admissions threshold. The financial health index is the first principal component of standardized versions of an individual's credit score, past year payment delinquency status, past year amount past due, and an indicator for ever having declared bankruptcy. The sample includes all Georgia high school graduates between 2004 and 2008 who first took the SAT in their senior year and who were matched to financial data.

Table A1 - Matching College Board to Financial Outcomes Dataset

	Matched to Financial Outcome Data (1)	Matched to Financial Outcome Data and Valid Income Measure (2)
(A) All students		
USG access	0.0002	0.0016
	(0.0028)	(0.0034)
N	70,404	70,404
(B) By high school income		
Low income	0.0004	0.0062
	(0.0040)	(0.0044)
N	32,416	32,416
Middle/high income	-0.0006	-0.0032
	(0.0040)	(0.0048)
N	37,988	37,988
(C) By race/ethnicity		
URM	0.0015	-0.0001
	(0.0043)	(0.0050)
N	30,253	30,253
Non-URM	-0.0010	0.0012
	(0.0036)	(0.0048)
N	40,151	40,151

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. The sample includes all Georgia students from the graduating high school cohorts of 2004-2008, including those who were and were not matched to credit bureau data.

Table A2 - Predicted Outcomes Balance

Predicted	USG enrollment (1)	4-year college enrollment (2)	B.A. completion (3)	Estimated household income (4)	Logarithm of estimated household income (5)	Financial health index (6)	Total student loan balance (7)	Residence in Georgia (8)
(A) Full RD sample								
USG access	0.000	-0.001	0.001	0.080	0.001	0.002	-0.080	0.000
	(0.001)	(0.002)	(0.001)	(0.074)	(0.001)	(0.002)	(0.143)	(0.000)
Control mean	0.35	0.55	0.34	63.97	10.99	-0.11	23.72	0.81
N	66,356	66,356	66,356	66,356	66,356	66,356	66,356	66,356
(B) By high school income								
Low income	0.000	-0.001	0.001	0.226**	0.003**	0.006**	-0.307	-0.001
	(0.002)	(0.002)	(0.001)	(0.103)	(0.001)	(0.003)	(0.213)	(0.000)
Control mean	0.35	0.53	0.29	59.71	10.94	-0.20	27.80	0.83
N	30,629	30,629	30,629	30,629	30,629	30,629	30,629	30,629
Middle/high income	0.000	-0.001	0.001	-0.056	-0.001	-0.002	0.147	0.000
	(0.002)	(0.002)	(0.002)	(0.103)	(0.001)	(0.003)	(0.198)	(0.000)
Control mean	0.36	0.56	0.38	67.98	11.04	-0.03	19.90	0.79
N	35,727	35,727	35,727	35,727	35,727	35,727	35,727	35,727
(C) By student race/ethnicity								
URM	-0.001	-0.003	-0.001	0.055	0.001	0.002	-0.185	0.000
	(0.002)	(0.002)	(0.002)	(0.065)	(0.001)	(0.002)	(0.166)	(0.000)
Control mean	0.39	0.61	0.32	57.06	10.90	-0.28	35.53	0.82
N	28,569	28,569	28,569	28,569	28,569	28,569	28,569	28,569
Non-URM	0.002	0.002	0.002	0.017	0.000	0.000	0.141	0.000
	(0.002)	(0.002)	(0.002)	(0.062)	(0.001)	(0.001)	(0.107)	(0.000)
Control mean	0.32	0.49	0.35	69.89	11.07	0.03	13.62	0.80
N	37,787	37,787	37,787	37,787	37,787	37,787	37,787	37,787

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is a reduced form estimate of being above the threshold on the listed predicted outcome, where predictions are based on race, gender, PSAT score and an indicator for PSAT-taking. All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. The sample includes all Georgia students from the graduating high school cohorts of 2004-2008 who first took the SAT in senior year and who were matched to credit bureau data. Control means are for observations 10 SAT points below the threshold.

Table A3 - Enrollment in USG Sectors

	Research university	Comprehensive university	State university
	(1)	(2)	(3)
(A) All students			
USG access	0.013***	0.034***	0.004
	(0.003)	(0.005)	(0.005)
Control mean	0.027	0.105	0.157
N	66,356	66,356	66,356
(B) By high school income			
Low income	0.012***	0.031***	0.011
	(0.004)	(0.006)	(0.008)
Control mean	0.030	0.081	0.195
N	30,629	30,629	30,629
Middle/high income	0.015***	0.037***	0.001
	(0.004)	(0.008)	(0.006)
Control mean	0.024	0.127	0.120
N	35,727	35,727	35,727
(C) By student race/ethnicity			
URM	0.018***	0.038***	0.014
	(0.005)	(0.008)	(0.010)
Control mean	0.036	0.099	0.225
N	28,569	28,569	28,569
Non-URM	0.009***	0.033***	0.004
	(0.003)	(0.008)	(0.006)
Control mean	0.018	0.109	0.098
N	37,787	37,787	37,787

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All columns show reduced form estimates of being above the threshold on enrollment in a USG college sector (with mean sector enrollment just below the threshold listed at bottom). All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. Research universities (Augusta, Georgia Institute of Technology, Georgia State, University of Georgia) are the most selective USG colleges, comprehensive universities (Georgia Southern, Kennesaw State, University of West Georgia, Valdosta State) are the next most selective, and state universities are the least selective.

Table A.4 - Robustness to Bandwidth and Controls, B.A. Completion

Bandwidth	40	60	80	100
	(1)	(2)	(3)	(4)
(A) All students				
USG enrollment	0.500**	0.352***	0.355***	0.274***
	(0.217)	(0.136)	(0.099)	(0.081)
CCM	0.088	0.151	0.149	0.165
N	48,145	66,356	80,480	92,409
(B) By high school income				
Low income	0.428	0.267	0.279**	0.251**
	(0.274)	(0.178)	(0.126)	(0.104)
CCM	0.262	0.291	0.229	0.220
N	22,119	30,629	37,213	42,690
Middle/high income	0.536	0.407**	0.400***	0.249**
	(0.331)	(0.198)	(0.145)	(0.113)
CCM	-0.061	0.031	0.086	0.155
N	26,026	35,727	43,267	49,719
(C) By student race/ethnicity				
URM	0.609**	0.306**	0.276**	0.184**
	(0.239)	(0.151)	(0.111)	(0.089)
CCM	0.028	0.163	0.197	0.245
N	20,621	28,569	34,738	39,807
Non-URM	0.243	0.438**	0.445***	0.373***
	(0.340)	(0.213)	(0.160)	(0.122)
CCM	0.210	0.118	0.083	0.076
N	27,524	37,787	45,742	52,602

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variable estimate of the impact of USG college enrollment on B.A. completion within six years of high school graduation (with control complier outcome means listed at bottom). All local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects, as well as PSAT scores and indicators for sex, race, and PSAT-taking status.

Table A.5 - Robustness to Bandwidth and Controls, Financial Health Index

	Without controls				With controls			
Bandwidth	40	**60**	80	100	40	60	80	100
	(1)	**(2)**	(3)	(4)	(5)	(6)	(7)	(8)
(A) All students								
USG enrollment	0.252	0.100	0.117	0.166	0.206	0.073	0.067	0.106
	(0.540)	(0.365)	(0.276)	(0.216)	(0.556)	(0.375)	(0.280)	(0.218)
CCM	-0.494	-0.144	-0.161	-0.220	-0.480	-0.131	-0.130	-0.183
N	47,895	65,995	80,048	91,918	47,895	65,995	80,048	91,918
(B) By high school income								
Low income	-0.168	-0.276	-0.170	0.112	-0.240	-0.363	-0.263	0.016
	(0.717)	(0.512)	(0.356)	(0.308)	(0.746)	(0.514)	(0.359)	(0.308)
CCM	-0.368	0.019	-0.026	-0.279	-0.349	0.071	0.034	-0.215
N	21,995	30,464	37,021	42,472	21,995	30,464	37,021	42,472
Middle/high income	0.704	0.473	0.432	0.209	0.670	0.521	0.440	0.191
	(0.831)	(0.476)	(0.398)	(0.281)	(0.832)	(0.497)	(0.408)	(0.285)
CCM	-0.705	-0.350	-0.376	-0.233	-0.687	-0.385	-0.386	-0.225
N	25,900	35,531	43,027	49,446	25,900	35,531	43,027	49,446
(C) By student race/ethnicity								
URM	-0.219	-0.131	-0.079	0.063	-0.251	-0.144	-0.107	0.043
	(0.616)	(0.407)	(0.327)	(0.281)	(0.652)	(0.415)	(0.330)	(0.283)
CCM	-0.512	-0.262	-0.259	-0.351	-0.524	-0.257	-0.241	-0.337
N	20,543	28,444	34,591	39,640	20,543	28,444	34,591	39,640
Non-URM	0.903	0.418	0.299	0.208	0.928	0.440	0.309	0.215
	(0.845)	(0.562)	(0.392)	(0.264)	(0.876)	(0.587)	(0.408)	(0.274)
CCM	-0.593	-0.140	-0.172	-0.175	-0.605	-0.154	-0.182	-0.186
N	27,352	37,551	45,457	52,278	27,352	37,551	45,457	52,278

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variable estimate of the impact of USG college enrollment on an index of financial health. All local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects. Column 2 is our preferred specification. Columns 5-8 adds controls for PSAT scores and indicators for sex, race, and PSAT-taking status.

Table A.6 - Robustness to Bandwidth and Controls, Total Student Loan Balance ($1,000s)

	Without controls				With controls			
Bandwidth	40	**60**	80	100	40	60	80	100
	(1)	**(2)**	(3)	(4)	(5)	(6)	(7)	(8)
(A) All students								
USG enrollment	10.672	11.913	3.852	-5.382	11.325	11.900	5.223	-3.576
	(16.654)	(11.254)	(9.250)	(7.337)	(16.805)	(11.323)	(9.064)	(7.254)
CCM	22.427	13.718	17.292	23.527	22.422	13.769	16.441	22.466
N	48,145	66,356	80,480	92,409	48,145	66,356	80,480	92,409
(B) By high school income								
Low income	6.423	5.977	4.573	-3.209	6.540	9.727	8.449	-0.091
	(23.557)	(15.765)	(11.814)	(9.897)	(23.535)	(15.027)	(11.145)	(9.469)
CCM	40.175	27.519	27.304	31.564	40.491	25.094	24.774	29.536
N	22,119	30,629	37,213	42,690	22,119	30,629	37,213	42,690
Middle/high income	12.585	16.634	5.148	-4.446	14.261	13.472	3.865	-4.363
	(23.141)	(15.075)	(13.151)	(9.770)	(23.160)	(15.697)	(13.418)	(9.889)
CCM	7.163	3.007	8.192	17.553	6.238	4.683	8.749	17.514
N	26,026	35,727	43,267	49,719	26,026	35,727	43,267	49,719
(C) By student race/ethnicity								
URM	9.647	3.897	-0.181	-9.390	5.029	2.930	0.623	-9.169
	(21.123)	(15.249)	(12.450)	(10.323)	(22.646)	(15.400)	(12.357)	(10.253)
CCM	32.377	34.305	36.237	43.271	35.199	34.932	35.690	43.096
N	20,621	28,569	34,738	39,807	20,621	28,569	34,738	39,807
Non-URM	17.793	27.122**	18.659*	13.821*	17.473	26.593*	17.892*	12.841
	(20.675)	(13.265)	(10.379)	(7.769)	(21.210)	(13.739)	(10.684)	(7.989)
CCM	6.745	-9.616	-4.669	0.572	7.156	-9.455	-4.416	1.058
N	27,524	37,787	45,742	52,602	27,524	37,787	45,742	52,602

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variable estimate of the impact of USG college enrollment on total student loan balance. All local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects. Column 2 is our preferred specification. Columns 5-8 adds controls for PSAT scores and indicators for sex, race, and PSAT-taking status.

Table A.7 - Robustness to Bandwidth and Controls, In-State Residence

	Without controls				With controls			
Bandwidth	40	**60**	80	100	40	60	80	100
	(1)	**(2)**	(3)	(4)	(5)	(6)	(7)	(8)
(A) All students								
USG enrollment	0.082	0.003	0.055	0.043	0.088	0.005	0.060	0.052
	(0.170)	(0.118)	(0.092)	(0.077)	(0.176)	(0.120)	(0.093)	(0.078)
CCM	0.792	0.845	0.797	0.799	0.789	0.845	0.793	0.793
N	48,145	66,356	80,480	92,409	48,145	66,356	80,480	92,409
(B) By high school income								
Low income	0.240	0.104	0.129	0.080	0.269	0.120	0.145	0.098
	(0.210)	(0.144)	(0.118)	(0.107)	(0.215)	(0.140)	(0.116)	(0.106)
CCM	0.623	0.766	0.745	0.759	0.601	0.756	0.734	0.746
N	22,119	30,629	37,213	42,690	22,119	30,629	37,213	42,690
Middle/high income	-0.083	-0.077	-0.013	0.018	-0.099	-0.089	-0.017	0.020
	(0.273)	(0.179)	(0.136)	(0.104)	(0.282)	(0.187)	(0.141)	(0.106)
CCM	0.970	0.913	0.850	0.841	0.986	0.924	0.855	0.841
N	26,026	35,727	43,267	49,719	26,026	35,727	43,267	49,719
(C) By student race/ethnicity								
URM	0.370**	0.170	0.191*	0.157*	0.402**	0.181	0.209**	0.175**
	(0.185)	(0.122)	(0.100)	(0.086)	(0.199)	(0.124)	(0.101)	(0.086)
CCM	0.610	0.773	0.756	0.754	0.587	0.765	0.742	0.740
N	20,621	28,569	34,738	39,807	20,621	28,569	34,738	39,807
Non-URM	-0.241	-0.129	-0.049	-0.041	-0.257	-0.138	-0.052	-0.037
	(0.325)	(0.198)	(0.148)	(0.113)	(0.338)	(0.208)	(0.154)	(0.116)
CCM	0.993	0.905	0.815	0.836	1.005	0.912	0.816	0.833
N	27,524	37,787	45,742	52,602	27,524	37,787	45,742	52,602

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each coefficient is an instrumental variable estimate of the impact of USG college enrollment on an indicator for residing in Georgia. All local linear regression discontinuity models use the listed bandwidth and include high school and cohort fixed effects. Column 2 is our preferred specification. Columns 5-8 adds controls for PSAT scores and indicators for sex, race, and PSAT-taking status.

Table A.8 - Effects for Male Students

	First stage	Instrumental variable estimate									
	USG	4-year	2-year	B.A.	A.A.	Estimated Household Income ($1000's)	Log(Estimated Household Income)	Financial health index	Student loan balance ($1000's)	Any mortgage	Still lives in Georgia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(A) All students											
USG access / enrollment	0.032***	1.065***	-1.131***	0.425	-0.441*	14.004	0.269	0.418	26.676	0.075	-0.113
	(0.010)	(0.261)	(0.405)	(0.302)	(0.239)	(17.337)	(0.240)	(0.945)	(24.780)	(0.314)	(0.313)
Control mean / CCM		-0.065	1.131	0.075	0.477	43.078	10.665	-0.353	-5.353	0.035	0.912
N	28,730	28,730	28,730	28,730	28,730	28,730	28,730	28,567	28,730	28,730	28,730
(B) By high school income											
Low income	0.029**	0.490	-0.968	0.417	-0.625	50.193	0.847	0.439	14.160	0.115	0.152
	(0.014)	(0.365)	(0.617)	(0.459)	(0.419)	(31.888)	(0.549)	(1.696)	(40.605)	(0.409)	(0.451)
Control mean / CCM		0.510	0.968	0.362	0.522	9.834	10.178	-0.534	34.016	-0.110	0.709
N	12,555	12,555	12,555	12,555	12,555	12,555	12,555	12,482	12,555	12,555	12,555
Middle/high income	0.038**	1.410***	-1.164**	0.424	-0.283	-6.300	-0.061	0.424	34.972	0.083	-0.285
	(0.015)	(0.418)	(0.495)	(0.367)	(0.260)	(23.736)	(0.304)	(0.939)	(28.596)	(0.415)	(0.383)
Control mean / CCM		-0.410	1.164	-0.125	0.402	61.433	10.936	-0.241	-29.961	0.103	1.051
N	16,175	16,175	16,175	16,175	16,175	16,175	16,175	16,085	16,175	16,175	16,175
(C) By student race/ethnicity											
URM	0.036**	0.896**	-0.849*	0.548	-0.035	26.348	0.514	0.982	32.238	0.326	0.286
	(0.015)	(0.389)	(0.460)	(0.455)	(0.225)	(24.012)	(0.386)	(1.335)	(41.795)	(0.321)	(0.421)
Control mean / CCM		0.104	0.849	0.138	-0.083	25.857	10.339	-0.502	23.458	-0.174	0.648
N	11,648	11,648	11,648	11,648	11,648	11,648	11,648	11,576	11,648	11,648	11,648
Non-URM	0.036***	1.099***	-1.366***	0.302	-0.704**	1.575	0.037	0.127	13.322	-0.070	-0.310
	(0.014)	(0.276)	(0.505)	(0.335)	(0.353)	(24.153)	(0.306)	(1.231)	(23.770)	(0.430)	(0.399)
Control mean / CCM		-0.099	1.366	0.092	0.848	57.069	10.928	-0.256	-13.525	0.124	1.027
N	17,082	17,082	17,082	17,082	17,082	17,082	17,082	16,991	17,082	17,082	17,082

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Column 1 shows first stage estimates of being above the threshold on enrollment in a USG college. The remaining columns show instrumental variable estimates of the impact of USG college enrollment on the listed outcome (with control complier outcome means listed at bottom). All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. The sample includes only male students.

Table A.9 - Effects for Female Students

	First stage	Instrumental variable estimate									
	USG	4-year	2-year	B.A.	A.A.	Estimated Household Income ($1000's)	Log(Estimated Household Income)	Financial health index	Student loan balance ($1000's)	Any mortgage	Still lives in Georgia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(A) All students											
USG access / enrollment	0.064***	0.546***	-0.294**	0.394***	-0.011	9.738	0.133	-0.031	6.684	0.073	0.030
	(0.009)	(0.105)	(0.137)	(0.146)	(0.095)	(8.385)	(0.110)	(0.365)	(13.223)	(0.108)	(0.117)
Control mean / CCM		0.454	0.294	0.134	0.027	52.235	10.836	-0.045	20.625	0.139	0.828
N	37,626	37,626	37,626	37,626	37,626	37,626	37,626	37,428	37,626	37,626	37,626
(B) By high school income											
Low income	0.070***	0.576***	-0.330**	0.258	-0.055	15.584	0.155	-0.577	5.697	0.068	0.080
	(0.014)	(0.152)	(0.152)	(0.188)	(0.124)	(10.382)	(0.139)	(0.487)	(17.258)	(0.125)	(0.134)
Control mean / CCM		0.424	0.330	0.244	0.042	47.707	10.790	0.252	24.139	0.114	0.772
N	18,074	18,074	18,074	18,074	18,074	18,074	18,074	17,982	18,074	18,074	18,074
Middle/high income	0.061***	0.530***	-0.273	0.505**	0.015	2.952	0.106	0.572	6.920	0.062	-0.001
	(0.013)	(0.131)	(0.226)	(0.224)	(0.139)	(12.587)	(0.161)	(0.550)	(19.113)	(0.175)	(0.192)
Control mean / CCM		0.470	0.273	0.038	0.025	57.226	10.880	-0.435	19.230	0.161	0.875
N	19,552	19,552	19,552	19,552	19,552	19,552	19,552	19,446	19,552	19,552	19,552
(C) By student race/ethnicity											
URM	0.096***	0.520***	-0.380***	0.269*	-0.009	5.645	0.114	-0.464	-0.363	-0.003	0.123
	(0.013)	(0.128)	(0.120)	(0.147)	(0.080)	(5.903)	(0.094)	(0.387)	(15.621)	(0.087)	(0.108)
Control mean / CCM		0.480	0.380	0.159	0.050	50.365	10.769	-0.133	34.132	0.088	0.799
N	16,921	16,921	16,921	16,921	16,921	16,921	16,921	16,868	16,921	16,921	16,921
Non-URM	0.048***	0.654***	-0.218	0.671**	-0.064	14.576	0.148	0.591	34.862*	0.110	-0.114
	(0.012)	(0.168)	(0.273)	(0.283)	(0.190)	(16.800)	(0.206)	(0.597)	(18.546)	(0.250)	(0.238)
Control mean / CCM		0.346	0.218	0.037	0.025	54.310	10.932	-0.014	-7.492	0.274	0.912
N	20,705	20,705	20,705	20,705	20,705	20,705	20,705	20,560	20,705	20,705	20,705

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Column 1 shows first stage estimates of being above the threshold on enrollment in a USG college. The remaining columns show instrumental variable estimates of the impact of USG college enrollment on the listed outcome (with control complier outcome means listed at bottom). All local linear regression discontinuity models use a bandwidth of 60 SAT points and include high school and cohort fixed effects. The sample includes only female students.