



December 2018

The Effectiveness of Washington's College Bound Scholarship Program

The 2015 Washington State Legislature directed the Washington State Institute for Public Policy (WSIPP) to conduct a comprehensive evaluation of the effect of Washington's College Bound Scholarship (CBS) program on secondary and postsecondary educational attainment. The CBS program is an early commitment program that guarantees financial assistance for college early in a student's academic career, conditional on a pledge from the student to fulfill certain requirements. This report presents an evaluation of the effect of Washington's early commitment program on student outcomes.

We provide more information on the CBS program and its relationship with other state aid programs in [Section I](#). [Section II](#) describes our evaluation approach, including data, methods used in the analysis, and outcomes of interest. [Sections III and IV](#) present our analysis of the effects of the College Bound pledge (described in [Section I](#)). [Sections V and VI](#) report our findings on the effects of the scholarship portion of the program. We provide a discussion of study limitations in [Section VII](#), and we conclude with a summary and next steps in [Section VIII](#).

Summary

Washington's College Bound Scholarship (CBS) program provides financial assistance to low-income undergraduate students. At public institutions, CBS covers full tuition and fees, plus a book stipend. Eligible students at corresponding private institutions receive the equivalent dollar value. To receive the scholarship, students must sign a pledge in middle school promising to graduate high school with at least a 2.0 GPA and no felony convictions and file a FAFSA or WASFA. Students who complete the pledge requirements and have family incomes at or below 65% of the state median family income during college can receive their full CBS award. The program started in the 2007-08 academic year with the first CBS cohorts entering college in the 2012-13 academic year.

This report presents results of our analysis of the effectiveness of pledge eligibility and signing as well as scholarship eligibility and receipt on education outcomes. We find that pledge signing has little effect on student outcomes. For those students who are eligible to receive the scholarship, however, we find positive effects on college enrollment, persistence, credit accumulation, and degree receipt. Receiving CBS dollars has some positive effects on attainment for college students and for 2-year college students specifically.

Suggested citation: Fumia, D., Bitney, K., & Hirsch, M. (2018). *The effectiveness of Washington's College Bound Scholarship program* (Document Number 18-12-2301). Olympia: Washington State Institute for Public Policy.

I. Program Background

Washington's College Bound Scholarship (CBS) program started in the 2007-08 academic year. Modeled after Indiana's 21st Century Scholars program, the first statewide early commitment program, CBS was created by the Washington State Legislature to increase college opportunities for low-income students by limiting financial barriers that may prevent them from accessing college and by informing them of these opportunities early in their academic careers.¹

CBS provides financial assistance to low-income students who sign a pledge in 7th or 8th grade promising to graduate from a Washington high school with at least a 2.0 grade point average (GPA), avoid felony convictions, and file a Free Application for Federal Student Aid (FAFSA) or a Washington Application for State Financial Aid (WASFA). Students who fulfill these requirements and have incomes below 65% of the state median family income (MFI) at the time of college attendance receive CBS funding. CBS covers full tuition and fees, plus a book stipend, at public institutions in Washington and the equivalent amount at corresponding private institutions ([Exhibit 1](#) shows the current CBS award amounts). Both full-time and part-time students can receive CBS funding with awards prorated based on enrollment intensity.

College Bound Scholarship Evaluation— Legislative Direction

The Washington state institute for public policy shall complete an evaluation of the college bound scholarship program and submit a report to the appropriate committees of the legislature by December 1, 2018. The report shall complement studies on the college bound scholarship program conducted at the University of Washington or elsewhere. To the extent it is not duplicative of other studies, the report shall evaluate educational outcomes emphasizing degree completion rates at both secondary and postsecondary levels. The report shall study certain aspects of the college bound scholarship program, including but not limited to:

- (a) College bound scholarship recipient grade point average and its relationship to positive outcomes;*
- (b) Variance in remediation needed between college bound scholarship recipient and their peers;*
- (c) Differentials in persistence between college bound scholarship recipients and their peers; and*
- (d) The impact of ineligibility for the college bound scholarship program, for reasons such as moving into the state after middle school or change in family income.*

Second Substitute Senate Bill 5851, Chapter 244, Laws of 2015.

Notes:

Data limitations prevented us from conducting a full analysis of part (d). We address this part of the assignment in Appendix IV. As specified by the legislative assignment, our evaluation complements a study of the College Bound Scholarship program occurring at the University of Washington. Goldhaber, D., Long, M., Gratz, T., & Rooklyn, J. (2017). *The effects of Washington's College Bound Scholarship program on high school grades, high school completion, and incarceration*. CEDR Working Paper No. 05302017-2-1. Seattle, WA: University of Washington.

¹ [RCW 28B.118.005](#).

Exhibit 1

College Bound Scholarship Award Amounts (2017-18)

Institution/sector	Amount
University of Washington	\$10,802
Washington State University	\$10,591
Central Washington University	\$7,248
Eastern Washington University	\$6,757
The Evergreen State College	\$7,177
Western Washington University	\$7,379
Private 4-year	\$11,904
Western Governor's University- Washington	\$6,280
Community & Technical Colleges (CTC)	\$4,438
CTC Applied Baccalaureate	\$6,757
Private 2-year non-profit	\$4,438
Private 2-year for-profit	\$4,467

Notes:

These award amounts reflect the minimum amount of state aid a CBS-eligible student can expect to receive. Because CBS is a last dollar program, most students will not receive the above amount from CBS funds directly. Much of the actual aid received comes from other state aid programs, primarily the State Need Grant, and CBS covers the remainder up to the CBS award amount. This report focuses on aid at public institutions in Washington.

Source: Washington Student Achievement Council.

Students must satisfy numerous eligibility criteria to receive CBS funding. First, students must be "pledge eligible," meaning they meet requirements allowing them to sign the pledge. Students are pledge eligible if they are in 7th or 8th grade (8th or 9th grade for those with an expected high school graduation in 2012) and satisfy any of the following:

- Participate in a free- or reduced-price lunch (FRL) program,
- Have a family income that would qualify them for FRL participation (referred to as income eligible),

- Live with a family that receives Temporary Assistance for Needy Families (TANF) or basic food (Supplemental Nutrition Assistance Program (SNAP)) benefits,² or
- Are in foster care.

Second, students must be "pledge signers." Pledge signers are pledge eligible and sign a pledge in eligible grades promising to graduate from a Washington high school with at least a 2.0 GPA, avoid felony convictions, and file for financial aid using a FAFSA or WASFA.³

Third, students who sign and fulfill the requirements of the pledge must meet the following requirements to be eligible to receive CBS in college:

- Have a family income at or below 65% of the state MFI,
- Enroll in an eligible undergraduate program⁴ by fall term within one academic year of high school graduation (e.g., a student graduating in spring of the 2011-12 academic year must enroll by fall of the 2013-14 academic year),
- Use no more than four academic years of funding, and

² The Washington Administrative Code (WAC) governing the College Bound Scholarship program does not identify basic food benefits as an avenue to pledge eligibility. The Washington Student Achievement Council, which oversees state financial aid programs, includes basic food benefits because requirements for these benefits are similar to those for TANF and free- or reduced-price lunch (S. Weiss, WSAC, personal communication, 7/17/2018).

³ WAC 250-84-030. During our analysis period, students could only file a FAFSA.

⁴ Eligible undergraduate programs lead to a Baccalaureate, Associate's, undergraduate professional degree, or qualifying vocational degree and must be at a college or university participating in the State Need Grant program. WAC 250-84-060.

- Use all funding within five academic years of August of their high school graduation year.⁵

Receipt does not need to be continuous. Students must satisfy the income requirement each year they receive CBS, but they can receive funding in any year they are eligible regardless of whether they were eligible in the prior year. Students who fulfill all requirements and receive CBS dollars are “CBS recipients.” A summary of these requirements is included in [Exhibit 2](#).

Exhibit 2

CBS Eligibility Requirements as Used in This Report

Term	Definition
Pledge-eligible student	A student in 7 th or 8 th grade (8 th or 9 th for those with an expected high school graduation in 2012) who satisfies at least one of the following: <ul style="list-style-type: none"> • Receives free- or reduced-price lunch (FRL) services, • Has an income at or below the threshold for FRL eligibility, • Receives Temporary Assistance for Needy Families (TANF) or Supplemental Nutrition Assistance Program (SNAP) benefits, or • Is in foster care.
Pledge signer	A student who is pledge eligible and signed the pledge.
CBS-eligible student*	A student who signed the pledge and satisfies the following requirements: <ul style="list-style-type: none"> • Graduates from a Washington high school, • Has at least a 2.0 cumulative GPA, • No felony convictions, and • Has a family income at or below 65% of the state MFI.
CBS recipient**	A student who is CBS eligible and satisfies all of the following: <ul style="list-style-type: none"> • Filed a FAFSA, • Has a family income at or below 65% of the state MFI, • Enrolled by fall term of the academic year following high school graduation, • Uses all four years of CBS within five academic years of high school graduation, and • Receives CBS dollars.

Note:

* The Washington Student Achievement Council (WSAC) refers to students who satisfy the pledge requirements, which includes filing a FAFSA, as College Bound Scholars. Our definitions separate the FAFSA requirement from other pledge requirements.

**Some students may be CBS-eligible in college and not receive CBS dollars because they receive their full CBS award from other state aid sources. These students are excluded from our main analyses of CBS receipt but are included in our analysis in Appendix IV.

⁵ [WAC 250-84-060](#).

CBS and Other State Aid

As a last dollar program, the actual amount of dollars received from the CBS program equals the CBS award amount (i.e., 100% tuition plus a \$500 book stipend) less any other state aid. Other state aid includes the State Need Grant (SNG), the Opportunity Scholarship, the State Board of Community and Technical Colleges (SBCTC) Opportunity Grant, and the American Indian Endowed Scholarship awards.

The State Need Grant (SNG) program, which is available to all CBS-eligible students, is the largest state aid program. SNG provides financial assistance to students with family incomes at or below 70% of the state MFI, although SNG receipt is not guaranteed. For example, in the 2016-17 academic year, 20,769 SNG-eligible students received no SNG funding.⁶

Students with family incomes at or below 50% of MFI are eligible for the maximum SNG award. Awards are then prorated based on MFI; for example, students with family incomes between 65% and 70% of the state MFI are eligible for half of the full SNG award. [Exhibit 3](#) shows the eligible awards for CBS and SNG.⁷

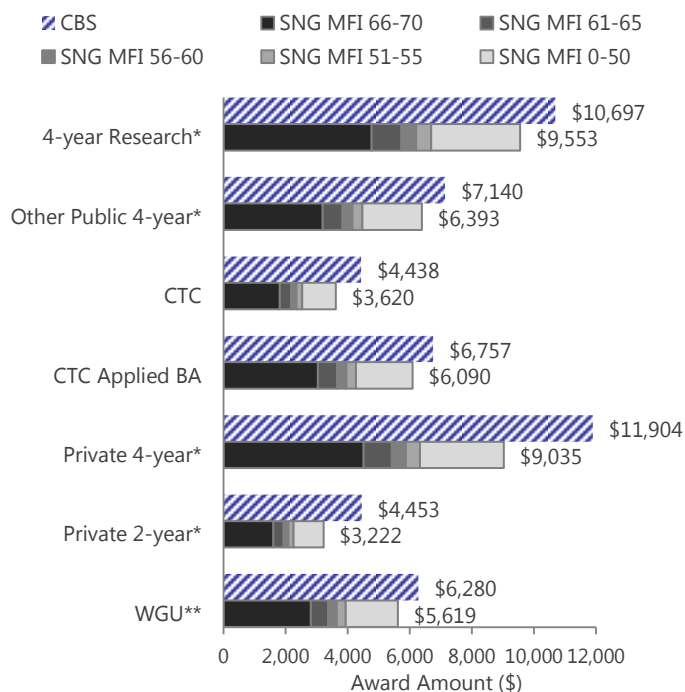
Although all CBS-eligible students could receive SNG, prior to the 2015-16 school year, between 20% and 30% of CBS recipients received no SNG award (see [Exhibit 4](#)). In 2015, the Washington State Operating Budget guaranteed all CBS-

⁶ WSAC. [Financial aid overview](#).

⁷ WSIPP previously evaluated the SNG program and found that SNG receipt increased re-enrollment and graduation rates. Bania, N., Burley, M., & Pennucci, A. (2013). [The effectiveness of the state need grant program: Final evaluation](#). (Doc. No. 14-01-2301). Olympia: Washington State Institute for Public Policy.

Exhibit 3

Maximum Award Amounts for CBS and SNG (2017-18)



Notes:

For each MFI category, the total SNG award is the sum of all lower MFI categories. For example, the award for students in the 61%-65% MFI category is the sum of the darkest gray 66%-70% MFI bar and the next darkest gray 61%-65% MFI bar.

*Award amounts averaged across all schools in this sector. Actual award amounts for a given institution within this sector may differ.

**WGU refers to Western Governor's University.

Source: Washington Student Achievement Council. (2017). *State Need Grant and College Bound Scholarship program manual 2017-18*. Olympia, WA: Washington Student Achievement Council and WSIPP calculations.

eligible students would receive their full SNG award prior to receiving any CBS dollars. CBS-eligible students also typically receive CBS dollars because CBS awards exceed SNG awards by between \$600 and almost \$7,000, depending on family income and the institution attended.⁸

⁸ For example, for the 2017-18 school year, the maximum CBS award is \$667 more than the SNG award amount for students at Eastern Washington University with a family income between 0% and 50% of the state MFI. On the high end, students attending a private, for-profit 4-year institution with a family income between 61% and 65% of the state MFI have a CBS award that is \$6,794 more than their maximum SNG award. Washington Student Achievement Council.

Exhibit 4

Average CBS and SNG Award Amounts for CBS Recipients

	Academic year			
	2012-13	2013-14	2014-15	2015-16
CBS recipients	4,689	8,343	11,672	14,605
Percent of CBS recipients who receive SNG	70%	74%	80%	100%
Average CBS award	\$2,750	\$2,447	\$2,308	\$1,343
Average SNG award for CBS recipients who receive SNG	\$5,620	\$5,915	\$5,817	\$5,742

Notes:

Award amounts are averaged over all institutions and MFI categories. Award amounts for specific institutions and MFI categories will differ from those reported here.

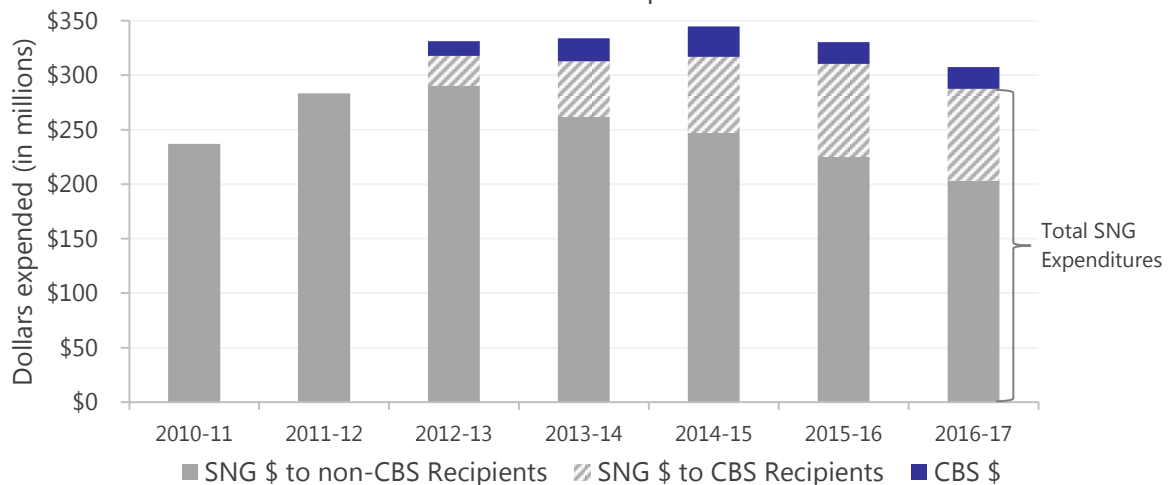
Source: WSAC. (2017). *College Bound Scholarship report*. Olympia, WA: Washington Student Achievement Council.

The requirement to provide an SNG award to CBS-eligible students before awarding CBS dollars has reduced the per-student CBS dollars received over time. [Exhibit 4](#) illustrates the relationship between CBS and SNG awards for the first four cohorts of CBS recipients.

Expenditures for SNG outweigh all other state aid programs including CBS, although a portion of SNG expenditures goes to CBS recipients ([Exhibit 5](#)). The SNG program, therefore, plays an important role in determining the actual dollar amount students receive from the CBS program and the funds available to similar low-income students who are not CBS eligible.

Exhibit 5

Total SNG and CBS Expenditures



Notes:

Expenditures inflated to 2017 dollars.

Source: Washington Student Achievement Council and WSIPP calculations.

(2017). *State Need Grant and College Bound Scholarship program manual 2017-18*. Olympia, WA: Washington Student Achievement Council.

Research on Washington's College Bound Scholarship Program

WSIPP previously evaluated whether early commitment programs across the country improve student outcomes; we found mixed effects on educational attainment and achievement.⁹ Most research on the effectiveness of these early commitment programs evaluated programs outside of Washington State. An important exception is concurrent work at the Center for Education and Data Research (CEDR).

First, CEDR studied what factors explain whether a student signs the pledge in middle school.¹⁰ They find various student characteristics are associated with pledge signing including higher test scores and participation in gifted services. They also find that female students and non-white students are more likely to sign the pledge.

The study also highlights a variety of school, district, and program implementation characteristics that influence whether a student signs the pledge. They find districts with high sign-up rates tend to have (1) district-level "buy-in" coupled with individuals who take responsibility for the program in their schools, (2) access to data that allows schools to identify and target eligible students, (3) guidance counselors with time to build relationships with students, and (4) strong college-going cultures.

CEDR also evaluated the effects of Washington's College Bound pledge on high school graduation, high school GPA, and incarceration as an adult.¹¹ They find that being eligible to sign the pledge reduces a student's cumulative GPA in 12th grade and find only a spurious effect on on-time high school degree receipt. On the other hand, they find that pledge eligibility reduces the likelihood of incarceration. Researchers from CEDR will also extend their analysis to evaluate the effects of the pledge on various college outcomes and juvenile incarceration.

WSIPP's current report on the effects of the College Bound pledge complements the Goldhaber, et al. (2017) study. We focus on the effects of pledge eligibility and signing on college (rather than high school) outcomes.¹² Because we evaluate the effects of pledge signing as well as eligibility, we do report the effects of pledge signing on some high school outcomes. We also provide findings of the effect of the pledge on charges and convictions (rather than incarceration) prior to high school completion ([Appendix I](#)). Additionally, our report evaluates the effects of the actual scholarship award among students who satisfy the pledge requirements. Our results of the effects of eligibility to sign the pledge on high school outcomes are consistent with the current results reported by CEDR. We therefore report results from the CEDR study when relevant.

⁹ Hoagland, C., Bitney, K., Cramer, J., Fumia, D., & Lee, S. (2018). *Interventions to promote postsecondary attainment: April 2018 update* (Doc. No. 18-04-2301). Olympia: Washington State Institute for Public Policy.

¹⁰ Goldhaber, D., Long, M.C., Person, A., & Rooklyn, J. (2016). *Why do middle school students sign up for Washington's College Bound Scholarship Program? A mixed methods evaluation*. CEDR Working Paper. WP# 2016-3. Center for Education Data & Research.

¹¹ Goldhaber et al. (2017).

¹² We use a similar research design to evaluate the effects of pledge eligibility as that used in the CEDR study, although our approach varies slightly in our methodology and variable definitions. These methodological choices may result in differences in our findings. For more detail on our methodology compared to that used in the CEDR study, see [Appendix II](#).

II. Evaluation Methodology

The CBS program's early commitment to fund a student's college education sets it apart from many aid programs. Thus, we begin by evaluating the effect of the early commitment portion of the program—the pledge—on education outcomes (Pledge Analysis).¹³

We next determine whether students who satisfy the pledge requirements—i.e., are CBS-eligible students or CBS recipients—have different education outcomes compared to similar students who were not eligible for the CBS scholarship (Scholarship Analysis).

In this section, we describe our data, research questions, study groups, and methods used in each of these analyses. We then describe the education outcomes.

Data Sources

For both the Pledge Analysis and the Scholarship Analysis, we use administrative education data from numerous sources. The data were collected and matched across data sources by the Education Research and Data Center (ERDC).¹⁴ Sources of data include the following:

- The Office of Superintendent for Public Instruction (OSPI), which provides information on students in Washington State K–12 public schools;
- The Public Centralized Higher Education Enrollment System (PCHEES), which provides college-related records for students at public 4-year institutions;
- The State Board of Community and Technical Colleges (SBCTCs), which provides college-related data for students at public 2-year institutions; and
- The Washington Student Achievement Council (WSAC), which provides financial aid records for students who enroll in Washington State higher education institutions and receive state need-based aid.

¹⁴ For additional information on the ERDC, please see [ERDC's website](#). ERDC states,

The research presented here utilizes confidential data from the Education Research and Data Center (ERDC), located within the Washington Office of Financial Management (OFM). Committed to accuracy, ERDC's objective, high-quality data helps shape Washington's education system. ERDC works collaboratively with educators, policymakers and other partners to provide trustworthy information and analysis. ERDC's data system is a statewide longitudinal data system that includes de-identified data about people's preschool, educational, and workforce experiences. The views expressed here are those of the author(s) and do not necessarily represent those of the OFM or other data contributors. Any errors are attributable to the author(s).

¹³ The results reported in the main report focus on academic outcomes. In Appendix I, we provide results for non-education outcomes including financial aid and criminal justice system involvement.

Additionally, through a separate agreement with the Administrative Office of the Courts (AOC), we obtained administrative data on felony and misdemeanor charges and convictions. ERDC matched these data to our initial dataset.

We provide estimates of the number of pledge-eligible students, pledge signers, CBS-eligible students, and CBS recipients in our data in [Exhibit 6](#). This exhibit demonstrates the trajectory from pledge eligibility to CBS receipt.

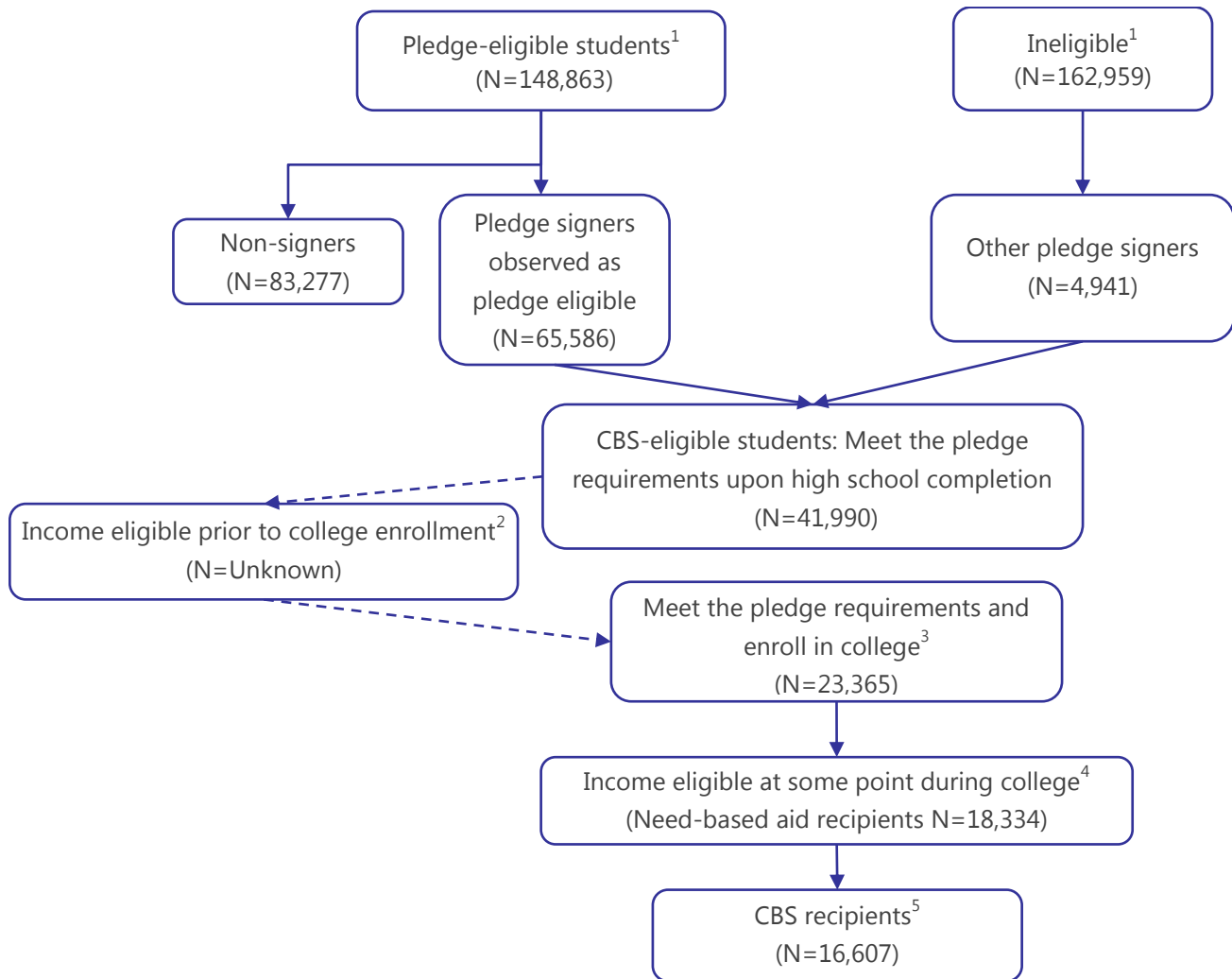
Importantly, we cannot capture the entire population of pledge-eligible students with available data. Our data includes only K–12 public school students; we cannot observe private- or home-school students. We also cannot observe a student’s foster care status or TANF or SNAP receipt. However, foster care students and students who qualify for TANF and SNAP are automatically enrolled in the FRL program and most should be captured through our definition. Finally, we cannot identify students who are income eligible only—i.e., students have an income that would qualify them for FRL receipt, and thus are eligible to sign the pledge, but do not receive FRL, TANF, or SNAP.

Similarly, we could not obtain family income data on CBS-eligible students who did not receive state need-based aid.¹⁵ Thus, our count of pledge-eligible and CBS-eligible students represents a subset of all pledge- or CBS-eligible students. However, we do have access to data on all pledge signers regardless of foster care status, TANF or SNAP receipt, or public school attendance.

¹⁵ Some possible reasons these students would not be included in the need-based aid data include not enrolling in college or not filing a FAFSA or WASFA.

Exhibit 6

Estimated Number of Students Satisfying CBS Program Requirements



Notes:

Source: WSIPP calculations using data from ERDC. Reported counts include all students observed in public school in 7th and 8th grade after CBS implementation (i.e., those with expected high school graduation in spring of 2012 through spring of 2015). Our analysis excludes some of these students due to missing data on student characteristics. For more information on how many of these students we exclude due to missing data, see Appendix V.

¹ Our data only allows us to observe eligibility based on FRL status. Students receiving TANF or basic food benefits or students who are in foster care are automatically FRL eligible. Goldhaber et al. (2017) estimate that FRL status captures about 87% of all eligible students. Additionally, as noted above, some students in the ineligible group are income eligible to sign the pledge, but we use the term “ineligible” for students who we do not observe receiving FRL services in the eligible grades. Students who are income eligible only will be in the “ineligible” group; thus, not all students in the “ineligible” are truly ineligible to sign the pledge.

² We do not have income data on students unless they enroll in college and receive need-based aid. Thus, we cannot determine exactly how many students of the 41,990 students have family incomes below 65% of the state MFI. We know 33,916 students who met the high school requirements for the scholarship received FRL services in 12th grade. Based on our calculations using data from the American Community Survey, more than 95% of family households in Washington State with an income at or below the threshold for FRL receipt have an income below 65% of Washington’s MFI. Thus, we estimate that most of these 33,916 students are income eligible for CBS, providing a reasonable lower bound estimate of the number of income-eligible students.

³ These counts are lower bound estimates. The current study includes information on students attending in-state public institutions only. Students who attend a Washington private institution or attend college out-of-state are not included in these counts. A supplement to this report (expected February 2019) will expand the analysis to students at other institutions.

⁴ We do not have income information on enrolled students who do not receive need-based aid. Thus, we can only determine eligibility for students receiving need-based aid not for all college enrollees.

⁵ Students who receive the full value of their CBS award amount from other state aid programs and receive no money from the CBS program are not included in this count.

Pledge Analysis

We first summarize our research questions, study groups, and methods used to examine the effects of the College Bound pledge.

Pledge Analysis Research Questions

Offering students the opportunity to sign the pledge early in their academic careers provides students with a unique opportunity to know prior to high school that they can realistically obtain funding for college. However, not all eligible students sign the pledge.

Accordingly, our study answers two questions related to the College Bound pledge. First, what is the effect of offering students the opportunity to sign the pledge early in their academic careers? That is, what is the effect of being eligible to sign the pledge on student outcomes? Answering this question can inform policymakers as to whether making the pledge available to students has any effects on education outcomes.

Because not all eligible students sign the pledge, an equally important second question is: what is the effect of actually signing the pledge on student outcomes? We address both questions in this report.

Pledge Study Groups

The administrative data follow students observed in 7th grade between the 2004-05 and 2009-10 school years. Students were followed through the 2015-16 school year. [Exhibit 7](#) defines the student cohorts used in our analysis and their expected grades and college enrollment based on on-time progression. We identify a student's cohort based on their last time observed in a grade. Retained students are included in the cohort corresponding to the last time they are observed in a given grade.

Effect of Pledge Eligibility. The treatment group for this research question includes students who are FRL in 7th or 8th grade (or 8th or 9th grade for Cohort Three). Students who are FRL in 7th or 8th grade prior to CBS implementation are pseudo eligible—they would have been eligible to sign the pledge if the pledge existed. The comparison group consists of those who are ineligible to sign the pledge—i.e., not FRL in 7th or 8th grade (8th or 9th for Cohort Three).¹⁶ We observe the treatment and comparison groups both prior to CBS implementation (two “pre-period” cohorts) as well as after CBS was implemented (four “post-period” cohorts or “CBS cohorts”).

Our data include 478,502 students observed in 7th grade. Of those, 432,457 students have characteristics measured in 7th and 8th grade and constitute our analytic sample for the

¹⁶ For ease, we use the term “ineligible” for students who we do not observe receiving FRL services in the eligible grades. Students who are income eligible only will be in the “ineligible” group; thus, not all students in the “ineligible” are truly ineligible to sign the pledge. We also use pledge eligible to refer to those who we observe receiving FRL in 7th or 8th grade (or 8th or 9th grade for Cohort Three). Some of these students are pseudo eligible because they were FRL in eligible grades before CBS existed.

pledge analysis.¹⁷ These include 138,339 from before CBS implementation (pre-period cohorts) and 294,118 in cohorts after CBS implementation (post-period cohorts).

Effect of Pledge Signing. To determine the effect of pledge signing, the treatment group includes all students who signed the pledge regardless of whether they were clearly pledge eligible.¹⁸ The comparison group includes students who did not sign the pledge.

Exhibit 7

School Grades and Postsecondary Years by Cohort Assuming On-Time Progression

	Expected high school graduation year	Pre-period cohorts		Post-period cohorts ("CBS cohorts")			
		Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 5	Cohort 6
		2010	2011	2012	2013	2014	2015
School year	2004-05	7	6				
	2005-06	8	7	6			
	2006-07	9	8	7	6		
	2007-08	10	9	8	7	6	
	2008-09	11	10	9	8	7	6
	2009-10	12	11	10	9	8	7
	2010-11	PS1	12	11	10	9	8
	2011-12	PS2	PS1	12	11	10	9
	2012-13	PS3	PS2	PS1	12	11	10
	2013-14	PS4	PS3	PS2	PS1	12	11
	2014-15	PS5	PS4	PS3	PS2	PS1	12
	2015-16	PS6	PS5	PS4	PS3	PS2	PS1

Notes:

PS refers to postsecondary or college years. Pre-period cohorts are those students who are 7th or 8th grade prior to CBS implementation. Post-period cohorts are those students who entered 7th or 8th grade (or 9th for Cohort Three) after CBS was implemented. Shading highlights the grades and years where students could be pledge eligible (dark gray) or pseudo eligible (light gray).

¹⁷ We exclude students with missing data on student characteristics used in the analysis. Thus, the sample sizes used in analyses that include these student characteristics are smaller. To see how these excluded students differ from those included in the analysis, see Appendix V.

¹⁸ We use the term "clearly pledge eligible" to refer to students we observe receiving FRL in eligible grades. We borrow this terminology from Goldhaber et al. (2017).

Pledge Analysis Methods

The “gold standard” approach to estimating statistically valid treatment effects is random assignment. Random assignment allows for a direct, unbiased comparison of outcomes between program participants—the treatment group—and non-participants—the comparison group. Under random assignment, we would expect no difference in characteristics between treatment and comparison group members. We could then attribute any differences in outcomes to being randomly assigned to, for example, pledge eligibility or pledge signing rather than differences in student characteristics.

However, we cannot randomly assign students to be pledge eligible or to sign the pledge. Students who sign the pledge, for example, may differ systematically from those who do not in ways that could affect their education outcomes—this difference is referred to as “selection bias.”

For instance, pledge signers may already have higher educational aspirations than non-signers. Higher aspirations may lead those students to sign the College Bound pledge, and they may also drive students to graduate high school *regardless of whether they signed the pledge*. Simply observing a higher high school graduation rate among pledge signers would not necessarily indicate that the pledge *caused* students to graduate at higher rates. As illustrated in this example, the difference in aspiration levels would actually explain the higher graduation rates.

Without the option of random assignment, we utilize “difference-in-differences” (see sidebar) to evaluate the effects of offering and signing the College Bound pledge on student outcomes.

Evaluating Effects Using Difference-in-Differences

We use a **difference-in-differences (DID)** design to determine the effect of pledge eligibility on student outcomes. A DID design compares the change before and after a program is implemented for a treatment group to the change before and after program implementation for a comparison group.

Under certain assumptions (discussed in [Appendix II](#)), DID eliminates both observed and unobserved changes during the period of implementation that affect both groups and could be erroneously attributed to the program. For example, high school graduation rates may be increasing for all students who were in middle school around the time of CBS implementation. If one observed an increase in graduation rates for pledge-eligible students, that increase may be mistakenly ascribed to the program when, in fact, it was just a general trend. DID designs can prevent this type of error.

The implementation of CBS is particularly suited to this type of design. Because the program started in 2007-08, we can identify a clear point in time where some students were in middle school prior to CBS and others were in middle school in the post-CBS period. The program also created a clear treatment group—low-income students who were eligible to sign the pledge—and a comparison group—students who were ineligible to sign the pledge. The effect of the program is the change in outcomes for the pledge eligible group before and after CBS implementation minus the change in the ineligible group. We also account for differences in student and school characteristics.

We then use our DID estimate combined with another statistical technique, **instrumental variable analysis**, to identify the effect of pledge signing. For more detail on both designs, see [Appendix II](#).

Scholarship Analysis

As discussed above, the second component of the CBS program is the actual scholarship award. Here we present our research questions, study groups, and methods used to determine the effect of the scholarship award on student outcomes.

Scholarship Analysis Research Questions

A relevant policy question is whether students who sign the College Bound pledge and satisfy the requirements to receive the scholarship—CBS-eligible students—have better outcomes than similar students who are not eligible for CBS. While the Pledge Analysis illustrates whether merely offering students an early commitment (the pledge) can affect outcomes, the Scholarship Analysis addresses whether those who satisfy the pledge and are in a position to use the scholarship have different outcomes. For a summary of the distinction between pledge eligibility and CBS eligibility, see [Exhibit 2](#).

The majority of CBS-eligible students who enroll in college receive CBS dollars. Consequently, the appropriate time to evaluate the effects of CBS eligibility on student outcomes occurs at the time of high school completion, while the effects of receiving CBS are measured in college. To capture the full potential effect of the scholarship award, we answer two related research questions.

First, do students who are eligible to receive CBS upon high school completion have different education outcomes compared to similar students who are not eligible to receive CBS? Answering this question will demonstrate whether those who satisfy pledge requirements and are in a position to access CBS funds have improved outcomes

compared to similar students who could not obtain CBS funding.

Second, do students who receive CBS dollars in college have different education outcomes compared to students who do not receive CBS dollars but did receive need-based aid? Because CBS recipients receive more need-based aid than similar low-income students who do not receive CBS (see [Appendix I](#)), we want to determine whether the increased funding for students who receive CBS in college improves education outcomes.

Scholarship Analysis Study Groups

As discussed, the scholarship analysis includes two components—the effect of CBS eligibility prior to college enrollment and the effect of CBS receipt among college enrollees.

Effect of CBS Eligibility. For the effect of CBS eligibility, the treatment group includes CBS-eligible students observed in 12th grade. Students in the treatment group must have signed the pledge, graduated high school with at least a 2.0 GPA, and had no felony convictions between pledge signing and high school completion. Unfortunately, we could not obtain data on a student's income at the time of high school completion to verify whether a student met the income threshold for CBS eligibility. We use FRL in 12th grade as a proxy for meeting the 65% MFI threshold. Consequently, we restrict our treatment group to those students who receive FRL in 12th grade.¹⁹

¹⁹ Because the income threshold for FRL receipt is below 65% of the state's MFI for most households, we assume that most students receiving FRL in 12th grade would meet the MFI threshold for CBS eligibility. Our findings do not necessarily apply to students who are not receiving FRL in 12th grade but are still CBS eligible based on their family income.

Our comparison group includes students in the pre-period cohorts who would have been eligible for the scholarship had it been available (i.e., pseudo-eligible high school graduates with at least a 2.0 GPA who had no felony convictions between 7th grade and high school completion). Comparison students must receive FRL in 12th grade.

We restrict these analyses to students in the first four cohorts, two from before CBS implementation, and two cohorts immediately after CBS implementation.²⁰

Effect of CBS Receipt. We next evaluate the effect of receiving CBS among the subset of CBS-eligible students who receive CBS dollars.

To do so, we define a treatment group consisting of CBS recipients from the first two CBS cohorts and a comparison group of need-based aid recipients from the two pre-period cohorts. Students from the comparison group must meet the pledge requirements and have a family income at or below 65% of the state MFI.

Scholarship Analysis Methods

As described above, our main goal is to minimize selection bias due to non-random assignment. To estimate the effects of CBS eligibility and receipt, we employ “propensity score matching” (see sidebar).

When reporting effects of CBS receipt, our main results disaggregate the effect by institution type to account for differences between students first enrolling at 2-year and 4-year institutions. We also estimate effects for all college students combined. We focus on the effects of CBS receipt during a student’s first year of college.

Evaluating Effects Using Propensity Score Matching

Ideally, treatment and comparison group participants should have similar observed (i.e., measured within the data) and unobserved (i.e., not measurable within the data) characteristics.

Propensity score matching attempts to create treatment and comparison groups that have similar *observable* characteristics.

Propensity score matching has four steps. First, we define a treatment group (e.g., CBS-eligible students or CBS recipients). We then define a potential pool of comparison students as those who are not CBS eligible or scholarship recipients.

Second, we predict a student’s likelihood, or probability, of being CBS eligible or receiving a scholarship based on that student’s background characteristics.

Third, we match treatment group students to comparison group students based on that predicted probability from step two. We discard unmatched comparison group students to arrive at our matched sample. This procedure should produce a matched comparison group that, on average, has the same observed background characteristics as the treatment group.

Finally, we conduct a **regression analysis** using this matched sample to determine the effect of the treatment—CBS eligibility or receipt—on education outcomes.

For more detail on these and other methods used in this analysis, see [Appendix III](#).

²⁰ A further discussion of this restriction can be found in Appendix III.

Outcomes

In the main report, we focus on the effects of the College Bound pledge and scholarship on high school and college outcomes, as specified in the legislative assignment. We report findings for some non-education outcomes—financial aid and criminal justice—in [Appendix I](#).

We focus on a student’s on-time progression (e.g., we measure whether a student completes high school or college “on time”). Given available data and the design of the CBS program, the evaluation is well-suited to address this progression (see [Exhibit 7](#) for an illustration of a student’s expected on-time progression). Because most students in our data who progress do so on time—more than 90% of students who graduate high school and more than 75% of students who enroll in college do so on time—this focus provides valuable information about the effects of CBS on progression for most students.

[High School Outcomes](#)

[On-Time High School Diploma Receipt](#)

We define on-time high school graduates as those who receive their diplomas by September 1 of their expected graduation year. Diploma receipt includes a regular high school diploma or a modified diploma received through an Individualized Education Program (IEP). We determine a student’s expected graduation date based on when he or she enters 9th grade. We do not consider students who transfer outside Washington’s public school system prior to their on-time high school graduation in the analysis of diploma receipt.

[High School \(12th Grade\) GPA](#). We use a student’s cumulative grade point average (GPA) for students at the end of 12th grade. Students who transfer outside Washington’s public school system or dropout prior to 12th grade are not included in this analysis.

[College Outcomes](#)

We examine the effects of pledge and CBS eligibility on college attainment (student progress through college), course taking (types of courses taken), and achievement (academic performance) at public universities and colleges.²¹

Only students enrolling in at least one college-level course will be considered enrolled or persisting. We do not consider students who enroll exclusively in basic skills, English as a Second Language, or “life-long learning” courses. Students who enroll in only non-college-level courses will be included as not enrolled or not persisting.

[College Enrollment](#). We define on-time enrollment as enrolling in a 2-year or 4-year program within one school year after completing high school on time. We consider students as enrolled even if they later withdraw for that year. For students who we do not observe completing high school on time, we measure on-time college enrollment relative to their expected, on-time high school graduation date. For this analysis, we only consider college enrollment occurring after high school. We do not consider students enrolled in concurrent programs as college enrollees unless they also enroll in college after high school. We include concurrent students as

²¹ A supplemental analysis using National Student Clearinghouse data will provide information about the effects of the program on enrollment and graduation at Washington private institutions and out-of-state institutions.

college graduates if they complete a college degree while in high school.

College Persistence. We measure college persistence as whether a student enrolls in two, three, or four consecutive years of college on time.²² Students must enroll in each year on time to be considered persisting for this analysis. We consider students who enroll at any point in the second, third, or fourth on-time year of college as enrolled for that respective year.

We generally do not differentiate re-enrollment status by institution type. Students who first enroll in a 2-year institution and re-enroll for an additional year at a 4-year institution are considered to persist in college and vice versa unless otherwise noted. Students who never enroll are defined as not persisting.

Credit Accumulation. We calculate the cumulative number of college credits earned one, two, three, and four years after a student's actual or expected on-time high school completion. We convert all credits to the quarter system where 45 credits equal one year of college. Credits are not disaggregated by institution type to allow for students who transfer from 2-year to 4-year institutions and vice versa.

We include only college-level courses in the credit calculations and exclude developmental course credits. We also only include college credits completed in college post-high school, meaning credits earned prior to high school completion (e.g., through Advanced Placement or Running Start) are not included in the total.

Students who never enroll in college have zero college credits.

College Completion. We consider on-time completion for 4-year degrees, i.e., completing a degree within four school years after on-time high school graduation. For example, if a student graduates high school on-time in the spring of 2012, we would consider a Bachelor's degree received by the 2015-16 school year to be on-time degree receipt. For 2-year degrees, we measure completion within two and three years of on-time high school completion. Students who do not attend college are included in these analyses as non-graduates.

College GPA. We focus on early achievement in college by measuring a student's cumulative GPA at the end of his or her first and second on-time years of college. We estimate college GPA at 2- and 4-year institutions separately. Students who never enroll in 2-year (4-year) institutions are not included in the analysis of 2-year (4-year) GPA. We focus on achievement in the first two years of college.

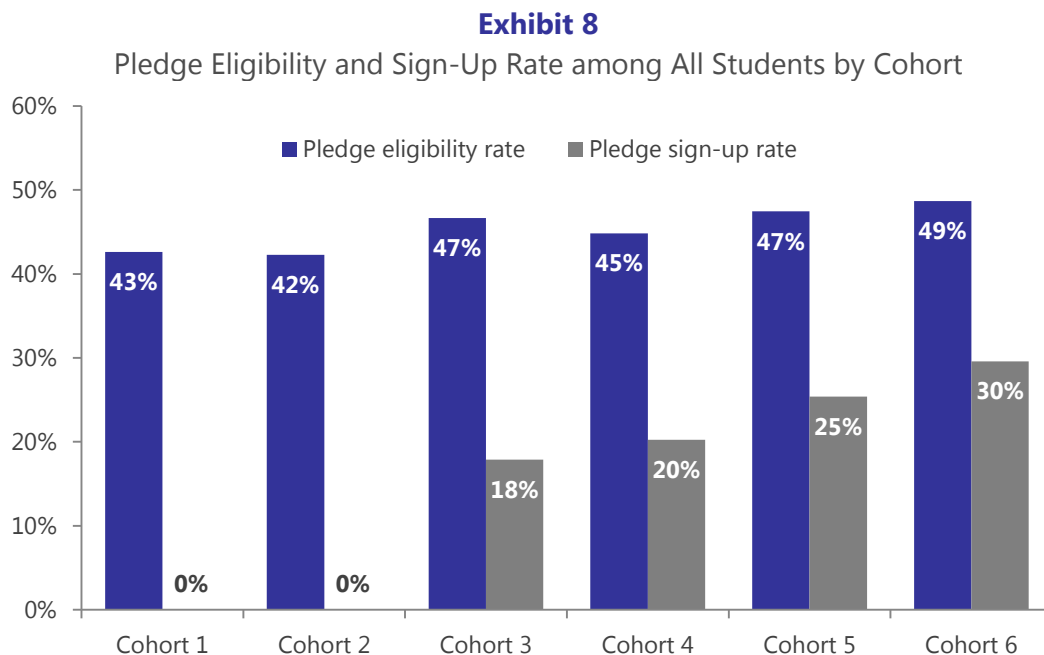
Developmental Course Taking. We consider a student enrolled in a developmental college course if he or she ever registers for these types of courses. We distinguish between math and English developmental courses. For most analyses, we do not differentiate between developmental course taking at 2-year or 4-year schools unless otherwise noted. Students who never attend college are included in the analyses as never taking a developmental course.

²² For all outcomes measured in the fourth year, we primarily consider students from Cohorts One through Three. Students who enter college earlier in Cohorts Four through Six may be included.

III. Description of Pledge Eligibility and Sign-Up Rates

This section provides background on the rates of pledge eligibility and sign-up for our analytic sample, which includes all students observed in 7th and 8th grade who had no missing student characteristic data. Because our main analysis focuses on the effects of pledge signing, we also provide detail on the characteristics of different types of pledge signers. Description of pledge-eligible students before and after CBS implementation and pledge signers is included in [Exhibits A9, A10, and A11](#) in [Appendix II](#).

About 48% of students in our post-implementation cohorts were eligible to sign the pledge. About 22% of all pledge-eligible and ineligible students in those cohorts did so. [Exhibit 8](#) displays eligibility and sign-up rates for each cohort in our sample. Between 42% and 49% of students in each cohort were pledge eligible. Rates of pledge eligibility rose from 42% to 47% between our second and third cohorts.



The increase in pledge-eligible students between Cohorts Two and Three partially has to do with the way we define our cohorts. Specifically, students who are retained a grade may end up in Cohort Three even if they started in an earlier cohort.²³

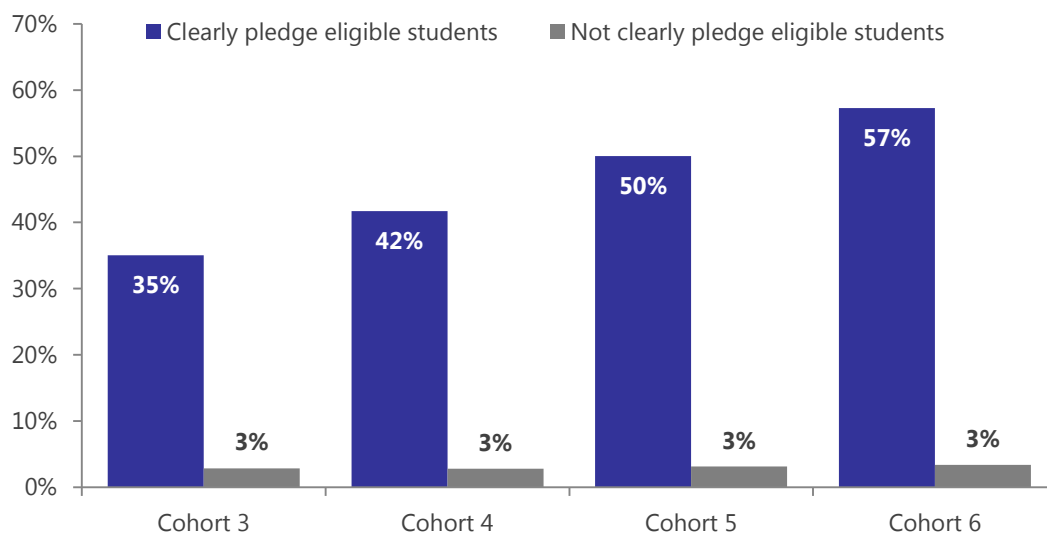
An alternative definition would define cohorts based on each student's starting cohort. We explore the relationship between this alternative cohort definition and our results in [Appendix II](#). In either case, our analysis accounts for changes in student characteristics around the time of implementation.

Our third cohort—the class of 2012 if progressing on time—was the first in our sample to contain students who could sign the College Bound pledge. Of the students in Cohort Three who received free- or reduced-price lunch and were therefore clearly eligible to sign the pledge, about 35% did so ([Exhibit 9](#)). The sign-up rate increased in each cohort and more than 50% of eligible students in Cohort Six signed the pledge.

WSAC has reported a continued increase in the pledge sign-up rate during the years outside our sample.²⁴ The sign-up rate among eligible students is about 71% for the high school class of 2021. According to WSAC, sign-up rates vary widely between school districts. We account for this variation in our analyses.

Exhibit 9

Pledge Sign-Up Rates for Clearly Pledge-Eligible and Ineligible Students



²³ For example, a student who is in 7th grade in 2005-06 would be in Cohort One. If that student were retained, then she would be in 7th grade in 2006-07. If the student then progressed normally, she would end up in 8th grade in 2007-08, putting her in Cohort Three. Similarly, if that student were in 8th grade in 2005-06 and then retained, she would be in 8th grade in 2006-07 and 9th grade in 2007-08 and still be in Cohort Three.

²⁴ WSAC. [College Bound](#).

Overall, in the post-period cohorts, 43% of clearly pledge-eligible students signed the pledge. About 3% of students who did not receive FRL during their eligibility grades signed the pledge. The disparity suggests FRL status is a valid, but imperfect, proxy for eligibility to sign the College Bound pledge.

We are aware of two reasons students who are seemingly ineligible may have been able to sign the pledge.

First, our measure of eligibility does not capture students who did not receive meal assistance. Some of these students were indeed income eligible to sign the pledge but we could not determine their eligibility based on our data. This reason probably explains most of the discrepancy between our eligibility definition and the pledge sign-up rate.

Second, without a reliable way to determine parental income, WSAC cannot verify income eligibility. Some students may have signed the pledge despite lacking eligibility.

We account for this incongruity between pledge eligibility and pledge signing when we evaluate the effect of signing the pledge.

Characteristics of Pledge-Eligible Students and Pledge Signers

Student characteristics varied between eligible students who signed the College Bound pledge, eligible students who did not sign the pledge, and students who signed the pledge who were not clearly pledge eligible based on their FRL status.

In terms of the factors associated with greater educational attainment, pledge-eligible students who signed the pledge were more advantaged than eligible students who did not sign it. They had higher 8th-grade standardized test scores and less involvement in the criminal justice system.

These groups differed in self-identified gender composition, as well as self-identified racial and ethnic composition. Prominently, eligible students who signed the pledge were more likely to be female and Hispanic and less likely to be White.

Pledge-eligible students more generally tended to be less advantaged than students who are ineligible in ways that might hinder educational attainment (see [Exhibit A10](#) in [Appendix II](#)). Eligible students had lower 8th-grade standardized test scores and higher levels of criminal charges and adjudications.

We account for many of these differences, including standardized test scores and criminal justice involvement, in our analysis.

Characteristics for pledge-eligible and pledge-signing students included in our analysis are provided in [Exhibits A9](#) and [A10](#) in [Appendix II](#).

IV. Effects of Pledge Eligibility and Pledge Signing

In this section, we report the estimated effects of the College Bound pledge on education outcomes at public institutions. We describe the effects of pledge eligibility on student outcomes, as well as the effects of signing the pledge. We present the full results in [Exhibit 12](#).

In [Exhibit 12](#), the numbers in the “effect” columns indicate how we estimate pledge eligibility affects pledge-eligible students, and how we estimate pledge signing affects pledge signers. For example, in the row designated by the “Cumulative GPA in 12th grade” outcome, the value in the effect column for pledge signing is -0.091. This suggests that signing the pledge in middle school, on average, reduces students’ 12th grade GPAs by 0.091 grade points.

As discussed in [Section II](#), we compare the change in outcomes for the treatment and comparison groups before and after CBS implementation. We focus on education outcomes and report effects of the College Bound pledge on criminal justice and financial aid outcomes in [Appendix I](#).

Technical details and sensitivity analyses can be found in [Appendix II](#).

High School Outcomes

As discussed previously, Goldhaber, et al. (2017) completed an evaluation of pledge eligibility on high school outcomes. They found that students who are eligible to sign the pledge are no more likely to graduate high school than ineligible students. They also found that eligibility had a small negative effect of a student’s cumulative 12th grade GPA. Our findings are consistent with their results ([Exhibit 12](#)). With respect to pledge signing, we find that students who sign the pledge are no more likely to complete high school on time. We also find that, on average, students who sign the pledge have lower cumulative GPAs in 12th grade by 0.091 grade points.²⁵

College Outcomes

In general, we find that pledge eligibility and signing have little effect on college attainment. We find that pledge eligibility and signing may reduce achievement in college.

College Attainment. For enrollment, we disaggregate the effects by cohort to illustrate how the effects of pledge eligibility change across cohorts over time. [Exhibits 10](#) and [11](#) show regression-adjusted on-time 2-year college enrollment and on-time 4-year college enrollment, respectively. The exhibits illustrate enrollment rates for all pledge-eligible and ineligible students,

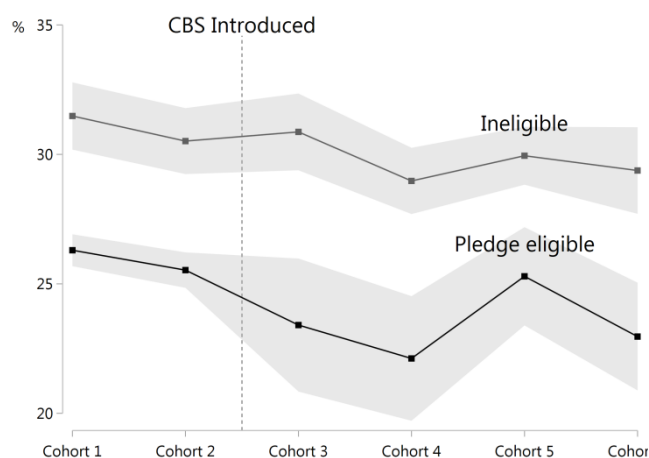
²⁵ One potential explanation for the observed reduction in GPA is that signing the pledge may lead students to take more rigorous courses such as college preparatory courses. In this case, students who sign the pledge could have lower GPAs because of the types of courses they take rather than their performance in those courses.

regardless of whether they signed the pledge. The trends do not portray visible gains in enrollment for pledge-eligible students relative to other students.

Exhibit 10 shows a drop in on-time enrollment after CBS implementation for Cohorts Three and Four. The enrollment rates increase for Cohort Five, more than the increase for ineligible students before dropping again for Cohort Six. Over the whole observation period, the difference in enrollment rates between pledge-eligible and ineligible students is about the same before and after CBS implementation, which is reflected in the fact that we find no effect of pledge eligibility on enrollment in a 2-year college.

Exhibit 10

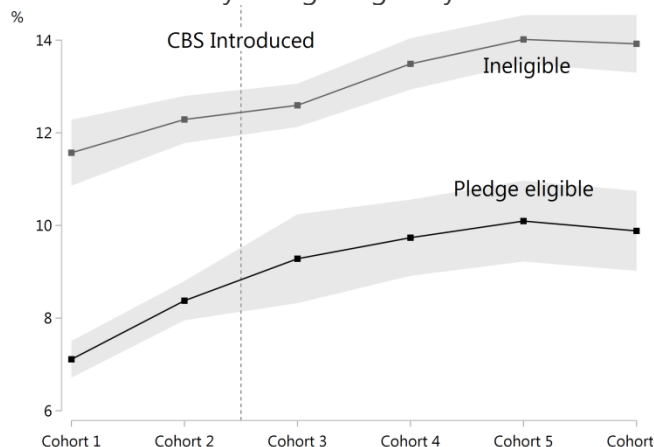
2-Year College On-Time Enrollment Rate, by Pledge Eligibility



For all cohorts, we generally find no effect of eligibility or signing on enrollment in, or graduation from, a public institution,

Exhibit 11

4-year College On-Time Enrollment Rate, by Pledge Eligibility



although evidence points toward a small effect on 4-year enrollment.²⁶ Similarly, we did not find an increase in college credit accumulation or persistence that we could attribute to pledge eligibility or signing.

In contrast, our findings suggest that pledge eligibility may decrease the average number of college credits students earn two years after completing high school by 0.68 credits, while pledge signing may reduce credits earned by an average of two credits.²⁷ The reduction in credits fades away by the third year.

²⁶ More than other outcomes, findings for 4-year college enrollment are sensitive to our model specification and sample definitions. We report the results from our preferred model that shows only marginal evidence of an effect, but we do find positive effects of eligibility on 4-year enrollment in many of our robustness checks (see Appendix II).

²⁷ When we use the alternative cohort and eligibility definitions described in Appendix II, we find no effect on second year credits earned. Using the alternative definitions, we find signing the pledge increases credits earned through a student's fourth year of college by 4.7.

College Achievement. For students at 4-year colleges, we find that students who were eligible to sign the have lower first-year GPA by 0.06 grade points. We find that 4-year college students who signed the College Bound pledge while in middle school have lower GPA by about 0.1 grade in the first year of college.²⁸ The first-year effects for pledge-eligible students and pledge signers subsequently fade away for those students who persist to a second year of college.

We find that students who are eligible to sign the pledge and attend 2-year colleges have lower first-year GPAs by 0.04 grade points and second-year GPAs by 0.05 grade points, on average. We also find average GPA reductions of 0.09 and 0.12 grade points for pledge-signing students during their first and second years, respectively, at 2-year colleges.²⁹

College Course Taking. We find no evidence that pledge eligibility or sign-up affect receipt of developmental math or English credit.

²⁸ As with the effects on cumulative 12th grade GPA, students who sign the pledge may have lower GPAs because the signing the pledge leads them to take change their course-taking behavior (e.g. they may take more STEM courses) rather than affect performance in their courses.

²⁹ When we use the alternative cohort and eligibility definitions described in Appendix II, we find no effect on first-year GPA for students at 2-year colleges.

Exhibit 12
Effects of Pledge Eligibility and Pledge Signing

Outcome	Pledge eligibility			Pledge signing		
	Effect	SE	N	Effect	SE	N
High school						
Cumulative GPA at end of 12 th grade	-0.036**	0.011	366,630	-0.091**	0.029	366,630
Proportion completing high school on time	-0.001	0.006	402,045	-0.002	0.016	402,045
Enrollment						
Proportion enrolling in any college on time	-0.003	0.005	432,083	-0.007	0.015	432,083
Proportion enrolling in 2-year college on time	-0.008	0.006	432,083	-0.021	0.015	432,083
Proportion enrolling in 4-year college on time	0.005	0.003	432,083	0.014	0.008	432,083
Credits earned						
Cumulative credit hours earned one year after high school completion	-0.086	0.178	426,844	-0.235	0.484	426,844
Cumulative credit hours earned two years after high school completion	-0.677*	0.306	355,364	-2.033*	0.916	355,364
Cumulative credit hours earned three years after high school completion	-0.750	0.435	284,975	-2.530	1.466	284,975
Cumulative credit hours earned four years after high school completion	0.003	0.621	213,371	0.010	2.262	213,371
Persistence						
Proportion enrolling in two consecutive years of college	-0.003	0.005	358,108	-0.010	0.016	358,108
Proportion enrolling in three consecutive years of college	-0.005	0.005	286,832	-0.015	0.017	286,832
Proportion enrolling in consecutive years of college	-0.002	0.005	214,445	-0.009	0.018	214,445
Graduation						
Proportion who graduated with 2-year degree within two years of on-time high school completion [^]	-	-	-	-	-	-
Proportion who graduated with 2-year degree within three years of on-time high school completion	-0.006	0.004	236,015	-0.018	0.011	236,015
Proportion who graduated with 4-year degree within four years of on-time high school completion	-0.001	0.004	172,202	-0.004	0.013	172,202
Course taking and achievement						
Proportion who ever take a remedial math course in college	-0.002	0.006	432,083	-0.006	0.015	432,083
Proportion who ever take a remedial English course in college	-0.006	0.005	432,083	-0.017	0.014	432,083
Cumulative GPA at end of 1 st year of college (2-year college)	-0.041*	0.021	114,199	-0.093*	0.046	114,199
Cumulative GPA at end of 2 nd year of college (2-year college)	-0.051*	0.022	69,472	-0.119*	0.053	69,472
Cumulative GPA at end of 1 st year of college (4-year college)	-0.058*	0.023	67,916	-0.102*	0.041	67,916
Cumulative GPA at end of 2 nd year of college (4-year college)	-0.016	0.023	48,754	-0.030	0.042	48,754

Notes:

* p<0.05, ** p<0.01, *** p<0.001

The number of asterisks next to each effect estimate indicates the level of confidence we ascribe to the effect. We can be reasonably confident that effect estimates with at least one asterisk are real effects. If an effect estimate has no asterisks, it means we cannot statistically distinguish the “true” effect from zero; i.e., the effect may, in fact, be zero.

[^] We exclude results for this outcome because its model failed an important statistical test. See Appendix II for discussion of this test.

V. Effects of Scholarship Eligibility

This section reports the effects of CBS eligibility prior to college enrollment on student outcomes. We report these effects for our full sample of CBS-eligible students and by student GPA to demonstrate how effects differ by a student's high school GPA.

As discussed in the evaluation methodology ([Section II](#)), we compare CBS-eligible students (those who signed the pledge in middle school and met all requirements of the pledge at the end of high school) to similar students who were not eligible to receive CBS because CBS was not yet implemented. As a reminder, because we cannot observe income at the time of high school completion, we use FRL status a proxy for family income and limit our analysis to FRL recipients in 12th grade.

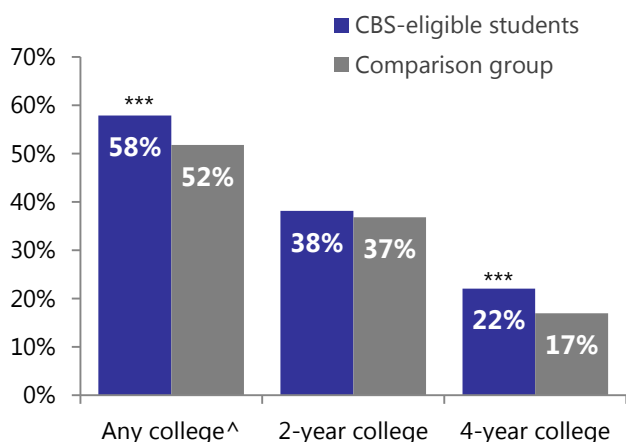
We present our full results of the effects of CBS eligibility on college attainment and achievement outcomes in [Exhibit 19](#). Additional information, including alternative specifications and sensitivity analyses, can be found in [Appendix III](#). Effects of CBS eligibility on financial aid outcomes are available in [Appendix I](#). [Appendix III](#) also includes the full set of results for effects separated by student high school GPA.

[Exhibit A16](#) (in [Appendix III](#)) presents the characteristics of our full sample before and after using our matching procedure. Our sample of scholarship eligible students includes 12,953 students in the third and fourth cohorts. The comparison group pool includes 20,252 students in the first and second cohorts who would have been eligible had the CBS scholarship been available to them. Before matching, scholarship-eligible students had higher 10th and 12th grade GPAs, were less likely to be White, and were less likely to speak English as their primary language. After our propensity score matching process, both the scholarship eligible and comparison group contained 12,028 students. Significant differences in the sample and comparison groups did not persist post-match. We provide characteristics for students by GPA category after matching in [Appendix III](#).

College Enrollment. Eligibility for the scholarship increases the probability of on-time enrollment by 6.1 percentage points. The enrollment rate went from 52% in the comparison group to 58.1% among CBS-eligible students (see [Exhibit 19](#)). This effect in the overall CBS-eligible population appears to be driven by a 5.1 percentage point increase in the probability of enrolling in a 4-year college ([Exhibit 13](#)).

Exhibit 13

Percent Enrolling in Any, 2-, or 4-Year College



Notes:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

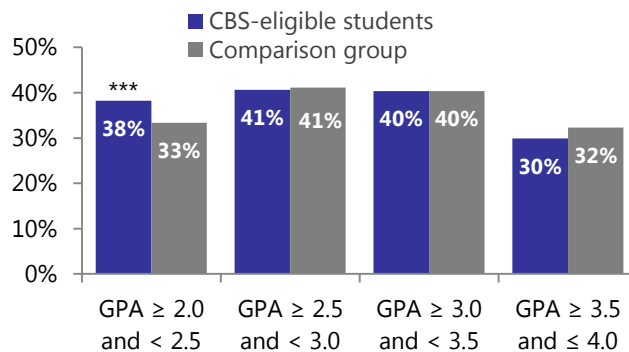
The number of asterisks indicates the level of confidence we ascribe to the effect. We can be reasonably confident that effect estimates with at least one asterisk are real effects. If an effect estimate has no asterisks, it means we cannot statistically distinguish the “true” effect from zero.

^a Sum of 2-year and 4-year enrollment is greater than any enrollment because students who enroll in both 2-year and 4-year institutions over time are included in both groups.

[Exhibits 14 and 15](#) display the results by GPA subgroup. CBS eligibility leads to a 4.9 percentage point enrollment increase at 2-year institutions for students who graduated with a high school GPA between 2.0 and 2.5. That effect declines as GPA increases, and we do not find evidence of an effect on 2-year college enrollment for students who graduate with higher GPAs. On the other hand, the largest effect on 4-year college

Exhibit 14

2-Year Enrollment, by High School GPA

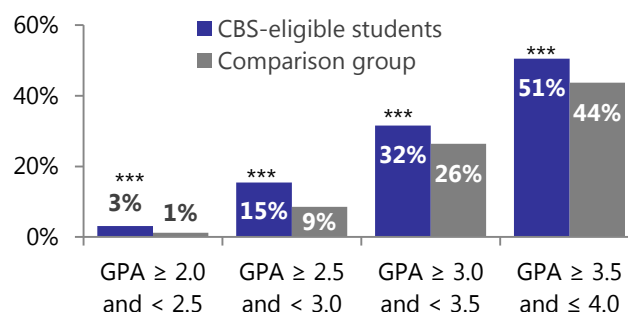


Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Exhibit 15

4-Year Enrollment by High School GPA



Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

enrollment occurs for students who graduate with GPAs between 3.5 and 4.0.

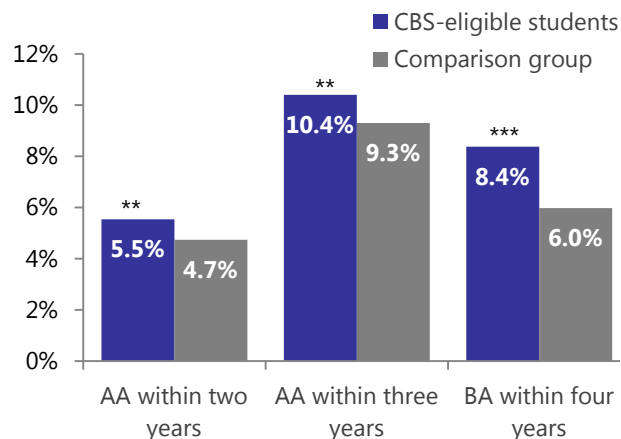
College Persistence. We find evidence that CBS eligibility increases whether a student enrolls continuously in two, three, or four years of college by 3 to 5 percentage points ([Exhibit 19](#)). We also find that these positive effects persist across GPA categories ([Exhibit A18 in Appendix III](#)).

Credit Accumulation. CBS-eligible students earn more college credits than similar ineligible students ([Exhibit 19](#)). Four years after high school completion, CBS-eligible

students have almost six additional credits earned, or more than one additional course. The estimated credit hour increases are largest for those who graduate high school with a GPA between 3.5 and 4.0 (Exhibit A17 in Appendix III).

Exhibit 16

Graduation, by Degree Type



Notes:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

AA refers to 2-year degrees and BA to 4-year degrees.

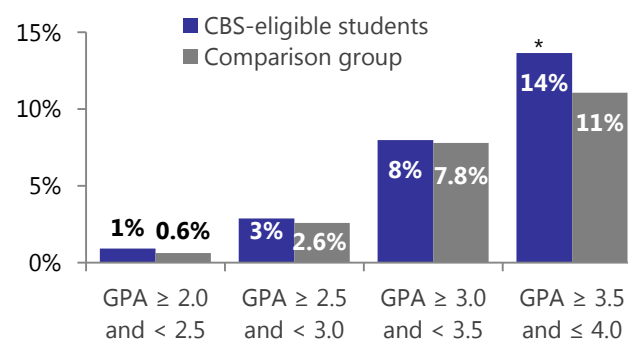
College Completion. We find positive effects of CBS eligibility on college graduation (Exhibit 16). We find a 0.8 percentage point increase (a 15% relative increase from 4.7% to 5.5%) in the probability that someone eligible for the scholarship will graduate with a 2-year degree within two years of on-time high school graduation. Similarly, we find a 1.1 percentage point increase (a 12% relative increase) in the proportion of CBS-eligible students who have a 2-year degree within three years of high school graduation. We estimate a 2.4 percentage point increase (a 40% relative increase) in the likelihood that CBS-eligible students will have a 4-year degree within four years of on-time high school graduation.³⁰

³⁰ Our data goes through the 2015-16 school year providing at most four academic years after high school graduation, if progressing on time. Thus, we ignore the requirement to use

Effects on 2-year and 4-year degree receipt appear to be driven by students in the higher GPA categories (Exhibits 17 and 18). For example, for those who graduate high school with a GPA above 3.5, we observe an 8.1 percentage point increase in the probability of graduation from a 4-year institution (Exhibit 18), larger than for any other GPA category.

Exhibit 17

Graduation from 2-Year School Within Two Years, by High School GPA

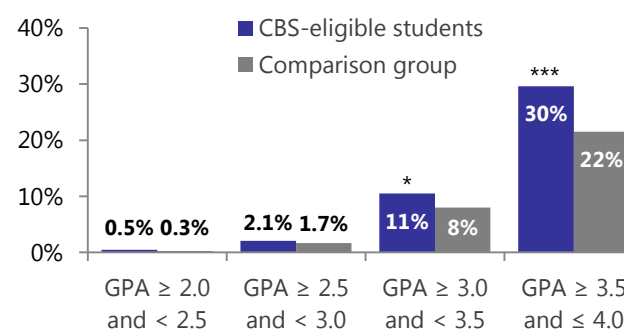


Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Exhibit 18

Graduation from 4-Year School Within Four Years, by High School GPA



Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the scholarship within five academic years of high school graduation.

Developmental Course Taking. We find no evidence indicating CBS eligibility has an effect on the rate of remedial course taking.

College GPA. We find no evidence indicating CBS eligibility has an effect on college GPA in the first two years at either 2-year or 4-year colleges.

Exhibit 19
Effects of CBS Eligibility

Outcome	Comparison group mean	Effect	SE	N
Enrollment				
Proportion enrolling in any college on time	0.52	0.061***	0.008	24,056
Proportion enrolling in 2-year college on time	0.37	0.013	0.007	24,056
Proportion enrolling in 4-year college on time	0.17	0.051***	0.006	24,056
Credits earned				
Cumulative credit hours earned one year after high school completion	13.31	2.067***	0.243	24,056
Cumulative credit hours earned two years after high school completion	25.52	3.544***	0.456	24,056
Cumulative credit hours earned three years after high school completion	32.58	4.900***	0.661	24,056
Cumulative credit hours earned four years after high school completion	34.80	5.969***	1.039	18,094 [^]
Persistence				
Proportion enrolling in two consecutive years of college	0.40	0.045***	0.007	24,056
Proportion enrolling in three consecutive years of college	0.30	0.033***	0.006	24,056
Proportion enrolling in four consecutive years of college	0.21	0.031***	0.007	18,144 [^]
Graduation				
Proportion who graduated with 2-year degree within two years of on-time HS completion	0.05	0.008**	0.003	23,886
Proportion who graduated with 2-year degree within three years of on-time HS completion	0.09	0.011**	0.004	23,635
Proportion who graduated with 4-year degree within four years of on-time HS completion	0.06	0.024***	0.005	17,760 [^]
Course taking and achievement				
Proportion who ever take a remedial math course in college	0.31	-0.008	0.006	24,056
Proportion who ever take a remedial English course in college	0.18	-0.005	0.006	24,056
GPA at end of 1 st year of college (2-year college)	2.51	0.006	0.020	8,623 ^{^^}
GPA at end of 2 nd year of college (2-year college)	2.66	0.024	0.019	6,204 ^{^^}
GPA at end of 1 st year of college (4-year college)	2.59	-0.026	0.030	4,715 ^{^^}
GPA at end of 2 nd year of college (4-year college)	2.68	0.036	0.026	4,002 ^{^^}

Notes:

* p<0.05, ** p<0.01, *** p<0.001

[^] Sample sizes are reduced because we only observe four years of college for the first CBS cohort.

^{^^} Sample sizes are reduced because these outcomes are conditional on enrolling in the first or second year of college.

VI. Effects of Scholarship Receipt

WSIPP's assignment included direction to evaluate outcomes for CBS recipients compared to their peers. As discussed in [Section III](#), we use propensity score matching to evaluate the effects of CBS receipt. The treatment group includes those receiving CBS dollars in their first on-time year of college. The comparison group consists of those receiving need-based aid in their first on-time year of college (in the pre-period). Findings from this analysis illustrate whether receiving CBS dollars, which generally provides students with more aid, affects college attainment, course taking, and achievement.

We first present a summary of our findings for all college students (detailed results are included in [Appendix III](#)). We then disaggregate our findings into 2-year and 4-year effects. Students who attend both 2-year and 4-year institutions in their first on-time year of college are included in both analyses.³¹

Some limitations restricted our analysis for CBS recipients. As illustrated in the previous section, CBS eligibility increases the likelihood that a student enrolls in a Washington public university. Because CBS eligibility causes some students to attend college, particularly at 4-year colleges, we are more likely to observe students in the CBS recipient group who were unlikely to enroll in an in-state public university without CBS. For these students, the most similar match may be a student who did not enroll

in a public university at all. However, we must limit the comparison group pool to students enrolled in a public university due to data availability. Consequently, we may not have the most suitable matches in our comparison group.

Because of this limitation, we may not be able to conclude that CBS receipt *causes* observed differences in outcomes between CBS recipients and the matched comparison group, mostly for 4-year college students.

Effects of CBS Receipt for All College Students

Overall, we find that CBS receipt positively affects educational attainment. Compared to need-based aid students who do not receive CBS, college students who receive CBS have more cumulative credits earned across all years. CBS recipients are more likely to enroll in four consecutive years of college, although we find no effect on the likelihood of enrolling in two or three years of college. CBS receipt also increases the likelihood that students receive a 4-year degree. Finally, we find that CBS receipt reduces the likelihood that students take developmental English and math courses and that CBS receipt reduces GPA in the first year of college by about 0.04 grade points but has no effect in the second year. Full results for all college students are included in [Exhibit A29 in Appendix III](#).

We next separate findings for 2-year and 4-year students because students may differ by the type of institution attended. Furthermore, tuition and the CBS award amount differs significantly by institution type.

³¹ About 8% of the matched sample for 2-year college students and 11% of the matched sample for 4-year college students enrolled in both 2-year and 4-year programs during their first on-time year of college.

Effects of CBS Receipt for 2-year Students

Exhibit A27 (in **Appendix III**) reports the means and proportions for all variables used in the analysis for the treatment and comparison groups before and after matching for students who first enroll in 2-year colleges. Prior to matching, CBS recipients differ from students in the comparison group in various ways. They are less likely to be White and are more likely to have higher 8th-grade test scores and are less likely to be FRL in earlier grades. They are more likely to be FRL in later grades, however. After matching, the treatment and comparison groups are very similar on all observed characteristics.³²

CBS receipt has varying effects on attainment and achievement for students at 2-year institutions. We generally find that CBS receipt increases numerous measures of attainment while having little effect on achievement.

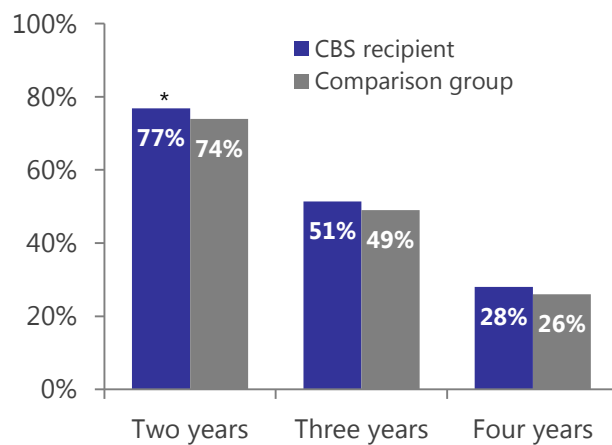
College Persistence. CBS recipients are more likely to enroll in two years of college than similar need-based aid recipients but are equally likely to have enrolled in three and four consecutive years of college (**Exhibit 20**).

Credit Accumulation. Students receiving CBS in their first year of college at 2-year institutions have greater credit accumulation throughout college. Four years after high school completion, students who received CBS in their first year have almost five more credits than similar students who do not receive CBS—the equivalent of one additional college course (**Exhibit 21**).

³² The matched sample excludes 7% of the treatment group due to no suitable matches. Prior to matching, there are 4,052 CBS recipients; after matching, there are 3,706.

Exhibit 20

Percent Enrolling in Two, Three, or Four Consecutive Years of College



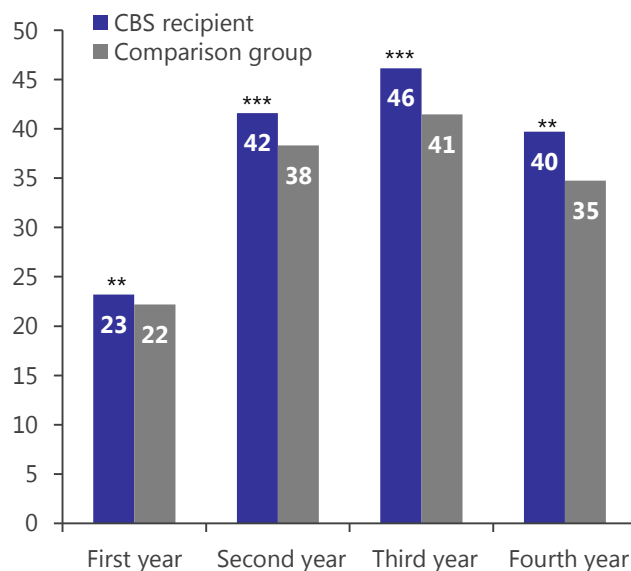
Notes:

* p<0.05, ** p<0.01, *** p<0.001

The number of asterisks indicates the level of confidence we ascribe to the effect. We can be reasonably confident that effect estimates with at least one asterisk are real effects. If an effect estimate has no asterisks, it means we cannot statistically distinguish the “true” effect from zero.

Exhibit 21

Cumulative Credits Earned in Each Year After High School Completion

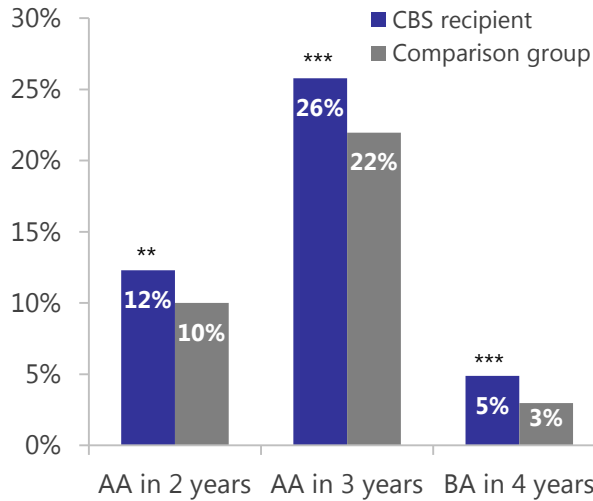


Note:

* p<0.05, ** p<0.01, *** p<0.001

Exhibit 22

Percent Completing Degree



Notes:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

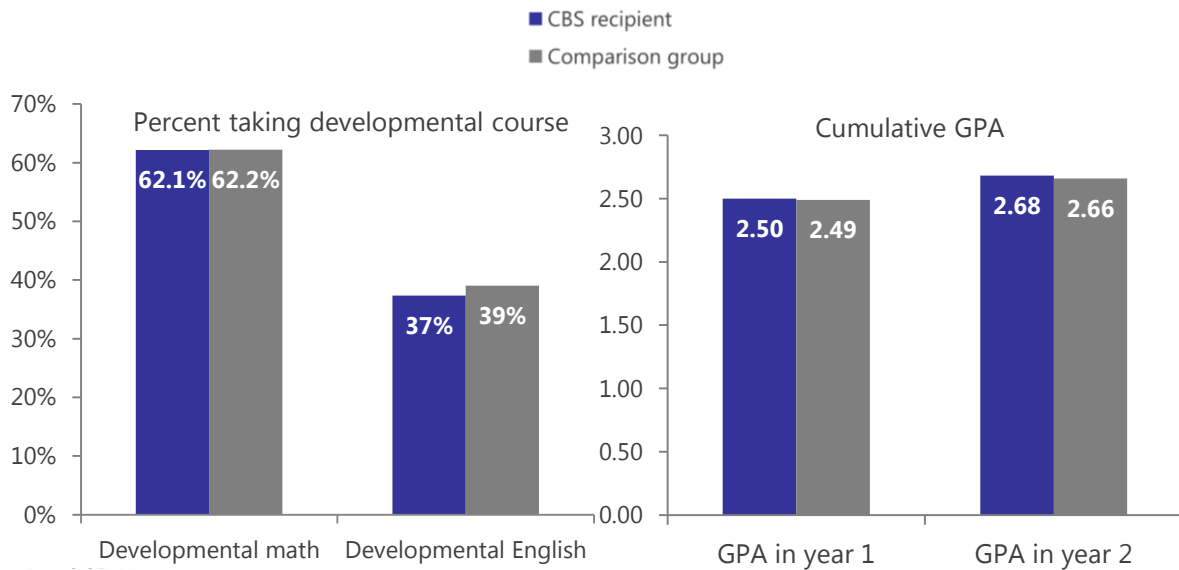
AA refers to 2-year degrees and BA to 4-year degrees.

College Completion. We find that CBS recipients first enrolling at 2-year institutions are more likely to complete 2-year degrees in two and three years and 4-year degrees in four years (from on-time high school completion) than similar need-based aid recipients (Exhibit 22).

College Course Taking and Achievement. Our results indicate that CBS receipt has no effect on participation in developmental math or reading courses or on GPA at 2-year institutions (Exhibit 23).

Exhibit 23

Effects on Course Taking and Achievement (2-Year)



Notes:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We include developmental courses at 2-year institutions only. GPA is calculated at 2-year institutions only.

We include developmental course credits taken at 2-year institutions only. GPA is calculated at 2-year institutions only.

Effects of CBS Receipt for 4-year Students

Exhibit A28 (in Appendix III) reports the means and proportions for all variables used in the analysis for the treatment and comparison groups before and after matching for students who enroll in 4-year colleges. CBS recipients who enrolled in 4-year institutions differ from the comparison group in various ways. Recipients are less likely to be White, have lower math scores in 8th grade, and are less likely to receive FRL in earlier grades but are more likely to receive FRL in later grades. Finally, recipients also have higher family incomes as a percent of the state MFI in their first year of college (31% among recipients compared to 27% in the comparison group). After matching, the treatment and comparison groups are very similar on all observed characteristics.³³

Overall, CBS receipt tends to have either no relationship or a negative relationship with outcomes for students who first enroll at 4-year institutions.

CBS recipients are less likely to enroll in two or three consecutive years of college and have fewer credits earned one year after high school completion, but we find no relationship in other years (Exhibit 24).³⁴

³³ With the matched sample, we exclude 860 students (25%) from the treatment group due to no suitable matches in the comparison pool. This exclusion reduces the treatment group to 2,403 down from a 3,263. Excluded students have similar characteristics to included students except with respect to income. Excluded students have a higher family income as a percent of the state MFI, although they are more likely to receive FRL in high school. We conduct a sensitivity analysis where we do not exclude these students and results are generally similar (see Appendix III).

³⁴ For students first enrolling at a 4-year institution, we consider persistence at 4-year institutions only.

Similarly, we find little evidence that CBS receipt is associated with BA degree receipt (Exhibit 25), course taking, or college achievement (Exhibit 26).³⁵

The findings for 4-year college students warrant further discussion. Importantly, as discussed in Section IV, we find that CBS eligibility increases on-time enrollment at 4-year public colleges in Washington. Because CBS eligibility causes some students to enroll in public 4-year schools in Washington, we know that some CBS recipients in 4-year colleges are “new” students who would not have otherwise enrolled in an in-state public 4-year institution. These new students may drive the findings presented in this section.

Without CBS, these students may have enrolled in a 2-year institution, a private school in Washington, an out-of-state school, or they may not have enrolled in college at all. When we limit our analysis to 4-year college students only, none of these new students will be in our comparison group because it consists of need-based aid recipients already attending public 4-year colleges. Thus, even when we compare CBS recipients to similar students who do not receive CBS based on observed characteristics, it is possible that unobserved differences will remain, and thus, the results presented in this section may not be causal.

Of particular concern for the analysis of 4-year college receipt are those students who would have enrolled in a 2-year college or not have enrolled in college at all without CBS. We might expect these students to perform less well than students who would

³⁵ For students first enrolling at a 4-year institution, we consider developmental course taking at 4-year institutions only.

have attended a 4-year college even without CBS.³⁶ These students may lead us to underestimate the effects of CBS receipt.

We attempt to determine whether these new students may lead us to underestimate the effects of CBS with two additional analyses. First, as discussed above and in [Appendix III](#), we combine 2-year and 4-year students and perform the same analysis comparing CBS recipients at any public institution to need-based aid recipients prior to the existence of the CBS scholarship. If the new students are students who would have been enrolled in 2-year institutions in the pre-period, they could be in our matched comparison group, which may mitigate some potential unobserved differences.

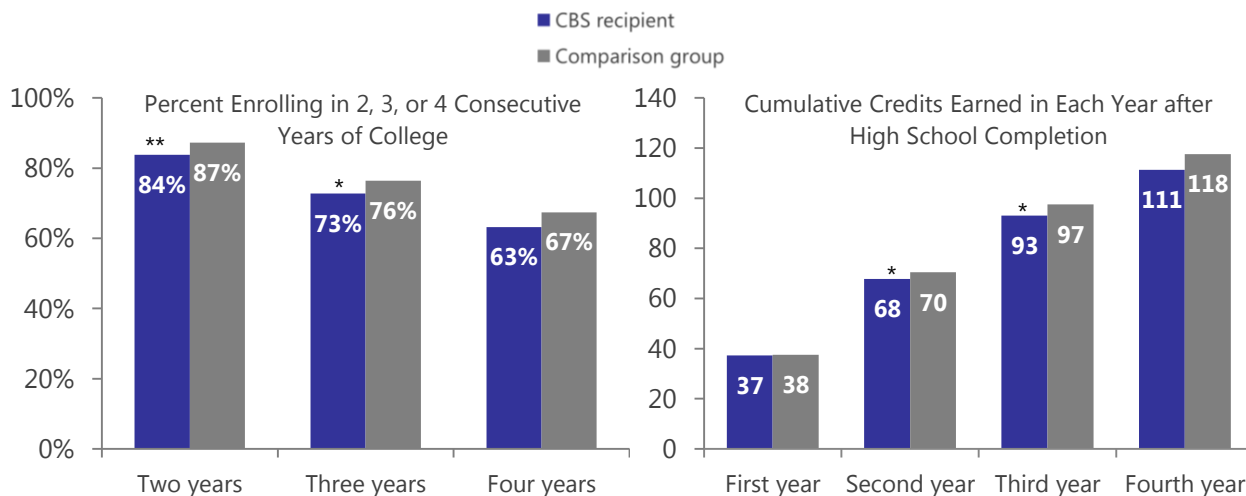
Second, we perform various sensitivity analyses to explore what the effects of CBS receipt might be if we exclude these new students (see [Appendix III](#) for the results of these analyses).

In general, unlike the findings for 4-year students overall, these additional analyses show that CBS receipt has either no effect or positive effects on college outcomes.

These findings suggest that the new students who enter 4-year colleges because of CBS may differ from students who would have entered 4-year colleges regardless of CBS receipt. These new students have greater educational attainment at 4-year institutions than similar students who do not enroll in 4-year schools at all, but they

Exhibit 24

Effects on Persistence and Credit Accumulation in Each Year of College



Notes:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We measure the percent enrolling in two, three, and four consecutive years at 4-year institutions only.

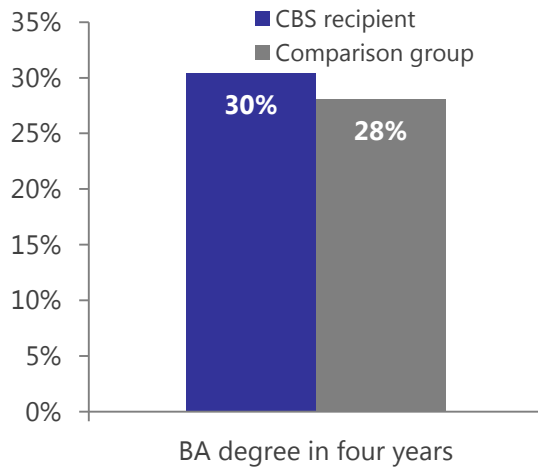
³⁶ Students who would have attended private or out-of-state schools could also cause bias in our results, but we might expect these students to lead to an overestimate of the effects of CBS receipt.

may not obtain the same number of credits or years of college as CBS recipients who would have enrolled even without CBS. Thus, the negative or non-existent relationships we observe between CBS

receipt and educational attainment for 4-year students seem at least partially due to students who would not have enrolled in college without CBS and may not reflect the effects of CBS receipt for all 4-year students.

Exhibit 25

Percent Earning an On-Time BA Degree

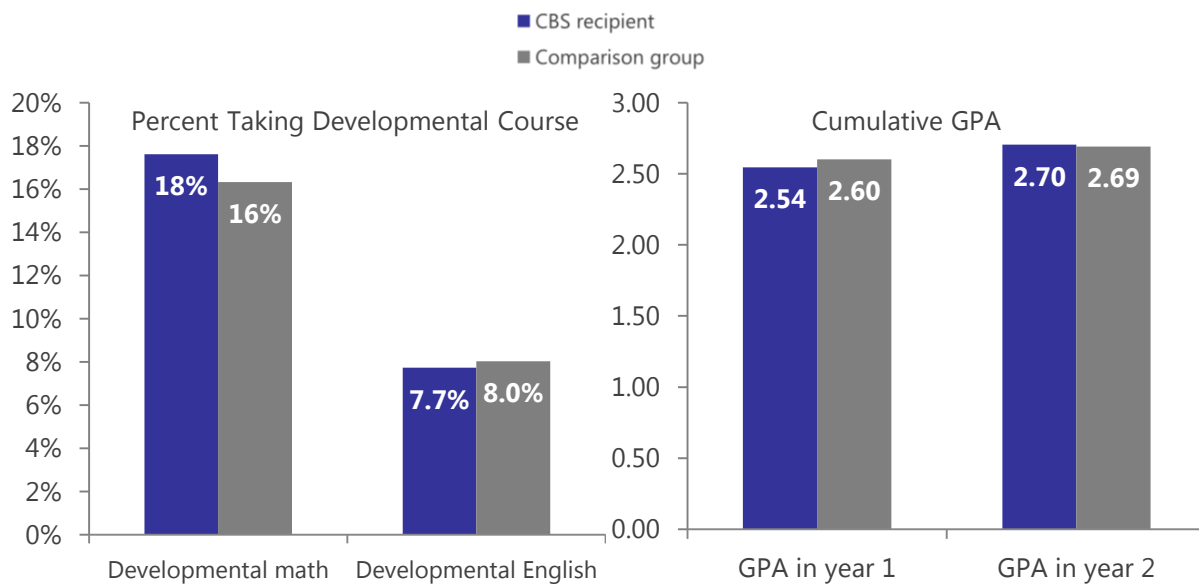


Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Exhibit 26

Effects on Course Taking and Achievement in College (4-Year)



Notes:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We include developmental courses at 4-year institutions only.

GPA is calculated at 4-year institutions only.

VII. Limitations

This analysis illustrates the varying effects that the College Bound pledge and scholarship have on education outcomes. However, several important limitations warrant further discussion.

First, as discussed, data availability prevents us from identifying all pledge-eligible students. We rely on FRL receipt to determine a student's eligibility to sign the pledge. Analysis by Goldhaber et al. (2017) suggests that using FRL receipt would capture about 87% of all eligible students.³⁷ They further show that, depending on how these unobserved pledge-eligible students respond to the pledge, estimates could fall anywhere between 0.87 and 1.10 times the effects reported here. While we could under- or overestimate the effects of the pledge on student outcomes, the range is relatively small.

Second, we also could not obtain data on income for all students or all students who filed a FAFSA. Family income is an important criterion for determining whether a student is eligible to receive CBS. Lacking family income data on all students restricts our ability to employ our preferred research design that can greatly reduce bias from unobserved factors. Although propensity score matching has many advantages, our preferred research design would require family income data for a larger sample of students beyond those already enrolled in college.

Additionally, without family income for students at the time of high school completion, we cannot entirely identify students whose family incomes fall at or below 65% of the state MFI. We use FRL receipt in 12th grade as a proxy for student income, but this limits our analysis to a subset of potentially eligible students, and we may misidentify some students using this proxy. Nonetheless, we believe FRL is a reasonable proxy and allows us to provide effects for a large portion of students whose family incomes would probably make them eligible to receive CBS.

Furthermore, we can only examine the effects of CBS receipt for students enrolling in a public college or university who receives need-based aid. Because CBS eligibility affects who enrolls in public institutions of higher education in Washington and, therefore, who is in our data (particularly at 4-year colleges), the effects of CBS receipt for college enrollees may be biased. As discussed in [Section VI](#), we perform various analyses to address this limitation. We demonstrate that our estimated effects of receipt, particularly for 4-year college students, may depend on whether we are focusing on students who would enroll regardless of CBS receipt or those who we think enroll because of CBS receipt.

Third, we cannot rule out the possibility that other factors caused our observed effects. The CBS program coincided with the beginning of the Great Recession, which would have affected family incomes of many in Washington State potentially changing who might be pledge eligible. Additionally, around the time that CBS students would be

³⁷ Goldhaber et al. (2017).

attending college, the economy began to recover and tuition prices started rising, potentially altering college choices. Because we rely on pre-period cohorts to identify the effect of CBS, other factors occurring at the same time may explain some of the effects of we observe.

The CBS program has also changed considerably from its first implementation. Sign-up rates have increased substantially. The relationship between CBS and SNG has also changed, potentially altering the likelihood that non-CBS students receive SNG funding and making the CBS program more attractive. Data availability and the strength of our research designs requires focusing on early CBS cohorts who may differ in important ways from more recent cohorts in both their student characteristics and the financial aid landscape they face. We cannot necessarily expect our findings to apply to students facing these different educational landscapes. Further analysis may be required to determine whether the findings continue to apply to more recent cohorts.

Finally, we delete observations with missing data on variables included in our analyses. While this approach does not necessarily bias our results, we cannot guarantee that findings are not sensitive to excluding observations with missing data. However, we are able to replicate the effects reported in Goldhaber et al. (2017) which used a method that imputes missing values. This suggests that our results may be insensitive to our approach to missing data.³⁸ [Appendix V](#) provides more detail about our approach to missing data.

Although these limitations have the potential to complicate our analyses or introduce bias into our estimates, we provide numerous sensitivity tests in the appendices and generally find our results are robust.

³⁸ Ibid.

VIII. Summary and Next Steps

This report describes our findings of the effectiveness of the College Bound Scholarship program on education outcomes for students in Washington public schools. We analyzed the effects of pledge eligibility and signing the College Bound pledge in middle school, CBS eligibility at the end of high school, and CBS receipt in the first year of college.

We find that signing the pledge has little effect on student outcomes. The pledge does not improve student outcomes unless those students who sign the pledge actually fulfill the pledge commitments and become CBS eligible when they complete high school.

For those students who are eligible to receive the scholarship, we find positive effects on college enrollment, persistence, credit accumulation, and degree receipt. The estimates of the effects of CBS eligibility are robust to various sensitivity analyses.

We find receiving CBS dollars has positive effects on attainment for all college students combined and for students at 2-year colleges. On the other hand, we find either no relationship or a negative relationship between CBS receipt and education outcomes for students at 4-year colleges. However, these findings should be interpreted cautiously as these results are not robust to our sensitivity analyses and could stem from differences between the treatment and comparison groups.

In summary, our findings suggest the College Bound Scholarship is effective for students who are in a position to access CBS funding when they are making their college-going decisions.

Next Steps

In this report, we focused on the effects of CBS on education outcomes at public institutions in Washington State. CBS may impact education outcomes at private institutions or at out-of-state colleges as well. To examine potential effects at other types of institutions, we obtained data from the National Student Clearinghouse that tracks students to most colleges across the country. Findings using these data will be published in February 2019.



Appendices

The Effectiveness of Washington's College Bound Scholarship Program

I.	Effect of College Bound Pledge and Scholarship on Non-Education Outcomes	38
II.	Pledge Eligibility and Pledge Signing.....	44
III.	Scholarship Analysis	61
IV.	Some Causes of Ineligibility	93
V.	Missing Data Examination	100

I. Effect of College Bound Pledge and Scholarship on Non-Education Outcomes

In this section, we present results from an analysis of the effects of CBS on financial aid and criminal justice outcomes. Although not specifically part of the legislative assignment, the CBS program design raises questions about potential effects on both criminal justice and financial aid outcomes. To meet the pledge requirements, a student must have no felony convictions prior to college. It is unclear whether this requirement reduces the likelihood of felony convictions or general criminal justice system involvement. Additionally, as a scholarship program, CBS could affect the amounts and types of aid a student receives. We examine the effects of the pledge on criminal justice outcomes prior to high school completion and the pledge and the scholarship on financial aid outcomes. This appendix describes these outcomes in more detail and reports the results of our analyses for the pledge eligibility, scholarship eligibility, and scholarship receipt sections.

Criminal Justice Outcomes

We focus on four types of criminal justice outcomes: misdemeanor charges, misdemeanor convictions, felony charges, and felony convictions. We consider charges or convictions that occur in Washington between the 7th grade school year and a student's actual high school completion year for students who completed high school. For students who drop out or transfer prior to completing high school, we measure charges and convictions between 7th grade and their expected completion year. Convictions that are later vacated when a student completes certain requirements (e.g., diversions) are considered convictions for this analysis. We evaluate only the effects of the College Bound pledge on criminal justice outcomes.

Financial Aid Outcomes

We focus on three financial aid outcomes: receipt of any need-based aid (including federal, state, institutional, or private aid), State Need Grant (SNG) aid, and loan aid. We limit our analysis of these outcomes to the first year of college. As in the analysis of college attainment and achievement, we focus on aid received during a student's on-time college years.

We measure effects on aid receipt and the total amount of each type of aid received. Students who do not attend college at all or do not attend on time are included in these analyses as not receiving any aid and receiving \$0 of aid. We construct the outcomes in this manner to avoid biasing our results by conditioning on a post-treatment variable, college enrollment. We evaluate the effects of both the pledge and the scholarship on financial aid outcomes

Effects of Pledge Eligibility and Signing on Non-Education Outcomes

Criminal Justice Outcomes

We find signing the College Bound pledge reduces the likelihood that students will have one or more felony convictions by high school graduation by 1.7 percentage points ([Exhibit A1](#)). We find no evidence of an effect on the receipt of misdemeanor convictions, or on the likelihood of charges for felonies or misdemeanors.³⁹

Exhibit A1

Effects of Pledge Eligibility and Pledge Signing on Criminal Justice Outcomes

Variable	Pledge eligibility			Pledge signing		
	Effect	SE	N	Effect	SE	N
Criminal justice						
Proportion with felony charge prior to HS completion [^]	-	-	-	-	-	-
Proportion with misdemeanor charge prior to HS completion	-0.004	0.005	432,083	-0.011	0.013	432,083
Proportion with felony conviction prior to HS completion	-0.006**	0.002	432,083	-0.017**	0.007	432,083
Proportion with misdemeanor conviction prior to HS completion	-0.003	0.004	432,083	-0.007	0.012	432,083

Notes:

* p<0.05, ** p<0.01, *** p<0.001

The number of asterisks next to each effect estimate indicates the level of confidence we ascribe to the effect. We can be reasonably confident that effect estimates with at least one asterisk are real effects. If an effect estimate has no asterisks, it means we cannot statistically distinguish the “true” effect from zero.

[^] We exclude results for this outcome because its model failed an important statistical test. See [Appendix II](#) for discussion of this test.

Financial Aid

We find no evidence suggesting pledge eligibility or sign-up changes the likelihood that a student will receive need-based financial aid during his or her first on-time postsecondary year ([Exhibit A2](#)).⁴⁰ On the other hand, those who sign the pledge receive an average of \$967 more in need-based financial aid than they would have received had they not signed the pledge.

We find signing the pledge makes students 4.1 percentage points more likely to receive SNG funding during their first on-time postsecondary year. We find no evidence suggesting signing the pledge causes a decrease in the amount of student loan dollars students receive during that first year.

³⁹ Our model of the effect of pledge eligibility on felony charges fails an important statistical test, the parallel trends test, which would suggest that our finding is unreliable. When we use the alternative cohort and eligibility definitions described in Appendix II, we find the felony charges model passes the statistical test and that the pledge reduces felony charges by 2.6 percentage points.

⁴⁰ When we use the alternative cohort and eligibility definitions described in Appendix II, we find signing the pledge increases a student’s likelihood of receiving need-based aid in their first year of college by 3.7 percentage points.

Exhibit A2

Effects of Pledge Eligibility and Pledge Signing on Financial Aid Outcomes

Variable	Pledge eligibility			Pledge signing		
	Effect	SE	N	Effect	SE	N
Proportion who receive need-based aid in first on-time year of college	0.001	0.005	426,844	0.004	0.013	426,844
Proportion receiving State Need Grant dollars in first on-time year of college	0.015***	0.003	426,844	0.041***	0.009	426,844
Loan dollars received in first on-time year of college	-\$37	\$25	426,844	-\$102	\$67	426,844
Need-based aid dollars received in in first on-time year of college	\$355 ***	\$50	426,844	\$967***	\$136	426,844

Note:

* p<0.05, ** p<0.01, *** p<0.001

Exhibits A3, A4, and A5, respectively, demonstrate trends in SNG receipt, average student loan receipt in dollars, and average funding from all financial aid in what would be a student's first year of on-time college attendance. The exhibits use regression-adjusted trends to compare rates or dollar amounts for all pledge-eligible and ineligible students, regardless of whether they signed the pledge. There seems to be a steeper increase in SNG receipt, loan aid amount, and total aid received for pledge-eligible students in Cohort Three immediately after CBS is introduced. Rates then level off or fall for Cohorts Four through Six.

Exhibit A3

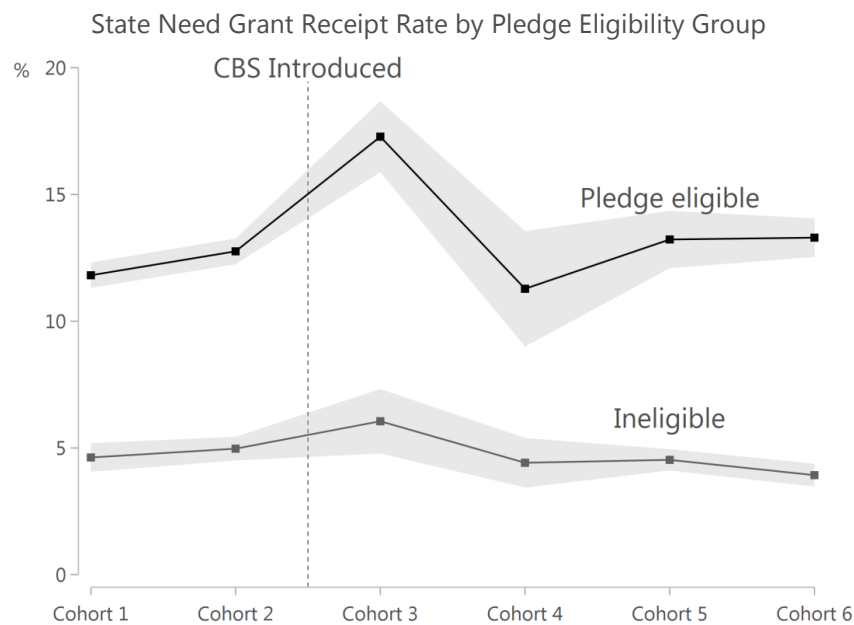


Exhibit A4

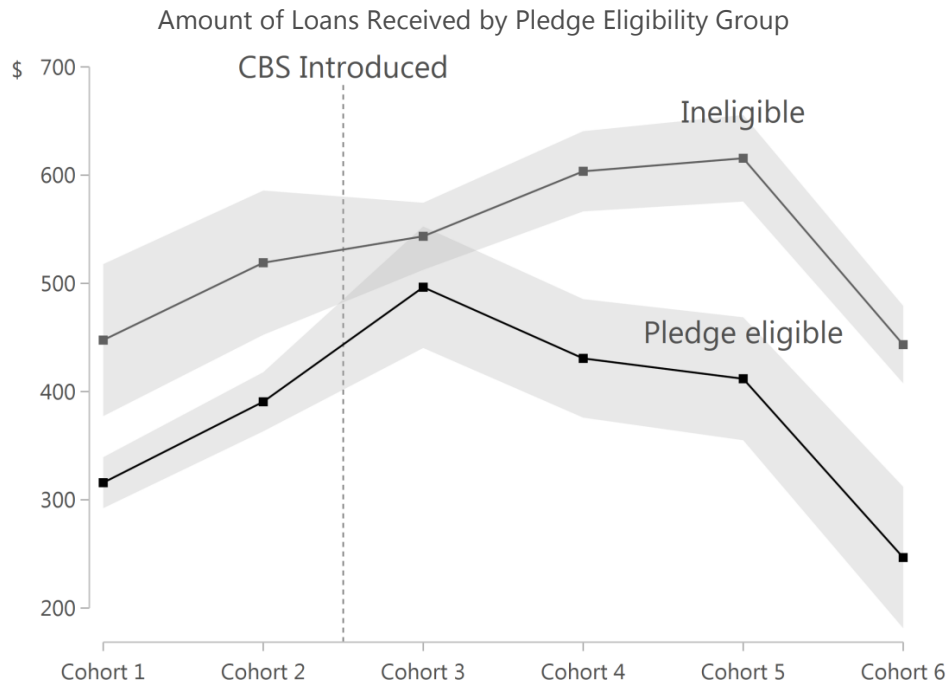
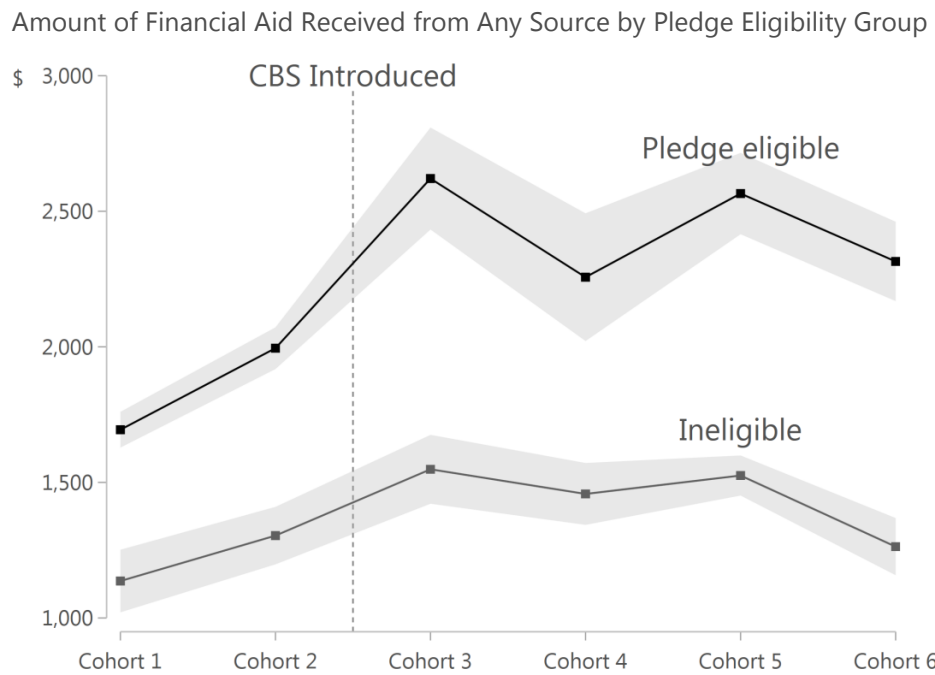


Exhibit A5



Effects of Scholarship Eligibility and Receipt on Financial Aid Outcomes

Using our propensity score matching (PSM) approach as described in [Section III](#), we find evidence that being eligible for the College Bound Scholarship increases the proportion of people receiving need-based aid in their first year of college ([Exhibit A6](#)). There is a positive effect on the total SNG and total need-based aid dollars for the CBS-eligible group, and we find evidence of a small increase in loan dollars.

Exhibit A6

Effects of Scholarship Eligibility on Financial Aid Outcomes

Outcome	Comparison group mean	Effect	SE	N
Proportion who receive need-based aid in first on-time year of college	0.42	0.083***	0.007	24,056
State need grant dollars received in first on-time year of college	\$1,090	\$755***	\$43	24,056
Loan dollars received in first on-time year of college	\$679	\$82**	\$29	24,056
Need-based aid dollars received in first on-time year of college	\$4,335	\$2,174***	\$113	24,056

Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These results are not conditional on enrolling in college. Part of the observed increases in aid will be due to the increased likelihood that the student attends college, while part will be due to changes in aid receipt for those who enroll in college. We, therefore, show the effects of CBS eligibility for those who enter college in [Exhibit A7](#). While these effects may suffer from post-treatment bias—because scholarship eligibility affects who enters college—we present them for reference. The results in [Exhibit A7](#) are generally similar to those in [Exhibit A6](#) except larger, which we would expect because we exclude the students who do not attend college and thus receive no aid. We also find no difference in loan dollars received when we limit the analysis to college enrollees.

Exhibit A7

Effects of Scholarship Eligibility on Financial Aid Outcomes Conditional on College Enrollment

Outcome	Comparison group mean	Effect	SE	N
Proportion who receive need-based aid in first on-time year of college	0.79	0.067***	0.008	14,748
State need grant dollars received in first on-time year of college	\$2,013	\$1,178***	\$72	14,748
Loan dollars received in first on-time year of college	\$1,257	\$24	\$57	14,748
Need-based aid dollars received in first on-time year of college	\$8,072	\$3,099***	\$156	14,748

Note:

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[Exhibit A8](#) shows the effects on these financial aid outcomes when limiting our analysis to college enrollees in general and to 2- or 4-year enrollees who receive need-based aid in their first year of college (as described in [Sections III](#) and [V](#)). We find that CBS receipt increases the amount of SNG and total aid received, with much of the increase in aid due to the increase in the CBS dollars received. Students receiving CBS in their first years of college also receive fewer loan dollars.

Exhibit A8

Effects of Scholarship Receipt on Financial Aid Outcomes for Students All College Students and Those First Enrolling in a 2-Year and 4-Year Institutions

Outcome	Comparison group mean	Effect	SE	N
All college students				
College Bound Scholarship dollars received in first on-time year of college	\$0	\$2,350 ***	\$46	12,366
State Need Grant dollars received in first on-time year of college	\$2,639	\$1,343 ***	\$64	12,366
Need-based aid dollars received in first on-time year of college	\$10,278	\$3,257 ***	\$123	12,366
Loan dollars received in first on-time year of college	\$1,501	-\$20	\$58	12,366
2-year college students				
College Bound Scholarship dollars received in first on-time year of college	\$0	\$1,991 ***	\$113	7,059
State Need Grant dollars received in first on-time year of college	\$1,197	\$387 **	\$108	7,059
Need-based aid dollars received in first on-time year of college	\$6,146	\$2,085 ***	\$107	7,059
Loan dollars received in first on-time year of college	\$373	-\$131 ***	\$27	7,059
4-year college students				
College Bound Scholarship dollars received in first on-time year of college	\$0	\$2,688 ***	\$150	4,805
State Need Grant dollars received in first on-time year of college	\$5,146	\$1,868 **	\$414	4,805
Need-based aid dollars received in first on-time year of college	\$17,713	\$2,522 **	\$578	4,805
Loan dollars received in first on-time year of college	\$3,806	-\$735 ***	\$94	4,805

Note:

* p<0.05, ** p<0.01, *** p<0.001

II. Pledge Eligibility and Pledge Signing

This section provides greater detail about the characteristics of pledge students and the methodology used to evaluate the effects of pledge eligibility and pledge signing. We discuss the results of tests of the assumptions of difference-in-differences (DID) analysis and present sensitivity analyses, including sensitivity to our sample and cohort definition decisions. Finally, we discuss our results in the context of a recent study on the effects of the CBS program by Goldhaber et al. (2017).

Characteristics of Pledge-Eligible Students

[Exhibit A9](#) provides characteristics of pledge-eligible and ineligible students in our analytic sample (observed in 7th and 8th grade with no missing student characteristics) before and after the introduction of the CBS program. Pledge-eligible students tend to be disadvantaged in ways that are associated with educational attainment (e.g., they are more likely to be disabled or have lower standardized test scores). We also observe some changes among pledge-eligible students from pre- to post-period (e.g., the proportion who were White decreases and the proportion whose primary language is not English increases). Ideally, we would observe similar changes in the ineligible student distributions such that differences between eligible and ineligible students are the same in pre- and post-periods. However, our methodology can account for these observed changes in student distributions even when they differ across eligibility status.

Exhibit A9

Student Characteristics by Pledge Eligibility and Pre/Post Period

Variable	Pre-College Bound Scholarship program		Post-College Bound Scholarship program	
	Pseudo eligible (N=58,729)	Ineligible (N=79,610)	Eligible (N=137,896)	Ineligible (N=156,222)
Proportion eligible for pledge based on FRL status in 7 th , 8 th , or 9 th grade	1.00	0.00	1.00	0.00
Proportion who signed the pledge	0.00	0.00	0.46	0.03
Proportion female	0.50	0.48	0.49	0.49
Proportion White	0.53	0.81	0.46	0.77
Proportion Black	0.07	0.03	0.07	0.02
Proportion Hispanic	0.23	0.03	0.28	0.05
Proportion Asian	0.07	0.07	0.06	0.08
Proportion American Indian/Alaska Native	0.04	0.01	0.02	0.01
Proportion other race	0.06	0.04	0.11	0.08
Age in 7 th grade	12.90	12.83	12.91	12.85
Standard dev.	0.46	0.41	0.43	0.37
Proportion with primary language non-English	0.21	0.03	0.27	0.05
Proportion with home language non-English	0.26	0.10	0.27	0.07
Proportion migrant	0.05	0.00	0.04	0.00
Proportion with disability	0.16	0.08	0.16	0.08
Proportion English language learner	0.12	0.01	0.11	0.01
Proportion who received academic assistance in 7 th grade	0.17	0.07	0.26	0.11
Proportion in gifted program in 7 th grade	0.01	0.05	0.02	0.07
Proportion taking modified math test in 8 th grade	0.05	0.02	0.05	0.02
Proportion taking modified reading test in 8 th grade	0.05	0.02	0.05	0.02
Standardized math score in 8 th grade	-0.32	0.32	-0.36	0.32
Standard dev.	0.94	0.93	0.92	0.96
Standardized reading score in 8 th grade	-0.25	0.25	-0.29	0.26
Standard dev.	0.99	0.92	1.01	0.91
Proportion with felony charge prior to 7 th grade	0.01	0.00	0.01	0.00
Proportion with misdemeanor charge prior to 7 th grade	0.03	0.01	0.03	0.01
Proportion with felony conviction prior to 7 th grade	0.00	0.00	0.01	0.00
Proportion with misdemeanor conviction prior to 7 th grade	0.02	0.00	0.02	0.00

Exhibit A10 shows the differences in the outcomes by pledge eligibility and period. The unadjusted difference-in-differences generally demonstrate small or negative effects of pledge eligibility on student outcomes reflecting our main findings in Exhibit 12, although many of the unadjusted effects are larger in absolute value than our adjusted effects.

Exhibit A10

Student Outcomes by Pledge Eligibility and Pre/Post Period

Outcome	Pre-College Bound Scholarship program				Post-College Bound Scholarship program				Unadjusted DID estimate
	Pseudo eligible		Ineligible		Eligible		Ineligible		
	Mean/ proportion	N	Mean/ proportion	N	Mean/ proportion	N	Mean/ proportion	N	
High school									
GPA in 12 th grade	2.42	45,823	2.93	71,285	2.39	109,527	2.98	140,339	-0.080
Standard dev.	0.85		0.79		0.86		0.77		
Proportion completing high school on time	0.64	53,528	0.86	74,979	0.64	126,533	0.87	147,354	-0.013
Enrollment									
Proportion enrolling in any college on time	0.33	58,729	0.50	79,610	0.32	137,896	0.49	156,222	0.000
Proportion enrolling in 2-year college on time	0.26	58,729	0.33	79,610	0.23	137,896	0.31	156,222	-0.003
Proportion enrolling in 4-year college on time	0.08	58,729	0.21	79,610	0.09	137,896	0.22	156,222	0.002
Credits earned									
Cumulative credit hours earned one year after high school completion	7.39	58,177	14.74	78,494	7.52	136,609	14.99	153,934	-0.116
Standard dev.	14.97		19.49		15.08		19.80		
Cumulative credit hours earned two years after high school completion	13.58	58,177	27.23	78,494	13.36	101,835	27.66	117,321	-0.648
Standard dev.	27.70		36.92		27.62		37.29		
Cumulative credit hours earned three years after high school completion	17.10	58,176	35.19	78,494	16.43	68,576	35.61	80,132	-1.088
Standard dev.	37.67		52.04		37.35		52.26		
Cumulative credit hours earned four years after high school completion	18.11	58,168	39.37	78,494	16.29	36,374	39.04	40,689	-1.490
Standard dev.	44.96		65.05		43.18		64.97		
Persistence									
Proportion enrolling in two consecutive years of college	0.23	58,177	0.40	78,494	0.23	102,714	0.40	119,128	-0.007
Proportion enrolling in three consecutive years of college	0.17	58,176	0.30	78,494	0.16	69,229	0.31	81,391	-0.011
Proportion enrolling in four consecutive years of college	0.11	58,168	0.22	78,494	0.11	36,766	0.23	41,416	-0.012
Graduation									
Proportion who graduated with 2-year degree within two years of on-time high school completion	0.04	44,429	0.07	70,590	0.06	69,729	0.10	104,445	-0.010
Proportion who graduated with 2-year degree within three years of on-time high school completion	0.07	44,267	0.11	70,527	0.11	47,375	0.18	74,204	-0.022
Proportion who graduated with 4-year degree within four years of on-time high school completion	0.04	43,494	0.11	70,021	0.05	23,293	0.14	35,775	-0.016
Course taking and achievement									
Proportion who ever take a remedial math course in college	0.20	58,729	0.21	79,610	0.14	137,896	0.15	156,222	0.003
Proportion who ever take a remedial English course in college	0.12	58,729	0.08	79,610	0.08	137,896	0.05	156,222	-0.012
GPA at end of 1 st year of college (2-year college)	2.36	14,132	2.66	25,576	2.36	29,153	2.70	45,652	-0.049
Standard dev.	1.05		0.98		1.09		1.03		
GPA at end of 2 nd year of college (2-year college)	2.53	9,677	2.73	17,424	2.52	16,372	2.78	26,332	-0.056
Standard dev.	0.88		0.83		0.91		0.88		

Outcome	Pre-College Bound Scholarship program				Post-College Bound Scholarship program				Unadjusted DID estimate
	Pseudo eligible		Ineligible		Eligible		Ineligible		
	Mean/ proportion	N	Mean/ proportion	N	Mean/ proportion	N	Mean/ proportion	N	
GPA at end of 1 st year of college (4-year college)	2.61	4,566	2.84	16,437	2.54	12,863	2.85	34,351	-0.078
Standard dev.	0.85		0.79		0.94		0.83		
GPA at end of 2 nd year of college (4-year college)	2.72	3,947	2.93	14,613	2.71	7,522	2.95	22,973	-0.037
Standard dev.	0.79		0.73		0.81		0.74		

[Exhibit A11](#) includes characteristics for pledge-signing students. We disaggregate all pledge signers into those signers who were clearly pledge eligible in the post-period—i.e., FRL in 7th or 8th grade (8th or 9th grade for Cohort Three)—and signers who were not clearly pledge eligible in the post-period—i.e., students whose eligibility we could not observe or were possibly not eligible to sign the pledge but did so anyway. We also show characteristics of students who were clearly pledge eligible in the post-period but did not sign the pledge. As discussed in the main report, students who signed the pledge tend to have characteristics that are associated with greater educational attainment and achievement such as higher test scores. Those students who are not clearly pledge eligible but sign the pledge tend to be the most advantaged with respect to characteristics associated with educational attainment or achievement (e.g., highest 8th-grade test scores, lowest rates of disability).

Exhibit A11

Student Characteristics by Pledge Sign-Up Status in the Post-Period

Variable	All pledge signers (N=71,579)	Clearly eligible pledge signers (N=66,492)	Other pledge signers (N=5,087)	Clearly eligible non-signers (N=88,120)
Proportion female	0.53	0.53	0.56	0.45
Proportion White	0.41	0.40	0.60	0.50
Proportion Black	0.08	0.08	0.04	0.07
Proportion Hispanic	0.31	0.32	0.12	0.24
Proportion Asian	0.08	0.07	0.12	0.05
Proportion American Indian/Alaska Native	0.02	0.02	0.02	0.03
Proportion other race	0.11	0.11	0.10	0.12
Age in 7 th grade	12.89	12.90	12.85	12.94
Standard dev.	0.42	0.42	0.39	0.45
Proportion with primary language non-English	0.32	0.33	0.14	0.22
Proportion with home language non-English	0.31	0.32	0.14	0.22
Proportion migrant	0.05	0.06	0.01	0.03
Proportion with disability	0.13	0.13	0.08	0.19
Proportion English language learner	0.12	0.13	0.03	0.10
Proportion who received academic assistance in 7 th grade	0.26	0.27	0.16	0.25
Proportion in gifted program in 7 th grade	0.03	0.03	0.06	0.01
Proportion retained in 7 th grade	0.00	0.00	0.00	0.01
Proportion retained in 8 th grade	0.01	0.01	0.00	0.03
Proportion retained in 9 th grade	0.05	0.05	0.02	0.14
Proportion taking modified math test in 8 th grade	0.04	0.04	0.02	0.06
Proportion taking modified reading test in 8 th grade	0.04	0.04	0.02	0.06
Standardized math score in 8 th grade	-0.21	-0.25	0.20	-0.46
Standard dev.	0.92	0.91	0.93	0.92
Standardized reading score in 8 th grade	-0.14	-0.17	0.25	-0.40
Standard dev.	0.99	0.99	0.91	1.02
Proportion with felony charge prior to 7 th grade	0.01	0.01	0.00	0.02
Proportion with misdemeanor charge prior to 7 th grade	0.03	0.03	0.01	0.05
Proportion with felony conviction prior to 7 th grade	0.00	0.00	0.00	0.01
Proportion with misdemeanor conviction prior to 7 th grade	0.02	0.02	0.01	0.04

Pledge Eligibility Analysis Methodology

As stated in the main report, we evaluate the effect of pledge eligibility using a difference-in-differences (DID) estimator. The estimator contrasts outcome trends before and after CBS implementation for clearly pledge-eligible and ineligible students. We model outcomes as functions of student characteristics and school-by-year fixed effects using the following equation:

$$y_i = \Delta_1(\text{Eligible}_i) + \Delta_2(\text{Post}_i) + \delta(\text{Eligible}_i \times \text{Post}_i) + x_i\beta + \alpha_i + u_i$$

y_i = outcome for individual i

δ = the parameter of interest, identified by the interaction $\text{Eligible}_i \times \text{Post}_i$

Eligible_i = indicator that student was clearly pledge eligible (i.e., received FRL in eligible grades)

Post_i = indicator that student belonged to post-implementation cohort

x_i = vector of observable student characteristics

α_i = school \times year unobserved effect

u_i = random error term

The vector x_i includes the characteristic variables reported in [Exhibit A9](#). They include gender, race, ethnicity, age, birth month, a bilingual indicator, an indicator for having a non-English primary language, an indicator for having a non-English language as the primary language spoken at home, an indicator for migrant status, a disability indicator, an indicator for receipt of academic assistance, an indicator for receipt of services for gifted students, indicators for criminal history before 7th grade, 8th grade standardized test scores for math and English, and indicators for having taken alternative standardized tests in 8th grade.

We account for unobserved heterogeneity between schools and across time using school-by-year fixed effects. “School” is defined as a student’s 7th grade middle school, while “year” is defined as a student’s 7th grade school year (if a student was retained in 7th grade, we use the year of the first time the student was in 7th grade). The fixed effects capture differences between schools that mediate or moderate the relationship between pledge availability and student outcomes, while allowing those differences to vary over time. The effects allow flexibility for each school to change idiosyncratically over time. Use of these fixed effects implies we identify the causal parameter of interest using variation within schools within years.

We use inverse probability weights in our DID models to adjust for potentially-nonlinear changes in covariate distributions over time. We use the method proposed by Stuart et al. (2014) to estimate propensity score weights for the DID context.⁴¹ Students are segmented into four groups: eligible and ineligible students, before and after CBS implementation. The weights are calculated as the ratio of a student’s probability of being in the group to which they actually belong to their probability of being in the group of clearly pledge-eligible students prior to program implementation. We estimate propensity scores using a multinomial logit model. The multinomial logit model controls for the same student characteristics included in our difference-in-differences regression model. A further discussion of propensity score models can be found in [Appendix III](#).

⁴¹ Stuart, E.A., Huskamp, H.A., Duckworth, K., Simmons, J., Song, Z., Chernerew, M.E., & Barry, C.L. (2014). Using propensity scores in difference-in-differences models to estimate the effects of a policy change. *Health Services and Outcomes Research Methodology*, 14(4), 166–182.

We use cluster-robust standard errors, clustered by 7th-grade school, in our DID models. Clustering by school allows for arbitrary correlation of model error terms, between student cohorts, within schools. Bertrand, Duflo, and Mullainathan (2004) show that cluster-robust standard errors reduce bias in the standard errors of DID estimators from auto-correlation.⁴² In addition, collapsing to a two-period, pre/post analysis also addresses the issues raised by Bertrand et al, 2004.

When we model dichotomous outcomes, our preferred approach uses a linear probability model. For DID estimation, the linear probability model will produce the difference in conditional means. Furthermore, we focus on the marginal effects produced from the linear probability models rather than the predicted values as linear probability models can predict probability values outside the range of 0%-100%. Parameters in linear probability models remain consistent and asymptotically normal even when a portion of the predicted values are outside the realistic range. However, we also conducted our analyses using logit regression and found similar results to those using linear probability models.

Pledge Signing Analysis Methodology

Following Scott-Clayton (2011), we use the DID estimator as an instrument for pledge signing. We then use Two-Stage Least Squares (2SLS) to estimate the effect of signing the College Bound pledge on student outcomes.⁴³ Our 2SLS specification isolates variation in a pledge signing indicator, allowing us to estimate the effect of signing the pledge for students who are clearly pledge eligible through receipt of free- or reduced-price lunch. Close to 46% of clearly-eligible students in our sample signed the pledge. To obtain an unbiased estimate with a treatment effect on the treated (ToT) interpretation, we use $\text{Eligible}_i \times \text{Post}_i$ as an excluded instrument for an indicator of pledge signing. In doing so we assume pledge availability only affects outcomes for those students who participate in the program by signing the pledge.

We estimate the following equations where we now include Signed_i as an indicator for pledge signing:

$$(1) \quad \widehat{\text{Signed}}_i = \tilde{\Delta}_1(\text{Eligible}_i) + \tilde{\Delta}_2(\text{Post}_i) + \tilde{\pi}(\text{Eligible}_i \times \text{Post}_i) + x_i\tilde{\beta} + \tilde{\alpha}_i + \varepsilon_i$$

$$(2) \quad y_i = \Delta_1(\text{Eligible}_i) + \Delta_2(\text{Post}_i) + \pi(\widehat{\text{Signed}}_i) + x_i\beta + \alpha_i + u_i$$

Equation (1) is a first-stage equation, which we use to predict Signed_i . The covariation between Signed_i and $\text{Eligible}_i \times \text{Post}_i$ is then used in the second-stage equation, Equation (2), to identify the effect of signing the pledge. The parameter of interest is π , the effect of signing the pledge.

de Chaisemartin and D'Haultfoeuille (2014) point out that use of this procedure in the DID context relies on two additional assumptions.⁴⁴ First, the effect of signing the pledge should be the same for our pre-implementation cohorts (were the program available then) and our post-implementation cohorts. Second, the pledge should have the same effect on eligible pledge-signing students as it would on “ineligible”

⁴² Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates?. *The Quarterly Journal of Economics*, 119(1), 249-275.

⁴³ Scott-Clayton, J. (2011). On money and motivation: A quasi-experimental analysis of financial incentives for college achievement. *Journal of Human Resources*, 46(3), 614-646.

⁴⁴ de Chaisemartin, C., & d'Haultfoeuille, X. (2014). *Fuzzy changes-in-changes*. CAGE Online Working Paper Series 184. Coventry, UK: Centre for Competitive Advantage in the Global Economy (CAGE).

pledge-signing students (were they to sign the pledge). In other words, we assume program effects are homogenous across time and among all eligible students.

Parallel Trends Tests

The validity of the DID framework relies on the assumption of parallel trends. This assumption states that in the absence of the College Bound program the eligible and ineligible student group outcomes would follow a similar trend over time. The groups' outcome levels need not be the same, but their secular trends and responses to external events must move in parallel. If we observe parallel trends prior to the implementation of an intervention, we may be able to determine whether a treatment effect exists by assessing whether the group trends deviate from parallel movement after implementation.

We cannot prove outcome trends would be parallel in the absence of the College Bound program because we cannot observe what would have happened without College Bound. We instead assess whether outcome trends were parallel before College Bound implementation and assume we can extrapolate to a counterfactual setting. Similarly, if trends for eligible and ineligible students diverge prior to CBS implementation, we conclude that other factors exist that may cause these groups to differ that could be unrelated to CBS implementation and our effect estimates may not reflect the true effects of the program.

We test for parallel trends by comparing the trends between eligible and ineligible students prior to the program's implementation using a regression-based test with a placebo DID specification. In other words, we specify a DID model as though the intervention took place between Cohorts One and Two. We use the null hypothesis $H_0 = \text{parallel trends}$. If the trends moved in parallel between Cohorts One and Two, we should find no DID effect. If we observe an effect, we can reject the null hypothesis for that outcome and conclude that the trends prior to implementation are not parallel.

Two outcomes failed this test in our preferred model.

- Proportion who graduated with 2-year degree within two years of on-time high school completion, and
- Proportion with felony charge prior to high school completion (this variable barely passes the parallel trends test, $p < 0.052$).

These results suggest that differing trends between the eligible and ineligible group occurred prior to CBS implementation may explain the effects we observe on on-time 2-year degree receipt and felony charges.

Testing for Changes in Covariate Distributions

Because we are using a multiple cross-section sample, our analysis is particularly susceptible to bias due to changes in covariate distributions from cohort to cohort or across time. We test for changes in covariate distributions across cohorts and find the distributions change for most variables. If covariate distributions change during the time of CBS implementation, then these changes in student characteristics could explain the effects of the CBS rather the program itself. To test for distribution changes in a covariate, conditional on other characteristics, we used a regression-based test with DID specification wherein the covariate of interest was treated as an outcome. In this setup, the difference-in-differences effect parameter should be zero if the covariate distributions in the treatment and control cohorts are changing in the same direction and at the same rate. We use the null hypothesis $H_0 = \text{no change in distribution}$. If we observe an effect, we reject the null hypothesis and conclude that student composition changed during the time of CBS implementation.

Using our unweighted models, we found that many covariate distributions changed over time. We repeated the tests using models with propensity score weights to adjust for changes in the distributions, and we found the weights substantially reduced differences in observable characteristics. We prefer the propensity score weighted regression because we can rule out changes in student composition over time as a potential cause of our observed effects.

Sensitivity Analysis

We explored the sensitivity and robustness of our results using several models. These models help us identify the extent to which our results are sensitive to changes in model or sample specification. Each model evaluates the effects of pledge eligibility. Results are reported in [Exhibit A12](#).

Our results are generally robust to inclusion of a homelessness indicator (which results in dropping many students due to missing data), to the use of 7th-grade test scores (which drops the first cohort due to missing data), and even to the exclusion of student covariates. Our results are robust to these alternative model specifications in the sense that the DID coefficients are similar in magnitude, sign, and statistical significance. In a few instances where coefficients differ in statistical significance, their magnitudes and standard errors tell the same story across models.

Our finding for on-time enrollment in a 4-year college is not robust across models. We did not find an effect of pledge eligibility on on-time enrollment at 4-year institutions in our preferred model, but the coefficient increases in magnitude and becomes statistically significant (without increased precision) when we add an indicator for homelessness or remove student covariates. However, the sensitivity of our null result to alternative specifications and samples does not necessarily mean that the pledge has an effect on 4-year enrollment.

Analysis without Propensity Score Weights

We estimated a DID model without propensity score weights. The model was otherwise the same as our preferred model. In lacking propensity score weights, this model does not control for changes in sample distributions over time.

Analysis that Conditions Sample on High School Graduation

We estimated the effects of pledge availability using a sample restricted to those students who completed high school on time. The model used is the same as our preferred model. This analysis is biased if the pledge affects high school graduation.

We estimated this model because one may assume that high school graduates benefit more from the College Bound pledge because many colleges require high school completion to attend. We find little difference between our reported findings and those limited to a set of high school graduates. The main exception is that we find a significant positive effect of enrollment in a 4-year college when conditioning on high school completion.

Analysis that Conditions Sample on College Enrollment

We estimated the effects of pledge availability using a sample restricted to those students who enrolled in college on time. The model used is the same as our preferred model. This analysis is biased if the pledge affects college enrollment.

We estimated this model because our preferred analysis uses the joint outcome of enrollment and persistence or graduation. Students who do not enroll are considered non-persisters or non-graduates. This outcome definition prevents post-treatment bias in our estimates but may be less intuitive. We, therefore, perform our analysis using a sample of college enrollees that estimates the effects of the College Bound pledge on persistence and completion for students who enroll. Results conditional on college are generally similar to those using the full sample both directionally and in terms of statistical significance.

Model that Controls for Student Homelessness

We estimated the effects of pledge availability using a model that controls for homelessness but that is otherwise the same as our preferred model. Approximately 10% of students have missing values in our homelessness variable, concentrated in the pre-period cohorts. Prior to 2007-08, schools could choose whether to report whether a student was homeless. Consequently, this variable is missing for 25%-30% of student in the pre-period cohorts. This missingness also results in the frequency of homeless students in our sixth cohort appearing to be about five times greater than in our first cohort. This suggests there may be bias in the variable, wherein homelessness may be underreported in earlier cohorts.

Model that Controls for 7th-Grade Test Scores in Lieu of 8th-Grade Test Scores

We estimated the effects of pledge availability using a model that controls for 7th-grade standardized test scores in lieu of 8th-grade test scores but that is otherwise the same as our preferred model. Because 7th-grade test scores are not available for our earliest student cohort, the sample used in this analysis only includes our second through sixth cohorts. Controlling for 7th-grade test scores is potentially safer than controlling for 8th-grade test scores, in that models using 8th-grade test scores will be biased if the pledge affects test scores. We use 8th-grade test scores in our preferred models to include all six of our student cohorts.

Model that Includes Fixed Effects but Not Covariates

We estimated a model that controls for school-by-year fixed effects but does not control for student characteristics. Results from this model will be biased if any student characteristics are correlated with both pledge availability and student outcomes.

Model that Includes Covariates but Not Fixed Effects

We estimated a model that controls for student characteristics but does not control for school-by-year fixed effects. Results from this model will be biased if school characteristics are correlated with both pledge availability and student outcomes.

Exhibit A12

Effect of Pledge Eligibility Across Models

Outcome	Preferred	Unweighted	HS graduates	College enrollees	Including homeless student indicator	Including 7 th grade test scores	FE without covariates	Preferred without FE
High school								
Cumulative GPA at the end of 12 th grade	-0.036** (0.011)	-0.017** (0.006)			-0.027* (0.012)	-0.031* (0.013)	-0.038*** (0.008)	-0.131** (0.045)
Proportion completing high school on time	-0.001 (0.006)	0.013*** (0.003)			0.001 (0.007)	-0.006 (0.007)	0.011** (0.003)	-0.033* (0.016)
Enrollment								
Proportion enrolling in any college on time	-0.003 (0.005)	0.006 (0.004)	0.011 (0.006)		0.001 (0.006)	-0.009 (0.007)	0.006 (0.004)	-0.014 (0.009)
Proportion enrolling in 2-year college on time	-0.008 (0.006)	-0.003 (0.004)	-0.000 (0.006)		-0.008 (0.006)	-0.011 (0.007)	-0.000 (0.004)	-0.017 (0.009)
Proportion enrolling in 4-year college on time	0.005 (0.003)	0.009*** (0.003)	0.013*** (0.004)		0.009* (0.004)	0.003 (0.004)	0.007* (0.003)	0.002 (0.003)
Credits earned								
Cumulative credit hours earned one year after high school completion	-0.086 (0.178)	0.179 (0.124)	0.228 (0.214)	-0.142 (0.315)	0.024 (0.238)	-0.213 (0.208)	0.135 (0.137)	-0.352 (0.238)
Cumulative credit hours earned two years after high school completion	-0.677* (0.306)	0.118 (0.241)	-0.424 (0.379)	-0.645 (0.607)	-0.375 (0.397)	-0.891* (0.399)	0.030 (0.266)	-1.313** (0.416)
Cumulative credit hours earned three years after high school completion	-0.750 (0.435)	0.129 (0.355)	-0.632 (0.562)	0.239 (1.035)	-0.584 (0.542)	-1.523** (0.558)	-0.022 (0.382)	-1.746** (0.608)
Cumulative credit hours earned three years after high school completion	0.003 (0.621)	0.818 (0.511)	0.028 (0.803)	1.919 (1.756)	-0.166 (0.710)	-1.012 (0.790)	0.620 (0.547)	-2.200 (1.293)
Persistence								
Proportion enrolling in two consecutive years of college	-0.003 (0.005)	0.003 (0.003)	0.001 (0.006)	-0.013 (0.008)	-0.001 (0.006)	-0.007 (0.007)	0.004 (0.004)	-0.013 (0.007)
Proportion enrolling in three consecutive years of college	-0.005 (0.005)	0.001 (0.003)	-0.002 (0.006)	-0.019 (0.010)	-0.003 (0.006)	-0.008 (0.006)	0.001 (0.004)	-0.013* (0.006)
Proportion enrolling in four consecutive years of college	-0.002 (0.005)	0.004 (0.003)	-0.004 (0.006)	-0.009 (0.011)	-0.002 (0.005)	-0.006 (0.006)	0.003 (0.004)	-0.014 (0.008)
Graduation								
Proportion who graduated with 2-year degree within two years of on-time high school completion	-0.001 (0.002)	-0.004 (0.002)	-0.001 (0.003)	-0.003 (0.005)	-0.002 (0.003)	0.002 (0.003)	-0.003 (0.002)	-0.001 (0.003)
Proportion who graduated with 2-year degree within three years of on-time high school completion	-0.006 (0.004)	-0.004 (0.003)	-0.006 (0.004)	-0.016* (0.007)	-0.007 (0.004)	-0.006 (0.005)	-0.001 (0.003)	-0.010* (0.005)
Proportion who graduated with 4-year degree within four years of on-time high school completion	-0.001 (0.004)	0.000 (0.003)	-0.002 (0.004)	-0.000 (0.007)	-0.000 (0.005)	-0.004 (0.005)	0.000 (0.003)	-0.006 (0.005)
Course taking and achievement								
Proportion who ever take a remedial math course in college	-0.002 (0.006)	0.005 (0.003)	0.004 (0.006)	-0.008 (0.008)	0.001 (0.007)	-0.005 (0.008)	0.007* (0.003)	-0.008 (0.010)

Proportion who ever take a remedial English course in college	-0.006 (0.005)	-0.010*** (0.002)	-0.003 (0.006)	-0.012 (0.008)	-0.004 (0.006)	-0.007 (0.006)	-0.008*** (0.002)	-0.020* (0.008)
Cumulative GPA at end of first year of college (2-year college)	-0.041* (0.021)	-0.020 (0.014)	-0.040 (0.021)	-0.040 (0.021)	-0.051* (0.022)	-0.049* (0.025)	-0.039** (0.015)	-0.048* (0.023)
Cumulative GPA at end of second year of college (2-year college)	-0.051* (0.022)	-0.025 (0.016)	-0.045* (0.022)	-0.051* (0.022)	-0.046 (0.024)	-0.051 (0.028)	-0.043** (0.017)	-0.042 (0.022)
Cumulative GPA at end of first year of college (4-year college)	-0.058* (0.023)	-0.055** (0.018)	-0.063** (0.023)	-0.058* (0.023)	-0.072** (0.026)	-0.052 (0.030)	-0.077*** (0.020)	-0.069** (0.024)
Cumulative GPA at end of second year of college (4-year college)	-0.016 (0.023)	-0.012 (0.020)	-0.016 (0.023)	-0.016 (0.023)	-0.009 (0.025)	-0.019 (0.029)	-0.034 (0.022)	-0.028 (0.022)

Notes:

* p<0.05, ** p<0.01, *** p<0.001

Standard error clustered at the 7th grade school level in parentheses.

Sensitivity to Sample Definition

One important consideration in our analysis of pledge eligibility and pledge signing is the impact of the Great Recession. The recession occurred at the same time as CBS implementation and could bias our results in at least two ways. First, if the Great Recession affected pledge-eligible students differently than ineligible students, then effects of the Great Recession would confound any observed effects.

Unfortunately, we have no direct way to test for this type of confounding. Second, the Great Recession could change the composition of pledge-eligible and ineligible students before and after CBS implementation. For example, more students may use FRL services after the Great Recession than would have without the recession. Any change in composition would mean that our pre-period treatment or comparison groups could differ in important ways from post-period groups.

We assume that students whose FRL status never changed during our analysis period may be less affected by the Great Recession, and thus tested the robustness of our results to a subgroup of students who are persistently FRL or never FRL during our observation period. To do this, we drop all students who were transitory FRL recipients, i.e., received FRL in some but not all grades between 7th and 12th grade. Thus, students in pledge-eligible group would have received FRL in every grade from 7th-12th, while students in the comparison group must have never received FRL. Here we assume outcomes for transitory FRL recipients follow different trends than those for other students. Their transitory incomes may cause trend divergence or they may have transitory incomes for reasons that would also cause their trends to diverge—consequences of a common factor. Results from this approach are substantively similar to those from our preferred model, but with this sample, the effect on 4-year college enrollment is positive and statistically significant, while the effects on college GPA and college credits earned are not statistically significant.

We could also employ an alternative approach wherein we dropped all students who had never received FRL at any point. The remaining students would have received FRL at some point, potentially increasing the comparability of the treatment and comparison groups. However, we believe that approach may exacerbate bias because students who received FRL in a transitory pattern may have been responding to the Great Recession differently than students who persistently received or did not receive FRL. Their transitory FRL patterns indicate differences from students with persistent patterns. In other words, this approach leads to treatment and comparison groups that are likely to differ in their outcome trends.

Sensitivity to Cohort Definition

As described in the main report, we use the last cohort a student enters to determine cohort assignment. This cohort definition ensures that all students who are actually eligible to sign the pledge in the post-period would be assigned to a post-period cohort. For example, a student receiving FRL services in 9th grade in the 2007-08 school year would be included in Cohort Two and not be eligible to sign the pledge. If that student were retained and remained in 9th grade in 2008-09, the student could be included in Cohort Two or Cohort Three and in the latter case, could sign the pledge. Our main cohort definition would define this student as pledge-eligible and in Cohort Three, but we also estimated our models using a set of alternative definitions for cohort assignment and pledge eligibility that would place this student in Cohort Two. The alternative eligibility definition flags students as clearly pledge eligible if they received free- or reduced-price lunch the first time they were in 7th and 8th grade (or 8th and 9th for Cohort Three), ignoring the possibility that students became eligible after being retained one or more grades. The alternative rule for cohort assignment places students in cohorts based on the first time they were in 7th grade. Results from these analyses are reported in [Exhibit A13](#).

Results from these analyses are generally similar to those obtained from our preferred models. There are two notable differences. First, the alternative cohort models suggest students earned about five credits more by the end of their fourth year in college, where our preferred model indicated no effect. Second, the alternative models indicate a statistically significant reduction in felony charges, where our preferred model failed to pass the parallel trends test (results available upon request).

Exhibit A13

Effects of Pledge Eligibility and Pledge Signing Using Alternative Cohort and Eligibility Definitions

Variable	Pledge eligibility			Pledge signing		
	Effect	SE	N	Effect	SE	N
High school						
Cumulative GPA at the end of 12 th grade	-0.030**	0.011	366,966	-0.077**	0.029	366,966
Proportion completing high school on time	0.008	0.007	402,582	0.020	0.018	402,582
Enrollment						
Proportion enrolling in any college on time	-0.000	0.006	432,670	-0.001	0.016	432,670
Proportion enrolling in 2-year college on time	-0.006	0.006	432,670	-0.016	0.016	432,670
Proportion enrolling in 4-year college on time	0.005	0.003	432,670	0.014	0.008	432,670
Credits earned						
Cumulative credit hours earned one year after high school completion	-0.074	0.178	427,055	-0.203	0.488	427,055
Cumulative credit hours earned two years after high school completion	-0.529	0.315	355,433	-1.601	0.953	355,433
Cumulative credit hours earned three years after high school completion	-0.097	0.464	284,995	-0.322	1.546	284,995
Cumulative credit hours earned four years after high school completion	1.375*	0.667	213,381	4.687*	2.267	213,381
Persistence						
Proportion enrolling in two consecutive years of college	-0.002	0.006	358,186	-0.005	0.017	358,186
Proportion enrolling in three consecutive years of college	-0.001	0.005	286,857	-0.002	0.017	286,857
Proportion enrolling in four consecutive years of college	0.008	0.005	214,456	0.026	0.018	214,456
Graduation						
Proportion who graduated with 2-year degree within two years of on-time high school completion	-0.001	0.002	288,855	-0.003	0.006	288,855
Proportion who graduated with 2-year degree within three years of on-time high school completion	-0.006	0.004	236,027	-0.019	0.011	236,027
Proportion who graduated with 4-year degree within four years of on-time high school completion	-0.000	0.004	172,205	-0.001	0.012	172,205
Course taking and achievement						
Proportion who ever take a remedial math course in college	0.003	0.006	432,670	0.009	0.015	432,670
Proportion who ever take a remedial English course in college	-0.002	0.005	432,670	-0.004	0.014	432,670
Cumulative GPA at end of first year of college (2-year college)	-0.039	0.021	114,250	-0.089	0.047	114,250
Cumulative GPA at end of second year of college (2-year college)	-0.052*	0.022	69,494	-0.124*	0.054	69,494
Cumulative GPA at end of first year of college (4-year college)	-0.052*	0.023	67,918	-0.093*	0.042	67,918
Cumulative GPA at end of second year of college (4-year college)	-0.010	0.023	48,754	-0.019	0.044	48,754

Notes:

* p<0.05, ** p<0.01, *** p<0.001

Notes on the Recent Study from the Center for Education Data & Research (CEDR)

Goldhaber et al. (2017) published a study estimating the effects of College Bound availability on high school outcomes—henceforth referred to as the CEDR study.⁴⁵ The CEDR study used a cohort definition similar to the alternative cohort definition described above using the first time a student is observed in 8th grade. Moreover, they did not use propensity score weights to adjust for changes in cohort distributions, and they use multiple imputation to account for missing data, while we exclude students with missing covariates. Our results may differ because of these methodological choices.

We found that availability of the pledge leads to an average decrease in high school GPA of 0.036 points among students eligible to sign it. This finding is larger than that reported by in the CEDR study. They found an average decrease in high school GPA of 0.012 among pledge-eligible students. When we use a statistical model similar to that used by CEDR (i.e., unweighted with similar cohort definition, though we still drop students with missing data), we find an average decrease in high school GPA of 0.013 points among clearly pledge-eligible students.

The CEDR study also found an increase in high school graduation from pledge availability, but their finding failed their falsification tests suggesting the increase was due to secular trends rather than pledge eligibility. When we use a statistical model similar to their model, we see a similar result with a similar failure of the statistical test. Our preferred model passes the statistical test after we use propensity score weights to adjust for potentially-nonlinear secular trends in some student characteristics. Similar to the conclusions in the CEDR study, when using our preferred model we found no evidence of an effect on high school graduation.

⁴⁵ Goldhaber et al. (2017).

III. Scholarship Analysis

This section provides more information on the scholarship analysis results. We provide greater detail on our propensity score matching approach for both scholarship eligibility and scholarship receipt and illustrate the robustness of our results to different sample and modeling decisions. We also provide complete results for our analysis of the effects of CBS eligibility by high school GPA category. Finally, we discuss the results of a regression discontinuity design, an alternative design to that used in the main report.

Ideally, we would evaluate the effect of scholarship eligibility and receipt using a random assignment approach to assign eligible students to the CBS scholarship group or a comparison group. Successful random assignment to treatment allows for an unbiased comparison of outcomes between participants and non-participants that is not confounded by observable characteristics (like academic achievement) or unobservable characteristics (like intrinsic motivation); thus, any differences in outcomes can be attributed to the effect of the treatment. However, because CBS scholarship eligibility and receipt are not randomly assigned, we are unable to use this approach.

Instead, we use propensity score matching (PSM) as our primary method to determine the effects of scholarship eligibility and receipt on student outcomes. PSM allows us to balance observed characteristics between treatment and comparison groups. Unlike random assignment, however, PSM cannot eliminate the risk that selection bias or other unobserved factors may threaten the validity of the findings. Thus, in addition to PSM, we also estimated the effects of scholarship eligibility using a regression discontinuity design (RDD) approach. RDDs have greater internal validity than PSM and can balance treatment and comparison groups on both observed and unobserved characteristics when certain assumptions are met. Limitations of the RDDs prevented us from utilizing it as our primary approach, but we check the robustness of our PSM results using an RDD.

Scholarship Eligibility Study Groups

To evaluate the effects of CBS eligibility, we focus on students who are CBS eligible at the time of high school completion. Students must be observed in 7th and 8th grade as required for our pledge analysis sample. For this analysis, we also require students to be observed in 12th grade because we use cumulative GPA in 12th grade to identify scholarship eligibility. The treatment group, which we also refer to as the CBS-eligible group, includes students in the first two CBS cohorts; i.e., those in 8th or 9th grade in 2007-08 or in 7th or 8th grade in 2008-09.⁴⁶ Treatment group students signed the pledge, graduated high school with a regular or Individualized Education Plan (IEP) diploma, finished with at least a 2.0 GPA, and had no felony convictions prior to high school completion.⁴⁷

⁴⁶ We limit our analysis to the first two CBS cohorts for two reasons. First, this type of pre-post analysis can suffer from bias due to temporal changes. We attempt to limit bias from changing trends by using those cohorts nearest to the CBS policy change. Still, bias from changing trends could arise. We check the robustness of our results using a contemporaneous comparison group. Second, because we use 1:1 nearest neighbor matching with no replacement, we cannot include treated students who do not have a match. If we used all four CBS cohorts, we would have a treatment group that was much larger than our comparison group requiring us to exclude many treated observations at random. By limiting to the first two cohorts, we avoid this random trimming. We perform a robustness check using all four CBS cohorts and kernel matching that does not discard any observations within the region of common support and find similar results.

⁴⁷ We do not consider GED recipients or those receiving an adult diploma as graduates because these students were not eligible to receive CBS at the time.

Additionally, treatment group students must be receiving FRL in 12th grade. CBS eligibility requires students to have an income below 65% of the state MFI. Unfortunately, we do not have access to a student's family income for CBS-eligible students at the time of high school completion. Because we cannot determine income eligibility for all students, we limit our analysis to FRL students in 12th grade. FRL income cutoffs are lower than 65% of MFI for most households.

This FRL restriction hopefully ensures that the significant majority of students who we identify as CBS eligible are indeed eligible to receive CBS, but it also limits our findings to students receiving FRL. We may not be able to generalize our findings to other students who are not receiving FRL but are still CBS eligible based on their family income. Although we do not know exactly how many students this might include, we can estimate this number using available data. We have data on students receiving need-based aid at public institutions. Based on these data, we estimate that for otherwise CBS-eligible students who are in their first year of college and receiving need-based aid, about 93% of those who received FRL services in 12th grade have an income below 65% of the state MFI, suggesting that FRL is a relatively good proxy for income eligibility for CBS. Furthermore, only about 20% of CBS-eligible students receiving need-based aid who have an income below 65% of the state MFI in their first year of college do not receive FRL in 12th grade. Our results may not generalize to this 20% of CBS-eligible students, but our results would apply to the majority of eligible students.

We draw the comparison group from a pool of students in the pre-period cohorts—those in 7th or 8th grade in 2005-06 or 2006-07—who were pledge eligible, graduated high school with a regular or Individualized Education Plan (IEP) diploma, finished with at least a 2.0 GPA, had no felony convictions prior to high school completion, and were FRL in 12th grade.

We draw the comparison group from a sample of pre-period students in an effort to mitigate differences between the treatment and comparison group. We could compare pledge-eligible students in the CBS cohorts who signed the pledge and meet the requirements for the scholarship to a contemporaneous group of pledge-eligible students in the CBS cohorts who did not sign the pledge but otherwise meet the requirements for the scholarship. We may worry, however, that most or all of the comparison group was less motivated to go to college as evidenced by their decision not to sign the pledge. Propensity score matching will not address this difference in unobserved motivation, and selection bias could remain or worsen after matching. By using a comparison pool from the pre-period, we can assume that at least some of the students in our pre-period cohorts would have had the motivation to sign the pledge if given the opportunity, and the main difference between the treatment and comparison pool is being eligible for CBS in the post period.

Using a pre-period comparison group can lead to bias due to temporal changes, however. For example, if college enrollment rates are increasing over time in general, then our analysis may incorrectly attribute this general trend to an effect of CBS. Similarly, if other events occur during the same period, then we might erroneously attribute the effects of another event to CBS. For example, if tuition increased substantially between the time that the pre-period and post-period cohorts would be entering college, as did occur for Washington public institutions,⁴⁸ then we might observe a decrease in enrollment for our treatment group. That decline could be caused by the tuition increase rather than CBS. We test the robustness of our results to different comparison groups to address the limitations of the various options.

⁴⁸ For example, tuition at Washington research universities increased about 14% between 2011-12 and 2012-13 when the first CBS cohort would enter college. See Guaranteed Education Tuition. [In-state tuition and GET payout value over time.](#)

In creating our study groups, we do not restrict our analysis to students who filed a FAFSA, even though that is a requirement for CBS eligibility. We only have data on FAFSA filing for students who signed the pledge, and FAFSA filing demonstrates a strong motivation to attend college. By imposing the FAFSA restriction on the treatment group only, we may create selection bias by creating a treatment group that is more motivated to attend college. Because we cannot observe this motivation, the treatment group may differ in important ways from the comparison group that would artificially increase our estimated effects of the program. Thus, we do not consider FAFSA filing when creating our study groups to evaluate the effects of scholarship eligibility, although we examine the robustness of our results to including the FAFSA restriction.

Scholarship Eligibility Propensity Score Matching

Although not a panacea for selection bias, PSM does have advantages over standard regression analysis. First, in PSM, the outcome plays no part in matching the treated and comparison groups. This emulates an experimental design by separating the research design stage—where we test various matching procedures to obtain a sufficiently matched sample—from the analysis stage—where we estimate the effect of the treatment using our matched sample. Second, matching can limit the importance of functional form in regression analysis.⁴⁹ Third, by imposing common support restrictions, we ensure that the comparison group does not differ substantially in their likelihood to be eligible for CBS in the post-period, i.e., we are not comparing CBS-eligible students to students who we would never expect to be eligible. Finally, by conducting a regression analysis on the matched sample using the covariates from the matching model, we further reduce any residual bias that may remain after matching and account for any correlation between matched pairs.

We match on the propensity score defined in the equation below:⁵⁰

$$(3) \quad p_i = \Pr(z_i = 1|X_i) = \left(\frac{e^{(\alpha + \beta_1 X_{1i} + \dots + \beta_k X_{ki})}}{1 + e^{(\alpha + \beta_1 X_{1i} + \dots + \beta_k X_{ki})}} \right)$$

In Equation (3), p_i represents the probability that individual i receives treatment z (i.e., the propensity score), α represents the intercept of the model, β_j represents the parameter of the model for covariate X_j , and e is the base of the natural logarithm. We use listwise deletion to exclude any observations with missing data on the included covariates. [Exhibit A14](#) below reports the results from the coefficients from the first stage model estimating the likelihood of CBS eligibility. We control for demographic characteristics, academic characteristics, criminal justice system involvement, and some 7th-grade school characteristics.

⁴⁹ Ho, D.E., Imai, K., King, G., & Stuart, E.A. (2007). Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference. *Political analysis*, 15(3), 199-236

⁵⁰ The propensity score was estimated using the `pscore` command and the matching procedures were performed using `psmatch2` in STATA

Exhibit A14

Logit Model Estimating the Likelihood of Being in the Treatment Group

Variable*	Coefficient	SE	p-value
Female	0.034	0.025	0.162
Black	0.265	0.047	0.000
Hispanic	0.276	0.035	0.000
Asian	0.021	0.046	0.654
American Indian/Alaska Native	-0.181	0.082	0.027
Other race	0.872	0.050	0.000
Age in 12 th grade	-0.076	0.021	0.000
English language learner	-0.092	0.068	0.174
Primary language non-English	0.320	0.042	0.000
Home language non-English	-0.190	0.041	0.000
Disability	-0.108	0.051	0.035
Migrant in 7 th grade	-0.085	0.053	0.105
Received academic assistance in 12 th grade	0.016	0.039	0.688
In gifted program in 12 th grade	0.670	0.073	0.000
GPA in 12 th grade	-0.051	0.044	0.245
GPA in 10 th grade	0.229	0.036	0.000
Participate in Running Start	0.178	0.034	0.000
Had felony charge prior to HS completion	-0.086	0.128	0.502
Had misdemeanor charge prior to HS completion	-0.196	0.109	0.073
Had misdemeanor conviction prior to HS completion	0.073	0.113	0.522
Standardized math score in 8 th grade	-0.023	0.020	0.241
Standardized reading score in 8 th grade	0.044	0.018	0.013
Took modified math test in 8 th grade	0.021	0.120	0.860
Took modified reading test in 8 th grade	-0.214	0.120	0.074
12 th grade school pledge eligible rate	1.046	0.086	0.000
12 th grade school average standardized math test scores	-0.226	0.077	0.003
12 th grade school average standardized reading test scores	0.788	0.073	0.000
Constant	-2.434	1.112	0.029
N	33,173		
Pseudo-R2	0.056		

Notes:

* We also include a set of variables indicating the pattern of free- or reduced-priced lunch status (e.g., FRL in 6th grade only, FRL in 6th and 7th grade, FRL in 7th grade only).

Our preferred matching procedure for the main analysis is 1:1 nearest neighbor matching, without replacement, with a caliper of 0.001.⁵¹ We allow ties, meaning that treatment group students are matched with all closest comparison group youth with identical propensity scores. Using 1:1 matching can reduce the bias between the treatment and comparison groups by only matching treated individuals with the most similar comparison group individual. By using a caliper, the propensity scores of the comparison group matches must fall within the caliper distance from a treated individual to be included. The caliper ensures that treated individuals are not matched with comparison group youth that are too dissimilar and also ensures sufficient overlap between the treated and comparison groups (i.e., a common support region). However, 1:1 caliper matching without replacement can also lead to a smaller common support

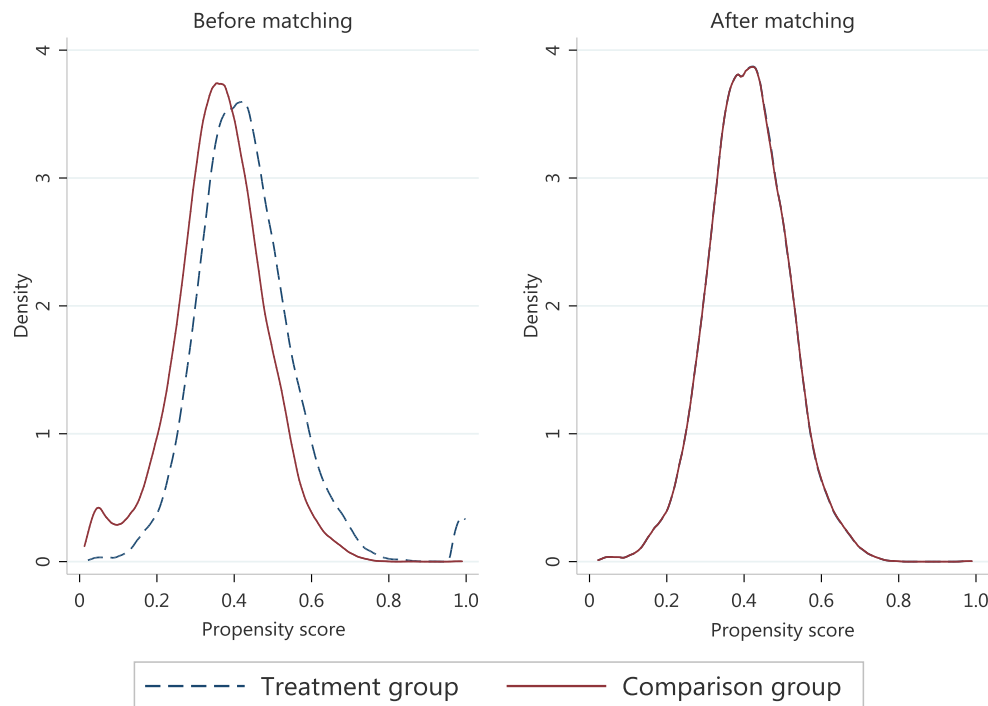
⁵¹ We employ this method for our main analysis based in part on recommendations in Austin, P.C. (2014). A comparison of 12 algorithms for matching on the propensity score. *Statistics in Medicine*, 33, 1057-1069 and Rosenbaum, P.R., & Rubin, D.B. (1985). Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *The American Statistician*, 39(1), 33-38. Additionally, Abadie and Speiss (2016) recommend nearest neighbor without replacement to produce correct standard errors from regression performed on the matched sample. Abadie, A., & Speiss, J. (2016). *Robust post-matching inference*. Harvard University. Unpublished manuscript.

region by excluding CBS-eligible students for whom no good match can be found.⁵² Furthermore, the variance of the estimated effect is higher with 1:1 matching, leading to larger confidence intervals. We test the robustness of our preferred method to propensity score weighting where only about 30 treatment group students are dropped because they fall outside of the common support region.

We first evaluate whether PSM improves differences in the distribution of the propensity scores between the treatment and comparison groups in [Exhibit A15](#). After matching, the propensity score distributions almost entirely overlap indicating similar likelihoods of being CBS eligible in the post period among the matched sample.

Exhibit A15

Distribution of the Propensity Scores Before and After Matching



We next evaluate balance in the covariates using the standardized percent difference (or bias) calculated as the difference in the mean/proportion for the treated and comparison groups divided by the pooled standard deviation for each covariate prior to matching multiplied by 100. This measure is preferred to traditional t-tests as the standardized difference is not influenced by the study's sample size. Additionally, t-tests are used for making inferences about a population based on a sample; balance, on the other hand, is an in-sample property. Standardized bias values greater than 10 usually indicate moderate imbalance while greater than 25 indicates severe imbalance.⁵³ [Exhibit A16](#) displays the standardized bias for each covariate in the propensity score model before and after matching as well as the p-value from a t-test comparing differences in means as a reference. After matching, most differences were reduced and the bias for all covariates is below 10.

⁵² We exclude about 925 CBS-eligible students or about 7%, because no good matches were found.

⁵³ Austin, P.C. (2009). Balance diagnostics for comparing the distribution of baseline covariates between treatment groups in propensity-score matched samples. *Statistics in Medicine*, 28(25), 3083-3107 and Stuart, E.A. (2010). Matching methods for causal inference: A review and a look forward. *Statistical Science: A Review Journal of the Institute of Mathematical Statistics*, 25(1), 1–21.

Exhibit A16

Study Groups Characteristics Before and After Matching

Variable*	Before matching				After matching			
	CBS eligible (N=12,953)	Comparison group (N=20,252)	p- value	Standardized difference (absolute value)	CBS eligible (N=12,028)	Comparison group (N=12,028)	p- value	Standardized difference (absolute value)
Demographic characteristics								
Propensity score	0.432	0.363	0.000	55.106	0.411	0.411	0.894	0.171
Proportion female	0.586	0.568	0.001	3.754	0.585	0.588	0.695	0.506
Proportion White	0.395	0.492	0.000	19.639	0.409	0.413	0.573	0.727
Proportion Black	0.075	0.072	0.314	1.137	0.077	0.078	0.847	0.248
Proportion Hispanic	0.336	0.269	0.000	14.654	0.334	0.333	0.837	0.265
Proportion Asian	0.091	0.093	0.695	0.441	0.094	0.091	0.449	0.977
Proportion American Indian/Alaska Native	0.018	0.028	0.000	6.272	0.019	0.019	0.777	0.366
Proportion other race	0.084	0.046	0.000	15.301	0.066	0.067	0.918	0.134
Age in 12 th grade	17.948	18.001	0.000	8.954	17.955	17.948	0.383	1.125
Academic characteristics								
Proportion English language learner	0.035	0.035	0.897	0.146	0.036	0.036	0.972	0.044
Proportion with primary language non-English	0.323	0.258	0.000	14.443	0.320	0.322	0.709	0.481
Proportion with home language non-English	0.364	0.311	0.000	11.159	0.363	0.365	0.728	0.449
Proportion with disability	0.091	0.125	0.000	10.907	0.096	0.096	0.965	0.056
Proportion migrant in 7 th grade	0.068	0.052	0.000	6.699	0.068	0.067	0.589	0.696
Proportion who received academic assistance in 12 th grade	0.107	0.103	0.192	1.471	0.107	0.108	0.677	0.536
Proportion in gifted program in 12 th grade	0.040	0.018	0.000	12.972	0.027	0.028	0.782	0.357
GPA in 12 th grade	2.914	2.842	0.000	13.642	2.897	2.901	0.643	0.598
GPA in 10 th grade	2.903	2.792	0.000	16.735	2.882	2.886	0.643	0.597
Proportion participating in Running Start	0.179	0.147	0.000	8.766	0.172	0.173	0.772	0.374
Standardized math score in 8 th grade	-0.036	-0.102	0.000	7.340	-0.055	-0.052	0.817	0.298
Standardized reading score in 8 th grade	0.091	-0.023	0.000	12.351	0.064	0.063	0.933	0.108
Proportion taking modified math test in 8 th grade	0.034	0.049	0.000	7.579	0.036	0.035	0.728	0.449
Proportion taking modified reading test in 8 th grade	0.033	0.048	0.000	7.817	0.035	0.034	0.621	0.638
Criminal justice system involvement								
Proportion with felony charge prior to HS completion	0.008	0.011	0.002	3.463	0.008	0.007	0.464	0.944
Proportion with misdemeanor charge prior to HS completion	0.096	0.119	0.000	7.515	0.100	0.098	0.746	0.418
Proportion with misdemeanor conviction prior to HS completion	0.087	0.107	0.000	6.740	0.090	0.088	0.525	0.819

7 th grade school characteristics								
7 th grade school pledge eligible rate	0.598	0.561	0.000	17.824	0.595	0.595	0.836	0.267
7 th grade school average 8 th grade standardized math test scores	-0.134	-0.110	0.000	7.275	-0.133	-0.132	0.704	0.490
7 th grade school average 8 th grade standardized reading test scores	-0.074	-0.082	0.017	2.699	-0.080	-0.082	0.679	0.533

Notes:

* We include a set of variables indicating the pattern of free- or reduced-lunch status (e.g., FRL in 6th grade only, FRL in 6th and 7th grade, FRL in 7th grade only). We do not include those indicators here, but the largest standardized difference after matching was 1.3.

Outcome Model

After matching, we perform regression on our matched sample—the outcome model. We use OLS regression on the matched sample to estimate the effect of CBS eligibility for continuous outcomes and OLS in the form of a linear probability model for dichotomous outcomes to estimate the marginal effects of CBS eligibility. Our outcome model uses the same covariates included in the matching model as well as fixed effects for a student's 12th-grade school. The regression model is weighted using a normalized weight based on the number of times a comparison group student was matched to a CBS-eligible student. A summary of results is presented in [Section IV](#) of the main report. We do not report our full regression results here for brevity, but results are available upon request.

Researchers using propensity score matching may employ bootstrapping to calculate standard errors to account for the fact that the propensity score is estimated. It may be unnecessary to employ bootstrapping when the regression analysis includes the covariates in the matching model.⁵⁴ Therefore, we use the analytical standard errors estimated from the regression performed on the matched data. We cluster our standard errors by 12th-grade school.

Robustness Checks

We test the sensitivity of our analysis to numerous decisions. Due to the sizeable number of outcomes and sensitivity analyses conducted, we only provide a summary of these analyses here. Full results are available upon request.

First, in our main analysis, we use a student's actual high school GPA when determining whether a student meets the 2.0 GPA requirement. We also used a student's actual GPA in the propensity score and regression models. However, we found in our analysis of the College Bound pledge that students who signed the pledge had lower GPAs. By using a student's actual GPA, we are using a variable that was affected by the treatment, which could lead to bias. For example, pledge signers may have lower GPAs because they are taking more advanced courses to prepare for college. By matching on their actual GPA, we will match pledge signers with lower GPAs to students from the pre-period who have similarly lower GPAs. Pledge signers would be more prepared for college in this example because signing the pledge induced them to take more advanced courses. We cannot match on this unobserved preparation, and thus, we could introduce selection bias by matching on actual GPA.

To test the sensitivity to using a student's actual GPA, we predict what a post-period CBS-eligible student's GPA would have been if CBS did not exist. We do this by using the pre-period cohorts to model GPA as a function of student characteristics. We then use the coefficients from that model and a student's actual characteristics for students in the post-period to predict the 12th-grade GPA for students in the post period. We use this predicted GPA to determine scholarship eligibility and as a covariate in propensity score and outcome models. Our results using the predicted GPA do not differ in any substantive way from results using actual GPA.

Second, as discussed previously, we do not include a FAFSA requirement when identifying treatment group students. We assess the sensitivity to this decision by requiring treatment group students to have filed a FAFSA (we do not have data on FAFSA filing for our comparison group). Results using this definition for the treatment group are similar to our preferred analysis, although somewhat larger, indicating a greater likelihood of enrolling and persisting in college and obtaining a degree. We might expect this result because students who file a FAFSA demonstrate motivation to attend college, and we

⁵⁴ Ho et al. (2007) and Gelman, A., & Hill, J. (2006). *Data analysis using regression and multilevel/hierarchical models*. Cambridge University Press.

cannot account for this difference in unobservable motivation. Results differ with respect to developmental course participation where we find a significant increase in the likelihood to participate in a developmental course when we condition on filing a FAFSA for the treatment group. We find no effect of CBS eligibility on developmental course participation in our main analysis.

Next, we used two contemporaneous comparison groups including students from the post-period cohorts who (1) did not sign the pledge or (2) were ineligible to sign the pledge but were FRL in other years or grades. We included a cohort fixed effect in the outcome models using these comparison groups. Estimated effects on enrollment, persistence, and credit accumulation using the contemporaneous comparison groups are larger than those using our preferred analysis, while effects on graduation are similar in size although not always significant. We also find positive effects on developmental course participation using either contemporaneous comparison group, although these results are not significant. While the size of the effects vary depending on whether we use a pre-period or contemporaneous comparison group, the main conclusions are generally comparable across our preferred analysis and those using contemporaneous comparison groups. Using 1:1 nearest neighbor matching results in excluding almost half of the treatment group due to a lack of suitable matches. Thus, we use kernel weighting for these sensitivity analyses.

Finally, we also attempted to match students within their 12th-grade schools. Results were nearly identical to our preferred analysis even though we lose about 30% of the treatment group due to a lack of suitable matches. Because we exclude more students, and we control for 12th-grade school in our outcome model, we prefer our main analysis to one that matches students within schools.

Scholarship Eligibility by GPA Category

To estimate the effects of CBS eligibility by GPA category, we perform our matching process and regression on the matched sample within each GPA category. We use the same covariates from the main propensity score model and regression. [Exhibit A17](#) presents the characteristics of students by GPA category after matching. We find no imbalance between the groups after matching. We also provide the full results by GPA category for all outcomes in [Exhibit A18](#).

Exhibit A17

Matched Study Groups Characteristics

Variable	GPA >= 2.0 and < 2.5		GPA >= 2.5 and < 3.0		GPA >= 3.0 and < 3.5		GPA >= 3.5 and <=4.0	
	CBS eligible (N=3,117)	Comparison group (N=3,117)	CBS eligible (N=3,617)	Comparison group (N=3,617)	CBS eligible (N=2,940)	Comparison group (N=2,940)	CBS eligible (N=1,881)	Comparison group (N=1,881)
Demographic characteristics								
Propensity score	0.374	0.374	0.403	0.403	0.428	0.428	0.449	0.450
Proportion female	0.494	0.494	0.573	0.569	0.648	0.658	0.669	0.672
Proportion White	0.363	0.373	0.408	0.407	0.462	0.468	0.461	0.453
Proportion Black	0.105	0.101	0.084	0.085	0.067	0.060	0.040	0.045
Proportion Hispanic	0.388	0.382	0.352	0.355	0.299	0.304	0.233	0.240
Proportion Asian	0.047	0.044	0.070	0.067	0.106	0.106	0.206	0.204
Proportion American Indian/Alaska Native	0.022	0.023	0.021	0.019	0.019	0.016	0.015	0.015
Proportion other race	0.075	0.077	0.066	0.068	0.046	0.046	0.045	0.043
Age in 12 th grade	18.045	18.044	17.952	17.953	17.918	17.911	17.877	17.869
Academic characteristics								
Proportion English language learner	0.052	0.051	0.043	0.045	0.027	0.029	0.013	0.015
Proportion with primary language non-English	0.308	0.305	0.312	0.318	0.311	0.317	0.342	0.339
Proportion with home language non-English	0.343	0.333	0.359	0.362	0.354	0.363	0.401	0.400
Proportion with disability	0.136	0.141	0.122	0.120	0.069	0.067	0.033	0.029
Proportion migrant in 7 th grade	0.078	0.079	0.073	0.074	0.061	0.064	0.042	0.047
Proportion who received academic assistance in 12 th grade	0.175	0.170	0.113	0.118	0.070	0.070	0.026	0.027
Proportion in gifted program in 12 th grade	0.009	0.009	0.019	0.016	0.037	0.037	0.049	0.044
GPA in 12 th grade	2.262	2.261	2.746	2.746	3.231	3.231	3.725	3.721
GPA in 10 th grade	2.188	2.178	2.728	2.731	3.252	3.257	3.733	3.732
Proportion participating in Running Start	0.078	0.078	0.140	0.144	0.220	0.219	0.306	0.305
Standardized math score in 8 th grade	-0.430	-0.434	-0.216	-0.222	0.082	0.084	0.666	0.656
Standardized reading score in 8 th grade	-0.273	-0.284	-0.074	-0.072	0.190	0.175	0.643	0.632
Proportion taking modified math test in 8 th grade	0.043	0.045	0.049	0.045	0.028	0.028	0.017	0.013
Proportion taking modified reading test in 8 th grade	0.041	0.039	0.046	0.044	0.029	0.028	0.016	0.014

Variable	GPA >= 2.0 and < 2.5		GPA >= 2.5 and < 3.0		GPA >= 3.0 and < 3.5		GPA >= 3.5 and <=4.0	
	CBS eligible (N=3,117)	Comparison group (N=3,117)	CBS eligible (N=3,617)	Comparison group (N=3,617)	CBS eligible (N=2,940)	Comparison group (N=2,940)	CBS eligible (N=1,881)	Comparison group (N=1,881)
Criminal justice system involvement								
Proportion with felony charge prior to HS completion	0.015	0.016	0.007	0.007	0.005	0.004	0.002	0.002
Proportion with misdemeanor charge prior to HS completion	0.155	0.154	0.108	0.107	0.068	0.065	0.043	0.039
Proportion with misdemeanor conviction prior to HS completion	0.139	0.138	0.100	0.097	0.061	0.059	0.038	0.035
School characteristics								
7 th grade school pledge eligible rate	0.606	0.602	0.601	0.601	0.586	0.583	0.559	0.565
7 th grade school average standardized math test scores	-0.157	-0.151	-0.146	-0.144	-0.117	-0.119	-0.072	-0.082
7 th grade school average standardized reading test scores	-0.098	-0.091	-0.089	-0.089	-0.074	-0.077	-0.048	-0.057

Exhibit A18

Effects of CBS Eligibility, by GPA Category

Variable	GPA ≥ 2.0 and < 2.5			GPA ≥ 2.5 and < 3.0			GPA ≥ 3.0 and < 3.5			GPA ≥ 3.5 and ≤ 4.0		
	Comparison mean	Effect	SE	Comparison mean	Effect	SE	Comparison mean	Effect	SE	Comparison mean	Effect	SE
Enrollment												
Proportion enrolling in any college on time	0.34	0.067***	0.013	0.48	0.062***	0.014	0.64	0.050***	0.014	0.70	0.050**	0.015
Proportion enrolling in 2-year college on time	0.33	0.049***	0.013	0.41	-0.005	0.012	0.40	0.000	0.013	0.32	-0.024	0.016
Proportion enrolling in 4-year college on time	0.01	0.019***	0.004	0.09	0.069***	0.009	0.26	0.052***	0.013	0.44	0.068***	0.017
Credits earned												
Cumulative credit hours earned one year after high school completion	4.98	1.444***	0.333	9.99	2.310***	0.442	18.39	1.857***	0.556	26.04	2.222**	0.713
Cumulative credit hours earned two years after high school completion	9.25	2.800***	0.639	19.05	3.540***	0.795	34.86	3.142**	1.157	49.91	4.558**	1.501
Cumulative credit hours earned three years after high school completion	11.18	3.667***	0.817	23.90	4.911***	1.127	44.38	4.145*	1.677	66.88	6.312**	2.094
Cumulative credit hours earned four years after high school completion	10.97	2.586*	1.191	23.40	5.273***	1.581	47.16	6.396**	2.459	76.63	10.143**	3.504
Persistence												
Proportion enrolling in two consecutive years of college	0.22	0.046***	0.012	0.35	0.043***	0.012	0.52	0.027	0.016	0.61	0.048**	0.018
Proportion enrolling in three consecutive years of college	0.13	0.041***	0.010	0.25	0.024*	0.011	0.40	0.011	0.015	0.52	0.042*	0.018
Proportion enrolling in four consecutive years of college	0.08	0.022*	0.010	0.15	0.025*	0.011	0.27	0.028	0.016	0.42	0.039	0.021
Graduation												
Proportion who graduated with 2-year degree within two years of on-time high school completion	0.01	0.003	0.002	0.03	0.003	0.004	0.08	0.002	0.007	0.11	0.026*	0.011
Proportion who graduated with 2-year degree within three years of on-time high school completion	0.02	0.013**	0.005	0.07	0.004	0.006	0.14	0.009	0.009	0.18	0.018	0.013
Proportion who graduated with 4-year degree within four years of on-time high school completion	s	0.002	0.002	0.02	0.004	0.004	0.08	0.025*	0.011	0.22	0.081***	0.021
Course taking and achievement												
Proportion who ever take a remedial math course in college	0.29	0.001	0.012	0.37	-0.011	0.013	0.34	-0.015	0.013	0.19	-0.024	0.013
Proportion who ever take a remedial English course in	0.20	0.005	0.011	0.23	-0.011	0.011	0.18	-0.013	0.010	0.08	-0.013	0.009

Variable	GPA \geq 2.0 and $<$ 2.5			GPA \geq 2.5 and $<$ 3.0			GPA \geq 3.0 and $<$ 3.5			GPA \geq 3.5 and \leq 4.0		
	Comparison mean	Effect	SE	Comparison mean	Effect	SE	Comparison mean	Effect	SE	Comparison mean	Effect	SE
GPA at end of 1 st year of college (2-year college)	2.01	-0.029	0.049	2.36	0.001	0.038	2.75	0.004	0.039	3.32	-0.019	0.050
GPA at end of 2 nd year of college (2-year college)	2.21	0.042	0.058	2.51	0.012	0.037	2.85	0.033	0.040	3.33	-0.024	0.051
GPA at end of 1 st year of college (4-year college) [^]	-	-	-	2.29	-0.242*	0.099	2.44	-0.008	0.051	2.89	0.068	0.040
GPA at end of 2 nd year of college (4-year college) [^]	-	-	-	2.42	-0.027	0.086	2.55	0.016	0.056	2.96	0.064	0.041
Financial aid												
State Need Grant dollars received in 1 st on-time year of college	\$281	\$230***	\$38	\$700	\$636***	\$65	\$1,554	\$1,074***	\$95	\$2,416	\$1,352***	\$142
State Need Grant dollars received in 2 nd on-time year of college	\$315	\$180***	\$37	\$782	\$365***	\$62	\$1,794	\$569***	\$110	\$2,880	\$1,165***	\$155
Loan dollars received in 1 st on-time year of college	\$164	\$66*	\$30	\$496	\$175**	\$55	\$1,130	\$59	\$80	\$1,229	\$142	\$96
Loan dollars received in 2 nd on-time year of college	\$179	\$24	\$32	\$500	\$48	\$54	\$1,233	\$138	\$87	\$1,415	-\$220*	\$104
Need-based aid dollars received in 1 st on-time year of college	\$1,454	\$1,153***	\$122	\$3,107	\$2,064***	\$180	\$6,051	\$2,530***	\$274	\$8,801	\$3,247***	\$342
Need-based aid dollars received in 2 nd on-time year of college	\$1,352	\$764***	\$121	\$2,933	\$1,383***	\$175	\$5,910	\$1,596***	\$287	\$8,983	\$2,756***	\$387

Note:

[^] We do not report results these outcomes due to small sample size (N<150). Cells with an "s" are suppressed in accordance with reporting standards for cells with small samples.

Effects of Scholarship Eligibility using Regression Discontinuity

As an additional check of the robustness of our findings regarding the effect of scholarship eligibility for CBS-eligible students, we use a regression discontinuity design (RDD) to compare students who graduate with GPAs within a narrow band around the established GPA cutoff of 2.0.

The regression discontinuity design takes advantage of the fact that students on either side of the 2.0 high school GPA cutoff can be assumed to be similar not only in the observable characteristics, such as those controlled for in matching, but also for unobserved characteristics as well, approximating the results from a random assignment study. This element of the design means that the estimates from an RDD will have greater internal validity, or account for more bias, than propensity score matching. For example, students who graduate with a high school GPA of 2.01 are CBS eligible, but these students may be similar in all other ways, including ways we do not observe, to students who graduate with a high school GPA of 1.99. However, an important limitation to this approach is that we can only confidently determine the effects of the program for students with a GPA near 2.0. Our earlier results separating our PSM results by GPA category demonstrated some heterogeneity in effects across GPA categories meaning that the RDD does not produce estimates that would apply to all students.

Sample and Data

For the regression discontinuity design, we use the administrative data with most of the restrictions described in our scholarship eligibility analysis: CBS-eligible students observed in 7th, 8th, and 12th grades who signed the pledge, graduated high school, were FRL eligible in 12th grade, and had no felony convictions between pledge signing and high school completion.⁵⁵ Different from our sample for that analysis, we do not restrict the sample to students with at least a 2.0 GPA because we use students with a GPA below 2.0 to identify the effect of the scholarship. Our sample includes students from Cohort Three to Cohort Six.

Methodology

With an RDD, we identify treatment and comparison groups based on eligibility cutoffs. The CBS program is particularly suited to this type of design because only students with a GPA above 2.0 (the cutoff) are eligible for the scholarship. We define a “treatment” group as those who have a high school GPA at or just above 2.0. The “comparison” group includes those with a GPA just below 2.0. As with a randomized experiment, we do not expect students just above and below the 2.0 GPA cutoff to vary in systematic ways that would affect the outcomes; in fact, RDD assumes that the only difference between students just above and below the cutoff is their CBS eligibility. Thus, we can determine the effect of CBS eligibility by comparing outcomes for these two groups.⁵⁶ Because we are defining eligibility as our treatment of interest (rather than receipt, which is conditional on enrollment), we have defined a sharp discontinuity; 100% of those above 2.0 in our sample are CBS eligible, by definition, and 100% of those below 2.0 should be ineligible.

Regression discontinuity analysis is restricted to individuals falling narrowly above and below the critical threshold. The exact bandwidth choice involves a trade-off between selection bias and precision; a larger bandwidth includes more observations increasing precision but may result in more dissimilarities between the groups as students far from the cutoff are included.⁵⁷ We test the results across a number of

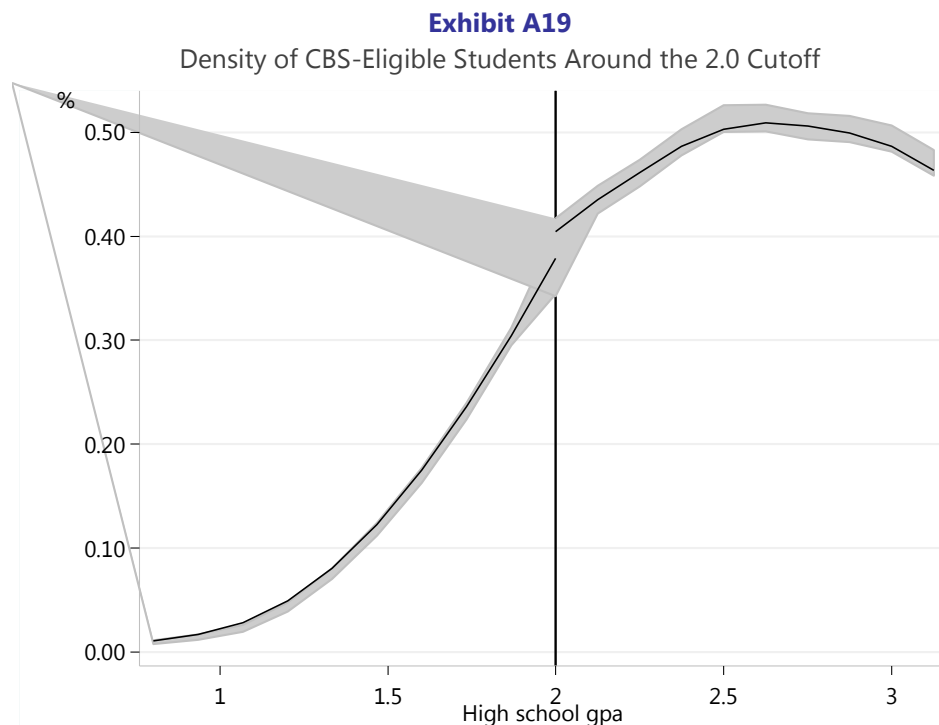
⁵⁵ See main report footnote 19 for a discussion of the FRL criteria.

⁵⁶ Jacob, R., Zhu, P., Somers, M.A., & Bloom, H. (2012). *A practical guide to regression discontinuity*. New York: MDRC.

⁵⁷ Cattaneo, M., Idrobo N., & Titiunik R. (2018). *A practical introduction to regression discontinuity designs: Volume I*. Cambridge Elements: Quantitative and Computational Methods for Social Science, Cambridge University Press.

specifications, but the results presented here are multiples of the bandwidths determined by a local linear RD estimator estimated with a triangular kernel function to determine weights.⁵⁸ The command “rdrobust” in Stata implements these choices and calculates a bandwidth that minimizes the mean squared error (MSE).⁵⁹ This method presents a data-driven way to choose between the bias-variance tradeoff. We use high school level cluster-robust standard errors in estimations.

A concern about obtaining unbiased estimates is the possibility of strategic behavior on the part of students or others in manipulating their high school GPA to be CBS eligible upon graduation. This behavior would appear in the data as an unusually high number of students with a GPA just above a GPA of 2.0. Research by Page, Iriti, Lowry, and Anthony (2018) used similar GPA dependent techniques to evaluate a place-based early promise program and did not find evidence of manipulation.⁶⁰ The data were examined for evidence of such strategic behavior with the local polynomial density estimators of Cattaneo, Jansson, and Xingei (2017) as implemented in their rddensity command in Stata.⁶¹ We find no evidence of manipulation using this test, which is illustrated in the probability density in [Exhibit A19](#). Although a small discontinuity in the density of students above the 2.0 cutoff, the discontinuity is not statistically significant.



Another threat to the validity of an RDD is the question of whether students near the cutoff are similar on both sides.⁶² Students need not be identical, but there should not be a discontinuity in characteristics at the cutoff. We performed our RD analysis with each covariate standing in as an outcome to test whether we observed a significant discontinuity around the 2.0 GPA cutoff. Descriptive statistics and the result of

⁵⁸ Ibid.

⁵⁹ Ibid

⁶⁰ Page, L.C., Iriti, J.E., Lowry, D.J., & Anthony, A.M. (2018). The promise of place-based investment in postsecondary access and success: Investigating the impact of the Pittsburgh Promise. *Education Finance and Policy*, 1-60.

⁶¹ Cattaneo, M., Jansson, M., & Xingei, M. (2017). *Simple local polynomial density estimators*. Working paper.

⁶² Skovron, C., & Titiunik, R. (2015). A practical guide to regression discontinuity designs in political science.

the discontinuity test of student characteristics are presented in [Exhibit A20](#). Eighth grade math test scores failed this test and revealed a discontinuity around the cutoff. Although we would ideally not observe any significant outcomes, we continue to report the results of our non-covariate adjusted regressions as they are similar to those with covariate adjustments.

Exhibit A20

Characteristics of Sample for Regression Discontinuity Design

Variable*	Below 2.0 cutoff (N=3,909)		Above 2.0 cutoff (N=6,141)		Weighted discontinuity test for balance
	Proportion/ mean	SD	Proportion/ mean	SD	Coefficient
Proportion female	0.42	0.49	0.47	0.50	-0.02
Proportion Black	0.10	0.30	0.10	0.30	0.00
Proportion Hispanic	0.42	0.49	0.39	0.49	0.00
Proportion Asian	0.03	0.18	0.05	0.21	0.00
Proportion American Indian/Alaska Native	0.02	0.14	0.02	0.13	0.00
Proportion other race	0.12	0.32	0.10	0.31	-0.02
Age in 12 th grade	18	0.70	18	0.59	0.01
Proportion English language learner	0.07	0.26	0.06	0.23	-0.02
Proportion with primary language non-English	0.36	0.48	0.33	0.47	0.00
Proportion with home language non-English	0.35	0.48	0.33	0.47	0.00
Proportion with disability	0.17	0.38	0.16	0.36	0.01
Proportion who received academic assistance in 12 th grade	0.24	0.43	0.18	0.38	-0.04
Proportion in gifted program in 12 th grade	0.01	0.08	0.01	0.10	0.00
Proportion retained in 8 th grade	0.01	0.09	0.01	0.08	0.00
Proportion retained in 7 th grade	0.00	0.06	0.00	0.06	0.00
Standardized math score in 8 th grade	-0.59	0.78	-0.45	0.77	0.11**
Standardized reading score in 8 th grade	-0.50	0.92	-0.35	0.89	-0.01
Proportion taking modified math test in 8 th grade	0.05	0.21	0.05	0.21	-0.01
Proportion taking modified reading test in 8 th grade	0.04	0.20	0.04	0.20	0.00

Notes:

* Statistics are reported for GPAs in the range of 1.65 to 2.35. Because each outcome generates a unique bandwidth for calculations, we report a representative bandwidth of 0.35.

** We tested differences in covariates on either side of the cutoff using various bandwidths. We found a relationship between math test score and the cutoff.

We also conducted a placebo test of outcomes in which we reran our regressions at different GPA thresholds where there was not a change in eligibility for the CBS. Some of these tests returned coefficients of marginal statistical significance, but not more than expected given the large number of placebo tests that were performed. As a result, we are confident that there is indeed a unique cutoff at the 2.0 GPA.

Another key assumption of regression discontinuity is that no other policy changes occur around the 2.0 GPA cutoff that could confound the treatment effect. If such is the case, we cannot assume that the CBS scholarship eligibility is responsible for the observed results. A known co-occurring policy change is that 4-year institutions require a 2.0 GPA as a condition of admission. To test this counterfactual, we investigate the effect of the 2.0 GPA cut-off among a similar group of students (high school graduates, no felony in high school, and free- or reduced-lunch eligible in 12th grade) among the cohorts before the implementation of the College Bound Scholarship. If the GPA policy for 4-year colleges confounds our analysis, we would expect an effect around the 2.0 GPA for students even without CBS available. Our tests of the RDD in the pre-period do not reveal systematic effects around a GPA of 2.0.

Students we define as ineligible based on our available data may actually receive CBS dollars, raising questions about the completeness of our data and our ability to identify all eligible students based on available data. To evaluate this potential concern, we expanded our treatment group to include those who receive CBS along with those who were eligible to receive CBS. The results did not differ significantly.

In addition to our optimized bandwidth choice, we tested a variety of bandwidths, quadratic functional forms, covariate adjustments, collapsing the dataset by GPA, and regressions that included school-fixed effects. We additionally restricted our analysis to only FAFSA filers (results were generally larger in magnitude but similar) and performed a donut-RD, which excludes students with GPAs between 1.99 and 2.01 (results were smaller in magnitude but similar). Results of these numerous analyses are not shown here but are available from the authors upon request. In nearly all cases, they were substantially similar to the findings reported. [Exhibit A21](#) below displays the results of our preferred RD specification at the optimal bandwidth for that outcome as well as at bandwidths of half and twice the size for sensitivity.

Exhibit A21

Scholarship Eligibility Effects Estimated with a Regression Discontinuity Design

Outcome	Half bandwidth	Chosen bandwidth	Twice bandwidth	N of chosen bandwidth	Total N in sample
Enrollment					
Proportion enrolling in any college on time	0.045**	0.056**	0.081**	11,011	39,501
Proportion enrolling in 2-year college on time	0.052**	0.052*	0.069*	10,869	39,501
Proportion enrolling in 4-year college on time	0.002	0.011*	0.014^	7,241	39,501
Credits earned					
Cumulative credit hours earned one year after high school completion	0.594^	0.975*	1.284*	13,202	39,499
Cumulative credit hours earned two years after high school completion	1.400*	2.422**	3.626**	9,838	27,183
Cumulative credit hours earned three years after high school completion	1.489	2.376	3.333	5,522	16,734
Cumulative credit hours earned four years after high school completion	-3.140*	-1.966	-2.160	3,248	8,436
Persistence					
Proportion enrolling in two consecutive years of college	0.036*	0.040*	0.054*	10,174	27,444
Proportion enrolling in three consecutive years of college	0.034*	0.033^	0.036	5,759	16,874
Proportion enrolling in four consecutive years of college	-0.000	-0.003	-0.002	2,959	8,507
Graduation					
Proportion who graduated with 2-year degree within two years of on-time high school completion	-0.004	0.000	0.006	5,177	26,348
Proportion who graduated with 2-year degree within three years of on-time high school completion	-0.003	0.004	0.017	4,432	16,624
Proportion who graduated with 4-year degree within four years of on-time high school completion	0.001	0.004	-0.002	1,522	7,870
Course taking and achievement					
Proportion who ever take a remedial math course in college	0.036**	0.029^	0.026	10,926	39,501
Proportion who ever take a remedial English course in college	0.029**	0.028^	0.030	10,901	39,501
GPA at end of 1 st year of college (2-year college)	-0.001	-0.087	-0.078	2,255	12,139
GPA at end of 2 nd year of college (2-year college)	-0.022	-0.113	-0.072	890	6,483

Note:

^ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

The RDD results differ in many ways from our main analysis of the overall effects of scholarship eligibility using propensity score matching. This disparity is not unexpected, as our analyses by GPA category revealed evidence of heterogeneous results across GPA ranges and our RDD specification typically included students with GPAs within the 1.5 to 2.5 GPA range. A more apt comparison is of effects using the RD approach and effects using PSM for those with a GPA between 2.0 and 2.5. Results for this subset are generally similar to our RD results. The most notable difference is that in our analysis with the RDD, we find no evidence of an increase in the proportion of who graduate with a 2-year degree within three years.

Scholarship Receipt Study Groups

To evaluate the effects of CBS receipt, we focus on a sample of students who receive CBS or would have been eligible to receive CBS in their first year of college. We first combine all college students in this sample, but because students who enroll in 2-year colleges may differ from those who enroll in 4-year institutions, we also conduct separate analyses for students at 2-year institutions and at 4-year institutions. We use the same treatment and comparison group definitions for each institution type as well as for the combined analysis. Students must be observed in 7th and 8th grade as required for our pledge analysis sample. For the scholarship analysis, we also require students to be observed in 12th grade because we use cumulative GPA in 12th grade to identify scholarship eligibility. As with the analysis of scholarship eligibility, we limit our analysis to students in the first four cohorts.

The treatment group, which we also refer to as the CBS recipients, includes any student in our sample who enrolls in college on-time and receives CBS dollars in their first year of college. CBS recipients come from the first two post-period cohorts. Our definition of CBS receipt undercounts the total number of CBS students. Some CBS-eligible students may receive funds equivalent to their full CBS award amount from other state aid sources and never receive CBS dollars. Our attempts to identify students receiving their full CBS award amount using financial aid data indicated about 15%-25% of students who seemed to receive fewer funds than their full CBS award amount. We could not determine why our attempts had such a high error rate (all eligible CBS students should receive their full award amount), but we decided that error rate was too high to use our calculated full CBS award from all state aid sources to identify CBS recipients. Thus, we define CBS recipients as those receiving funding from the CBS program. Because the State Need Grant constitutes the largest state aid program, and CBS awards exceed SNG awards, we assume that most students who are eligible for CBS will receive some CBS dollars.

We draw the comparison group from a pool of students in the pre-period cohorts who were pledge eligible, graduated high school with a regular or Individualized Education Plan (IEP) diploma and at least a 2.0 GPA, had no felony convictions prior to high school completion, enrolled in college on time, and received need-based aid. Students must also have a family income at or below 65% of the state MFI. Again, we use the pre-period students in an effort to identify a group of students who would be CBS recipients if they were in the post-period cohorts, but we test the robustness of this decision using a contemporaneous comparison group.

Scholarship Receipt Propensity Score Matching

We again match on the propensity score as defined in Equation (3) (on pg. 62) using listwise deletion to drop students with missing data. [Exhibit A22](#) below reports the results from the first stage model estimating the likelihood of CBS receipt for students first attending 2-year and 4-year institutions and all college students combined. We include demographic characteristics, academic characteristics, criminal justice system involvement, and family income as a percent of the state MFI. We use this model to calculate the propensity to be in the treatment group; that is the probability that a student would receive CBS in the post-period based on the student's background characteristics.

Exhibit A22

Logit Model Estimating the Likelihood of Being in Treatment Group

Variable	All college students			2-year college students			4-year college students		
	Coefficient	SE	P-value	Coefficient	SE	P-value	Coefficient	SE	P-value
Female	0.027	0.035	0.446	0.018	0.045	0.692	0.098	0.055	0.076
Black	0.283	0.063	0.000	0.199	0.083	0.017	0.116	0.098	0.238
Hispanic	0.338	0.046	0.000	0.307	0.057	0.000	0.244	0.078	0.002
Asian	-0.038	0.056	0.497	-0.015	0.076	0.842	-0.095	0.083	0.250
American Indian/Alaska Native	-0.159	0.134	0.237	-0.141	0.168	0.400	-0.328	0.217	0.131
other race	0.807	0.073	0.000	0.841	0.091	0.000	0.622	0.115	0.000
Age in 12 th grade	-0.045	0.042	0.278	-0.069	0.051	0.179	0.033	0.069	0.639
English language learner	-0.105	0.119	0.380	0.006	0.132	0.963	-0.408	0.278	0.143
Primary language non-English	0.342	0.057	0.000	0.461	0.075	0.000	0.222	0.085	0.009
Home language non-English	-0.170	0.055	0.002	-0.250	0.073	0.001	-0.089	0.083	0.284
Disability	0.223	0.089	0.012	0.307	0.096	0.001	0.347	0.251	0.166
Received academic assistance in 12 th grade	-0.067	0.062	0.282	-0.056	0.071	0.432	0.055	0.128	0.670
In gifted program in 12 th grade	0.624	0.092	0.000	0.785	0.153	0.000	0.446	0.112	0.000
GPA in 12 th grade	0.099	0.038	0.008	0.021	0.047	0.651	-0.260	0.072	0.000
Participate in Running Start	0.066	0.041	0.107	0.079	0.054	0.142	0.090	0.060	0.133
Had felony charge prior to HS completion	0.768	0.182	0.000	0.706	0.205	0.001	1.197	0.407	0.003
Had misdemeanor charge prior to HS completion	-0.237	0.174	0.173	-0.260	0.206	0.208	-0.208	0.303	0.492
Had misdemeanor conviction prior to HS completion	0.068	0.181	0.708	0.067	0.214	0.754	0.137	0.318	0.666
Standardized math score in 8 th grade	-0.007	0.027	0.794	0.008	0.035	0.816	-0.070	0.043	0.102
Standardized reading score in 8 th grade	0.068	0.024	0.006	0.077	0.031	0.012	0.049	0.039	0.208
Took modified math test in 8 th grade	0.074	0.210	0.723	0.045	0.221	0.838	0.679	0.734	0.355
Took modified reading test in 8 th grade	-0.004	0.218	0.985	-0.055	0.229	0.811	0.947	0.819	0.248
Free- or reduced-lunch in 7 th grade	-1.055	0.064	0.000	-0.956	0.080	0.000	-1.344	0.104	0.000
Free- or reduced-lunch in 8 th grade	-0.388	0.075	0.000	-0.349	0.094	0.000	-0.389	0.118	0.001
Free- or reduced-lunch in 9 th grade	-0.025	0.070	0.726	0.018	0.089	0.844	-0.060	0.111	0.587
Free- or reduced-lunch in 10 th grade	-0.112	0.066	0.092	-0.125	0.083	0.132	-0.101	0.107	0.343
Free- or reduced-lunch in 11 th grade	0.515	0.064	0.000	0.458	0.079	0.000	0.607	0.103	0.000
Free- or reduced-lunch in 12 th grade	0.388	0.061	0.000	0.323	0.077	0.000	0.504	0.095	0.000
Family income as percent of state MFI in a student's first on-time year	0.005	0.001	0.000	0.001	0.001	0.325	0.012	0.001	0.000
Constant	0.512	0.761	0.501	1.085	0.934	0.246	0.489	1.270	0.700
N	15,953			10,222			6,467		
Pseudo-R2	0.042			0.036			0.061		

Our preferred matching procedure for the main analysis is again 1:1 nearest neighbor matching without replacement with a caliper of 0.001 including ties. We exclude 346 students from the 2-year student analysis and 860 students from the 4-year student analysis because we could not find suitable matches. We checked the robustness of our matching results to analyses using propensity score weighting, which excludes considerably fewer treatment group students. We discuss the robustness of our results below.

We again evaluate our matching procedure beginning with the propensity score distributions. They almost entirely overlap for all students and those at both 2-year and 4-year institutions after matching (Exhibits A23, A24, and A25).

Exhibit A23

Distribution of the Propensity Scores Before and After Matching for Students Enrolling Any College

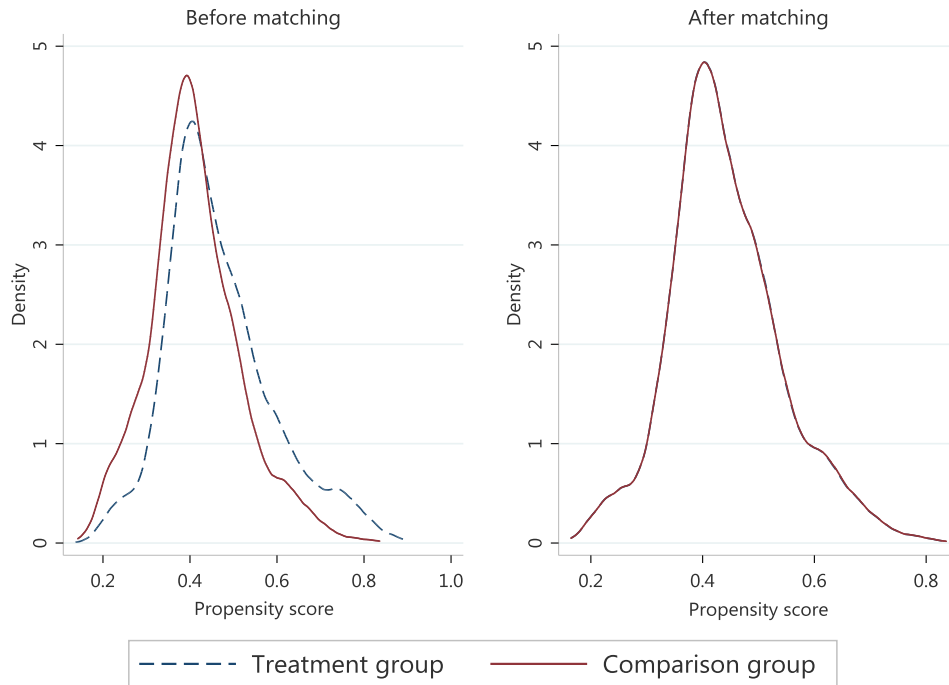


Exhibit A24

Distribution of the Propensity Scores Before and After Matching for Students Enrolling in 2-Year Institutions

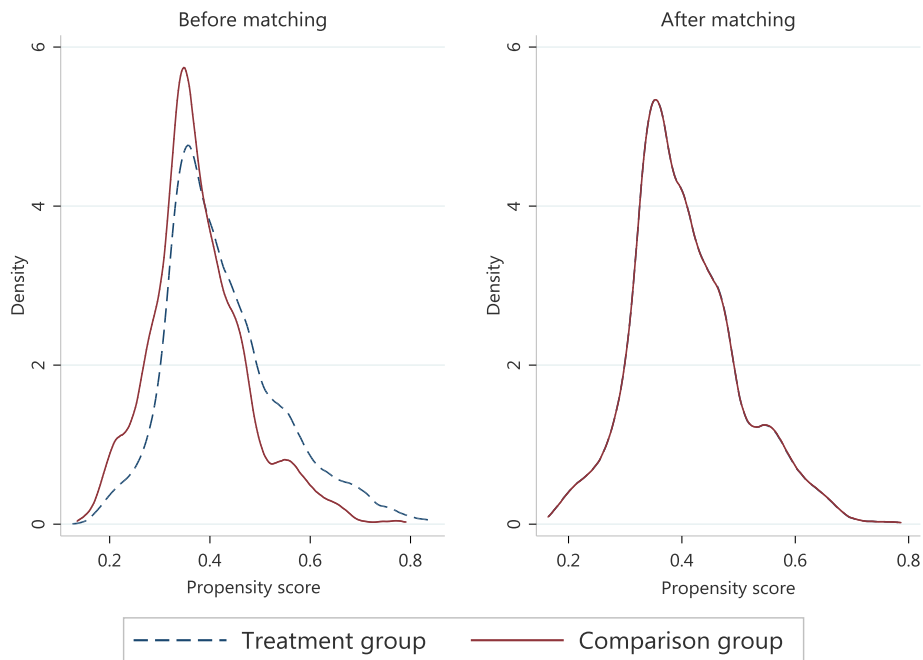
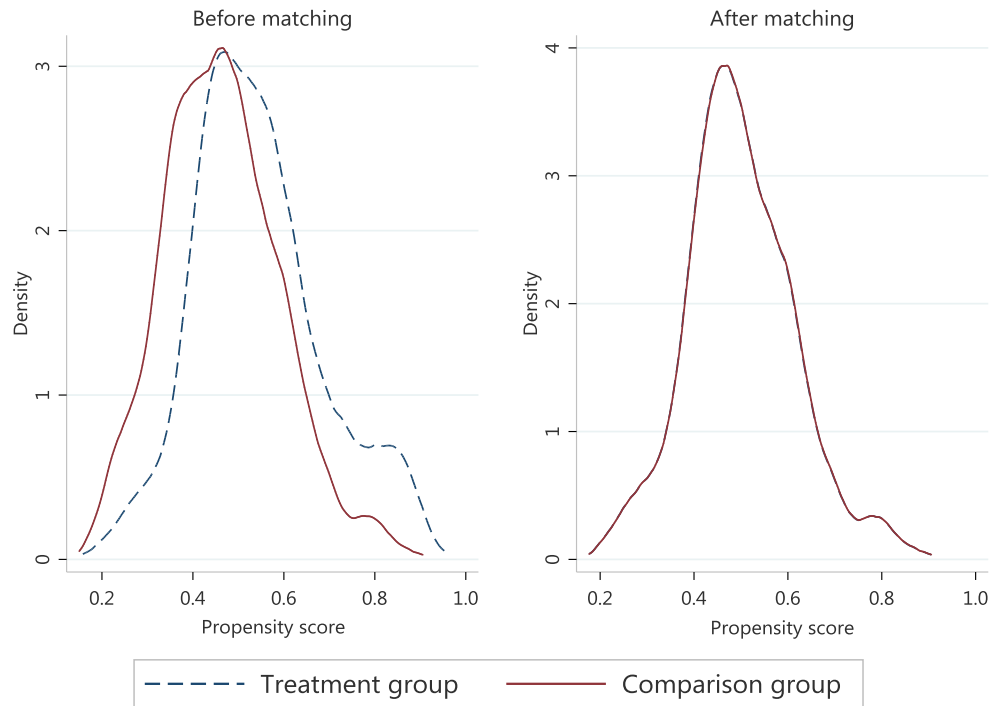


Exhibit A25

Distribution of the Propensity Scores Before and After Matching for Students Enrolling in 4-Year Institutions



Exhibits A26, A27, and A28 display balance measures for all college students and then separately for students first attending 2-year and 4-year institutions, respectively. We report the standardized percent difference for each covariate in the propensity score model before and after matching as well as the p-values for reference. After matching, most differences were reduced and the standardized difference for all covariates fell below 10.

Exhibit A26

Study Groups Characteristics Before and After Matching for All College Students

Variable	Before matching				After matching			
	CBS recipient (N=6,955)	Comparison group (N=8,998)	p-value	Standardized difference (absolute value)	CBS recipient (N=6,183)	Comparison group (N=6,183)	p-value	Standardized difference (absolute value)
Propensity score	0.467	0.412	0.000	48.453	0.442	0.442	0.903	0.219
Proportion female	0.603	0.597	0.411	1.312	0.601	0.601	0.985	0.033
Proportion White	0.409	0.473	0.000	12.999	0.417	0.422	0.585	0.983
Proportion Black	0.087	0.084	0.553	0.949	0.090	0.090	0.950	0.113
Proportion Hispanic	0.284	0.239	0.000	10.286	0.289	0.286	0.691	0.715
Proportion Asian	0.128	0.141	0.024	3.605	0.130	0.129	0.872	0.289
Proportion American Indian/Alaska Native	0.013	0.020	0.001	5.270	0.014	0.013	0.590	0.969
Proportion other race	0.079	0.043	0.000	14.895	0.059	0.060	0.849	0.342
Age in 12 th grade	17.851	17.860	0.159	2.245	17.856	17.854	0.808	0.437
Proportion English language learner	0.021	0.022	0.662	0.696	0.022	0.024	0.473	1.291
Proportion with primary language non-English	0.318	0.278	0.000	8.706	0.322	0.322	0.954	0.104
Proportion with home language non-English	0.362	0.342	0.008	4.217	0.372	0.371	0.941	0.134
Proportion with disability	0.054	0.049	0.152	2.297	0.053	0.053	1.000	0.000
Proportion who received academic assistance in 12 th grade	0.078	0.082	0.311	1.615	0.082	0.082	0.948	0.118
Proportion in gifted program in 12 th grade	0.047	0.024	0.000	12.380	0.033	0.034	0.842	0.360
GPA in 12 th grade	3.062	3.027	0.000	6.848	3.047	3.049	0.877	0.277
Proportion participating in Running Start	0.237	0.223	0.040	3.284	0.227	0.232	0.535	1.115
Proportion with felony charge prior to HS completion	0.012	0.007	0.001	5.491	0.008	0.008	0.614	0.907
Proportion with misdemeanor charge prior to HS completion	0.079	0.093	0.001	5.071	0.080	0.081	0.921	0.178
Proportion with misdemeanor conviction prior to HS completion	0.071	0.083	0.005	4.491	0.072	0.073	0.890	0.249
Standardized math score in 8 th grade	0.141	0.110	0.023	3.615	0.117	0.115	0.871	0.293
Standardized reading score in 8 th grade	0.252	0.190	0.000	7.357	0.225	0.220	0.748	0.579

Variable	Before matching				After matching			
	CBS recipient (N=6,955)	Comparison group (N=8,998)	p- value	Standardized difference (absolute value)	CBS recipient (N=6,183)	Comparison group (N=6,183)	p- value	Standardized difference (absolute value)
Proportion taking modified math test in 8 th grade	0.017	0.014	0.242	1.877	0.016	0.016	0.829	0.389
Proportion taking modified reading test in 8 th grade	0.015	0.013	0.278	1.739	0.014	0.015	0.820	0.408
Proportion free- or reduced-lunch in 7 th grade	0.862	0.942	0.000	27.333	0.920	0.920	0.894	0.239
Proportion free- or reduced-lunch in 8 th grade	0.890	0.931	0.000	14.479	0.933	0.930	0.434	1.408
Proportion free- or reduced-lunch in 9 th grade	0.870	0.884	0.012	4.025	0.895	0.895	0.977	0.053
Proportion free- or reduced-lunch in 10 th grade	0.856	0.850	0.319	1.591	0.870	0.872	0.788	0.483
Proportion free- or reduced-lunch in 11 th grade	0.868	0.815	0.000	14.690	0.873	0.874	0.787	0.487
Proportion free- or reduced-lunch in 12 th grade	0.878	0.831	0.000	13.537	0.883	0.883	1.000	0.000
Family income as percent of state MFI in student's first on-time year	30	29	0.000	8.083	30	30	0.959	0.092

Exhibit A27

Study Groups Characteristics Before and After Matching for Students Enrolling in 2-Year Institutions

Variable	Before matching				After matching			
	CBS recipient (N=4,052)	Comparison group (N=6,170)	p-value	Standardized difference (absolute value)	CBS recipient (N=3,706)	Comparison group (N=3,706)	p-value	Standardized difference (absolute value)
Propensity score	0.425	0.378	0.000	44.869	0.406	0.406	0.941	0.173
Proportion female	0.603	0.603	1.000	0.000	0.607	0.612	0.703	0.885
Proportion White	0.441	0.508	0.000	13.514	0.450	0.457	0.544	1.409
Proportion Black	0.077	0.078	0.859	0.359	0.079	0.081	0.732	0.796
Proportion Hispanic	0.290	0.245	0.000	10.123	0.294	0.290	0.702	0.890
Proportion Asian	0.099	0.107	0.215	2.499	0.100	0.097	0.640	1.085
Proportion American Indian/Alaska Native	0.014	0.020	0.017	4.748	0.015	0.015	0.923	0.224
Proportion other race	0.079	0.042	0.000	15.655	0.061	0.060	0.770	0.679
Age in 12 th grade	17.861	17.870	0.290	2.132	17.863	17.858	0.611	1.182
Proportion English language learner	0.029	0.027	0.477	1.444	0.030	0.031	0.788	0.625
Proportion with primary language non-English	0.323	0.272	0.000	11.265	0.325	0.317	0.471	1.676
Proportion with home language non-English	0.358	0.335	0.017	4.852	0.364	0.353	0.321	2.307
Proportion with disability	0.080	0.067	0.013	5.084	0.076	0.074	0.792	0.614
Proportion who received academic assistance in 12 th grade	0.099	0.101	0.729	0.699	0.101	0.099	0.757	0.720
Proportion in gifted program in 12 th grade	0.028	0.012	0.000	11.257	0.018	0.017	0.721	0.828
GPA in 12 th grade	2.888	2.876	0.231	2.433	2.884	2.887	0.794	0.606
Proportion participating in Running Start	0.234	0.218	0.065	3.740	0.224	0.222	0.823	0.519
Proportion with felony charge prior to HS completion	0.015	0.008	0.003	6.102	0.010	0.011	0.495	1.586
Proportion with misdemeanor charge prior to HS completion	0.090	0.105	0.012	5.019	0.090	0.091	0.839	0.471
Proportion with misdemeanor conviction prior to HS completion	0.082	0.094	0.028	4.427	0.081	0.082	0.932	0.197
Standardized math score in 8 th grade	-0.033	-0.048	0.367	1.822	-0.048	-0.040	0.691	0.922
Standardized reading score in 8 th grade	0.107	0.056	0.004	5.893	0.087	0.093	0.772	0.673

Variable	Before matching				After matching			
	CBS recipient (N=4,052)	Comparison group (N=6,170)	p- value	Standardized difference (absolute value)	CBS recipient (N=3,706)	Comparison group (N=3,706)	p- value	Standardized difference (absolute value)
Proportion taking modified math test in 8 th grade	0.025	0.020	0.124	3.140	0.024	0.023	0.819	0.530
Proportion taking modified reading test in 8 th grade	0.022	0.018	0.158	2.886	0.022	0.021	0.687	0.937
Proportion free- or reduced-lunch in 7 th grade	0.877	0.944	0.000	23.570	0.921	0.921	0.966	0.100
Proportion free- or reduced-lunch in 8 th grade	0.904	0.935	0.000	11.267	0.936	0.931	0.376	2.057
Proportion free- or reduced-lunch in 9 th grade	0.884	0.890	0.298	2.110	0.903	0.903	0.937	0.182
Proportion free- or reduced-lunch in 10 th grade	0.866	0.859	0.281	2.177	0.877	0.880	0.722	0.827
Proportion free- or reduced-lunch in 11 th grade	0.872	0.820	0.000	14.318	0.876	0.879	0.671	0.987
Proportion free- or reduced-lunch in 12 th grade	0.885	0.839	0.000	13.381	0.889	0.889	0.971	0.086
Family income as percent of state MFI in student's first on-time year	30	29	0.322	2.005	29	29	0.795	0.603

Exhibit A28

Study Groups Characteristics Before and After Matching for Students Enrolling in 4-Year Institutions

Variable	Before matching				After matching			
	CBS recipient (N=3,263)	Comparison group (N=3,204)	p- value	Standardized difference (absolute value)	CBS recipient (N=2,403)	Comparison group (N=2,403)	p- value	Standardized difference (absolute value)
Propensity score	0.544	0.464	0.000	59.147	0.500	0.501	0.891	0.396
Proportion female	0.610	0.588	0.077	4.396	0.596	0.602	0.659	1.273
Proportion White	0.364	0.398	0.006	6.847	0.370	0.374	0.743	0.947
Proportion Black	0.097	0.095	0.694	0.980	0.097	0.108	0.198	3.710
Proportion Hispanic	0.272	0.223	0.000	11.152	0.274	0.264	0.416	2.346
Proportion Asian	0.177	0.219	0.000	10.626	0.192	0.182	0.375	2.560
Proportion American Indian/Alaska Native	0.012	0.019	0.020	5.792	0.014	0.015	0.807	0.705
Proportion other race	0.078	0.047	0.000	13.083	0.054	0.057	0.571	1.635
Age in 12 th grade	17.834	17.837	0.786	0.676	17.833	17.842	0.395	2.455
Proportion English language learner	0.009	0.010	0.556	1.466	0.010	0.008	0.545	1.747
Proportion with primary language non-English	0.315	0.301	0.198	3.204	0.332	0.316	0.242	3.378
Proportion with home language non-English	0.372	0.369	0.794	0.649	0.392	0.377	0.286	3.079
Proportion with disability	0.018	0.010	0.006	6.884	0.010	0.012	0.490	1.992
Proportion who received academic assistance in 7 th grade	0.049	0.041	0.114	3.932	0.050	0.046	0.458	2.140
Proportion in gifted program in 12 th grade	0.073	0.048	0.000	10.185	0.057	0.057	0.950	0.179
GPA in 12 th grade	3.309	3.358	0.000	11.544	3.324	3.323	0.963	0.134
Proportion participating in Running Start	0.272	0.267	0.672	1.052	0.268	0.274	0.650	1.310
Proportion with felony charge prior to HS completion	0.009	s	0.002	7.675	s	0.004	0.284	3.088
Proportion with misdemeanor charge prior to HS completion	0.062	0.065	0.656	1.109	0.061	0.066	0.478	2.045
Proportion with misdemeanor conviction prior to HS completion	0.056	0.057	0.900	0.312	0.054	0.060	0.418	2.337
Standardized math score in 8 th grade	0.397	0.460	0.001	7.945	0.409	0.390	0.390	2.479
Standardized reading score in 8 th grade	0.471	0.481	0.594	1.326	0.468	0.451	0.461	2.129

Variable	Before matching				After matching			
	CBS recipient (N=3,263)	Comparison group (N=3,204)	p- value	Standardized difference (absolute value)	CBS recipient (N=2,403)	Comparison group (N=2,403)	p- value	Standardized difference (absolute value)
Proportion taking modified math test in 8 th grade	0.005	0.001	0.012	6.205	0.002	0.002	1.000	0.000
Proportion taking modified reading test in 8 th grade	0.004	0.001	0.013	6.154	0.001	0.001	0.655	1.291
Proportion free- or reduced-lunch in 7 th grade	0.834	0.942	0.000	34.840	0.920	0.927	0.357	2.657
Proportion free- or reduced-lunch in 8 th grade	0.866	0.923	0.000	18.603	0.928	0.922	0.411	2.374
Proportion free- or reduced-lunch in 9 th grade	0.848	0.868	0.021	5.727	0.881	0.885	0.686	1.166
Proportion free- or reduced-lunch in 10 th grade	0.835	0.829	0.529	1.564	0.851	0.859	0.436	2.245
Proportion free- or reduced-lunch in 11 th grade	0.857	0.802	0.000	14.675	0.861	0.862	0.933	0.241
Proportion free- or reduced-lunch in 12 th grade	0.864	0.811	0.000	14.327	0.865	0.869	0.671	1.226
Family income as percent of state MFI in student's first on-time year	31	27	0.000	19.066	29	30	0.683	1.176

Note:

Cells with an "s" are suppressed in accordance with reporting standards for cells with small samples.

Outcome Model

After matching, we perform regression on our matched sample—the outcome model. We use OLS regression on the matched sample to estimate the effect of CBS eligibility for continuous outcomes and OLS in the form of a linear probability model for dichotomous outcomes to estimate the marginal effects of CBS eligibility. We again use analytic standard errors clustered at the 12th-grade school level for the analysis using all college students because whether a student attends a 2-year or 4-year school is dependent on CBS receipt—i.e., it is a post-treatment variable. When we disaggregate by college, we cluster standard errors at the college level. Our outcome model uses the same covariates included in the matching model as well as fixed effects for a student's 12th-grade school for the full sample of college students or the first college attended for the models disaggregated by institution type. The regression models are weighted using the normalized weight based on the number of times a comparison group student was matched to a CBS recipient. [Exhibit A29](#) reports the full results for all college students. The main results for 2-year and 4-year college students are presented in the exhibits in [Section VI](#) of the main report.

Exhibit A29

Effects of CBS Receipt for Students Enrolling in Any College

Outcome	Comparison group mean	Effect	SE	N
Persistence				
Proportion enrolling in two consecutive years of college	0.80	0.010	0.007	12,366
Proportion enrolling in three consecutive years of college	0.60	0.010	0.010	12,365
Proportion enrolling in four consecutive years of college	0.42	0.027*	0.011	9,509
Credits earned				
Cumulative credit hours earned one year after high school completion	28.12	1.023***	0.234	12,366
Cumulative credit hours earned two years after high school completion	50.96	2.020***	0.561	12,366
Cumulative credit hours earned three years after high school completion	63.06	3.438***	0.986	12,365
Cumulative credit hours earned four years after high school completion	66.28	5.381***	1.603	9,477
Graduation				
Proportion who graduated with 2-year degree within two years of on-time HS completion	0.09	0.007	0.005	12,341
Proportion who graduated with 2-year degree within three years of on-time HS completion	0.17	0.002	0.007	12,339
Proportion who graduated with 4-year degree within four years of on-time HS completion	0.13	0.038***	0.009	9,516
Course taking and achievement				
Proportion who ever take a developmental math course in college	0.49	-0.040***	0.009	12,366
Proportion who ever take a developmental English course in college	0.28	-0.033***	0.007	12,366
GPA at end of 1 st year of college ¹	2.54	-0.039*	0.017	12,273
GPA at end of 2 nd year of college ¹	2.68	-0.009	0.015	9,928

Notes:

* p<0.05, ** p<0.01, *** p<0.001

¹ GPA measures the cumulative GPA at the highest institution level attended in a student's first or second on-time year of college. In other words, if a student attended a 4-year institution, we use the 4-year college GPA; otherwise, we use the 2-year college GPA.

Robustness Checks

For analyses of the effects of CBS receipt, a primary testable concern may be the reduction in the treatment group sample due to lack of comparison group matches. Thus, we perform propensity score weighted regression, which is useful when the comparison group is small relative to the treatment group.⁶³ Results using propensity score weighting were similar for 2-year college students. For 4-year college students, results using propensity score weighting are similar except we find a significant increase in on-time college completion using weighting. Because propensity score weighting includes more observations, the difference in significance is likely a difference in statistical power.

We also tested whether using a contemporaneous comparison group would impact our findings. Instead of using a pre-period comparison group, we drew our comparison group from a pool of students who did not receive CBS who met most of the eligibility criteria for CBS. Most of these students do not receive CBS because they did not sign the pledge. We include all four post-period cohorts and added a cohort fixed effect in this model. Overall, effects using a contemporaneous comparison group are similar but larger. We find significant positive effects of CBS receipt on persistence and similar effects on credit accumulation and degree receipt. We find no significant effects of developmental course taking unlike in our main analysis. For 2-year students, we find that CBS receipt has generally similar effects when we use a contemporaneous comparison group with the exception of developmental course participation where we find positive effects of CBS receipt compared to no effect in our main analysis. For 4-year students, we find statistically insignificant positive effects of CBS receipt on persistence and credit accumulation rather than significant negative effects as in our main analysis. These larger findings could suggest that temporal differences may lead us to underestimate the effects of CBS receipt; however, we could also observe larger effects using a contemporaneous comparison group if we fail to account for selection bias. Thus, we continue to report the results using a pre-period comparison group but acknowledge that our findings could underestimate some of the effects of CBS.

Bounding Effects for 4-year College Students

As discussed in [Section VI](#), we may be concerned that the effects of CBS receipt for students first enrolling in a 4-year institution could be affected by systematic differences in students who attend a public 4-year college in Washington because of CBS. If CBS receipt causes some students to attend public, in-state 4-year institutions who would not have done so otherwise, then these “new” students may be different from students who entered Washington public colleges or universities prior to CBS availability (the comparison group) in ways that we cannot observe. These unobservable differences could drive the estimated effects. We are particularly concerned about students who would not attend college at all or they may attend 2-year institutions rather than 4-year institutions without CBS. These students are most likely to lead to an underestimate of the effects of CBS receipt. While we cannot eliminate the bias caused by these new students, we can bound the estimated effects.⁶⁴

Because we estimate a negative effect of CBS receipt, we are most concerned that these new students will lead us to underestimate of the effect of CBS. We therefore focus on students who may have attended 2-year schools or not attended college at all rather than students who may have attended a private college or out-of-state school. This focus assumes that students who would have attended a 2-year college or never enrolled with CBS are less likely to succeed in a 4-year college than students who would have attended a 4-year school regardless of CBS receipt. We further assume that students who would have attended a 4-year private institution in Washington or an out-of-state school would not lead us to

⁶³ Steiner, P. & Cook, D. (2013). *Matching and propensity scores*. T. Little (Ed.). New York, NY: Oxford University Press.

⁶⁴ See, for example, Scott-Clayton (2011) and Lee, D.S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, 76(3), 1071-1102.

underestimate the effects of CBS receipt. If CBS causes these students to attend a 4-year public school in Washington, it is most likely that would perform as well or better than students who would attend a Washington public college or university. In that case, we would be more likely to overestimate the effects of CBS, but because we observe a negative effect of CBS, we assume this finding may be an underestimate of the effect and focus on students who may drive this negative effect. While our assumptions may be incorrect, if these new students are more likely to succeed, we would not expect to find negative effects of CBS. Thus, we assume these new students would have lower achievement and attainment levels than other students.

In [Section V](#) of the report, we estimate that the scholarship eligibility increases enrollment in in-state, public colleges or universities from 17% to 22.1%—a 5.1 percentage point increase. Because most CBS-eligible students will receive CBS dollars, we estimate that all of these new students will receive CBS. In other words, for every 22 CBS-eligible CBS recipients in a 4-year college, five of them would not be in college without CBS (23%). We use this information to bound our estimated effects of CBS receipt for 4-year college students. Although we cannot identify the 23% of students, we can assume that they constitute the bottom of the distribution for the treatment group for a given outcome and then estimate the effects of CBS receipt excluding these new students.

Following Scott-Clayton (2011), we trim the bottom 23% of students based on their first year college GPA or based on credits earned in the first year of college. We then re-estimate our propensity score and regression models. Again, by trimming the bottom of the distribution, we assume that students induced to attend 4-year schools are academically marginal students. Effects using these trimmed samples are shown in [Exhibit A30](#) alongside the effects estimated using the full sample. We repeat this analysis only trimming 12% of students to account for the fact that students at the bottom of the distribution may not drive the entire increase in enrollment we observe. Some of the new students may be students who would have attended 4-year schools but would not attend public, in-state schools; others may not have attended college or would have attended 2-year schools, but may perform just as well as students who would have attended a 4-year school without CBS. By only trimming 12% of the sample, we assume that students in the middle or top of the distribution drive half of the increase in enrollment.

Using the trimmed samples, we find that CBS receipt either has no effect or increases persistence, credit accumulation, GPA, and completion ([Exhibit A30](#)). These findings suggest that nonrandom selection may lead to an underestimate of the effects of CBS receipt. Furthermore, they suggest that students who would enroll in a 4-year college regardless of CBS may actually benefit from CBS receipt, while students who enroll because of CBS may not perform as well as other CBS recipients at 4-year colleges.

Exhibit A30

Results of Bounding Sensitivity Analysis

Outcome	All 4-year college students		Trim Bottom 23% of CBS recipients based on Credits earned by end of first year				Trim Bottom 11% of CBS recipients based on Credits earned by end of first year			
	First year GPA		First year GPA				First year GPA			
	Effect	N	Effect	N	Effect	N	Effect	N	Effect	N
Persistence										
Proportion enrolling in two consecutive years of college at 4-year institution	-0.035** (0.008)	4,805	0.054*** (0.009)	4,085	0.065*** (0.009)	4,157	0.024** (0.009)	4,557	0.031*** (0.009)	4,590
Proportion enrolling in three consecutive years of college at 4-year institution	-0.037* (0.010)	4,805	0.073*** (0.012)	4,085	0.081*** (0.012)	4,157	0.034** (0.012)	4,557	0.034** (0.012)	4,590
Proportion enrolling in four consecutive years of college at 4-year institution	-0.042 (0.025)	3,728	0.073*** (0.017)	3,150	0.093*** (0.017)	3,176	0.034* (0.016)	3,500	0.034* (0.016)	3,514
Credits Earned										
Cumulative credit hours earned one year after high school completion	-0.277 (0.139)	4,805	2.632*** (0.254)	4,085	4.055*** (0.228)	4,157	2.357*** (0.231)	4,557	2.357*** (0.231)	4,590
Cumulative credit hours earned two years after high school completion	-2.662* (0.764)	4,805	5.783*** (0.719)	4,085	7.501*** (0.704)	4,157	3.145*** (0.718)	4,557	3.145*** (0.718)	4,590
Cumulative credit hours earned three years after high school completion	-4.382* (1.554)	4,805	8.729*** (1.389)	4,085	10.418*** (1.388)	4,157	4.059** (1.363)	4,557	4.059** (1.363)	4,590
Cumulative credit hours earned four years after high school completion	-6.191 (4.410)	3,726	11.879*** (2.589)	3,148	15.151*** (2.609)	3,174	5.737* (2.499)	3,498	5.737* (2.499)	3,513
Graduation										
Proportion who graduated with 4-year degree within four years of on-time high school completion	0.023 (0.011)	3,771	0.073*** (0.016)	3,199	0.067*** (0.016)	3,229	0.049** (0.015)	3,547	0.049** (0.015)	3,572
Course taking and Achievement										
Proportion who ever take a remedial Math course in college	0.013 (0.021)	4,805	0.019* (0.009)	4,085	0.007 (0.008)	4,157	0.018* (0.008)	4,557	0.018* (0.008)	4,590
Proportion who ever take a remedial English course in college	-0.003 (0.005)	4,805	-0.006 (0.007)	4,085	-0.018** (0.006)	4,157	-0.004 (0.006)	4,557	-0.004 (0.006)	4,590
GPA at end of first year of college (4-year college)	-0.056 (0.060)	4,802	0.235*** (0.017)	4,083	0.177*** (0.018)	4,154	0.098*** (0.018)	4,554	0.098*** (0.018)	4,589
GPA at end of second year of college (4-year college)	0.012 (0.044)	4,118	0.117*** (0.019)	3,705	0.099*** (0.019)	3,787	0.054** (0.019)	4,047	0.054** (0.019)	4,091

Notes:

* p<0.05, ** p<0.01, *** p<0.001

Standard errors clustered at the college level in parentheses.

We can also perform a similar analysis where we trim the top 23% of the treatment group. Doing so would suggest that our main effects overestimate the effects of CBS receipt and that the effects are more negative for persistence and negative for GPA and completion. Because we are more concerned about underestimates resulting from inducing otherwise unlikely 4-year college students to attend college, we do not perform this bounding analysis. We can also perform a similar analysis for 2-year college students. However, we did not find a large effect of the scholarship on 2-year enrollment; we estimate that CBS eligibility induces only about 3.5% of CBS recipients at 2-year institutions to enroll in college who would not have otherwise. Thus, the potential bias in our effects for 2-year college students is most likely negligible. We also conducted similar bounding analyses for all college students. Results suggested that, while selection bias may drive some of our findings, our main results were much more robust—e.g., we continue to find positive effects on credit accumulation and degree receipt in many of our trimmed samples.

IV. Some Causes of Ineligibility

Our assignment directed us to determine “the effect of ineligibility due to factors such as entering the state after middle school or changes in family income.” We were unable to provide full analyses of these factors for various reasons.

We first attempted to estimate the effect of College Bound for those students who were not eligible to sign the College Bound pledge because they had moved to Washington after their eligibility period. To do so, we restricted our sample to students who we observe in Washington public schools in 9th grade but do not observe in 7th or 8th grade (“new 9th graders”). Importantly, we cannot determine why we do not observe students in the 7th or 8th grade. They may not be in Washington State or they may attend a private or homeschool.

Students in our third cohort were eligible to sign the pledge in 8th and 9th grade, while no other cohort was eligible to sign the pledge after 8th grade. However, our data suggest a large portion of new 9th grade students from Cohorts Four, Five, and Six signed the pledge. Close to half of new 9th grade students who signed the pledge in Cohorts Four, Five, and Six did so in 7th or 8th grade, suggesting they were in Washington schools before 9th grade—possibly private schools or homeschooled. This finding implies there may be many students who are new to our dataset in 9th grade but who were in Washington before 9th grade. In short, our best indicator of being new to Washington in 9th grade has a high error rate in accurately identifying new students. To do an unbiased analysis of the effects of (in)eligibility for students who entered Washington after middle school, we would need information to identify which students entered Washington from out of state, which we do not have.

Our assignment also asked about outcomes for students who are otherwise eligible for the scholarship but miss the 65% MFI cutoff. We attempted to evaluate the effects of CBS eligibility and receipt around the 65% MFI cutoff using a regression discontinuity design (see [Appendix III](#) for a description of this design). This analysis would inform how outcomes for students who have a family income just above 65% of the state MFI compare to CBS-eligible students with an income at or below 65% of the state MFI. To conduct this analysis, we would ideally have family income data on all pledge signers who filed a FAFSA and graduated high school with a 2.0 GPA and had no felony convictions. We could then compare outcomes for all students above and below the 65% MFI threshold. However, we were unable to obtain this income data for the full sample. Instead, we obtained data only for students enrolled in college who were receiving state need-based aid. This limited sample presents two important problems for a causal analysis of effects of CBS at the 65% MFI cutoff.

First, we cannot estimate the effect of CBS around the 65% MFI cutoff on enrollment in college. Receiving CBS may cause some students whose incomes just meet the 65% MFI to enroll, and we cannot observe that effect of CBS without income data on students prior to enrollment.

Second, if CBS does induce some students to enroll in college, they will only be present in the treatment group (by virtue of the fact the induced students will be enrollees and receiving aid). Our data for the treatment group, therefore, consists of students who would attend college regardless of CBS receipt and those who enrolled because they received CBS. For the comparison group, however, our data only includes students who would attend college regardless of CBS receipt. We cannot observe students who would attend college only if they received CBS in the comparison group because we do not have family income data for non-enrollees. If students who would attend college only if they receive CBS are different from students who would attend regardless, then the comparison group and treatment group will differ in

important ways. For example, students who attend college only if they receive CBS may put less value on a college degree and thus may be less likely to graduate, which would lower the expected graduation rate for the treatment group and increase the rate for the comparison group.

We could limit our analysis to those who enrolled in their first year of college to identify the effect of CBS in the first year at the 65% MFI cutoff on subsequent years. However, when we restrict our data to pledge signers who graduated high school with a 2.0 GPA and no felony convictions who enrolled in college and received need-based aid, our sample sizes decrease significantly making it difficult to detect any effects that might exist. In fact, WSAC reports that only 3% of enrolled students who were otherwise eligible for CBS had a family income between 66% and 70% of the state MFI, while 8% had incomes above 70% of the state MFI.⁶⁵

While data limitations caution us from proceeding with a full analysis of the effects of CBS eligibility and receipt around the 65% MFI cutoff, we can provide a description of the characteristics and outcomes for students around that cutoff. These findings can illustrate the extent to which students just missing the 65% MFI cutoff may differ from CBS-eligible students who just qualify. [Exhibit A31](#) and [A32](#) below presents the average characteristics for pledge signers who graduated high school with a 2.0 GPA and no felony convictions and who enrolled in 2-year or 4-year institutions in the first year after high school. We limit the sample to students with a family income between 60% and 70% of the state MFI. Because most aid programs require students to have a family income at or below 70% MFI, students receiving need-based aid with a family income above 70% of the state MFI may differ from students with incomes below 70% of the state MFI, limiting us to students with incomes between 60% and 70% of the state MFI. The group of CBS-eligible students includes students who are eligible to receive their full CBS award. Some of these students do not receive CBS dollars and thus are not CBS recipients as defined in [Exhibit 2](#). Those just below and just above the 65% MFI cutoff are similar on most characteristics except those related to income—FRL receipt and family income as a percent of the state MFI.

⁶⁵ WSAC. (2017).

Exhibit A31

Characteristics of Students Above and Below the 65% MFI Threshold First Attending 2-Year Institutions

Variable	CBS eligible 60%-65% of MFI			Ineligible for CBS 65%-70% of MFI			Difference	p-value
	Mean	SD	N	Mean	SD	N		
Proportion female	0.60	0.49	332	0.55	0.50	213	0.05	0.22
Proportion White	0.46	0.50	332	0.47	0.50	213	-0.01	0.79
Proportion Black	0.07	0.25	332	0.07	0.25	213	0.00	0.87
Proportion Hispanic	0.29	0.46	332	0.29	0.46	213	0.00	0.98
Proportion Asian	0.09	0.29	332	0.10	0.30	213	-0.01	0.75
Proportion American Indian/Alaska Native	0.02	0.13	332	0.00	0.07	213	0.01	0.18
Proportion other race	0.07	0.26	332	0.07	0.26	213	0.00	0.93
Proportion eligible for pledge based on FRL status in 7 th , 8 th , or 9 th grade	0.90	0.30	332	0.89	0.31	213	0.01	0.84
Proportion free- or reduced-lunch in 12 th grade	0.71	0.45	332	0.61	0.49	213	0.11	0.01
Age in 7 th grade	17.83	0.38	332	17.81	0.37	213	0.02	0.62
Proportion English language learner	0.02	0.12	329	0.03	0.17	212	-0.01	0.29
Proportion with primary language non-English	0.28	0.45	332	0.31	0.46	213	-0.03	0.50
Proportion with home language non-English	0.33	0.47	332	0.32	0.47	213	0.00	0.97
Proportion with disability	0.06	0.24	332	0.06	0.23	213	0.00	0.85
Proportion who received academic assistance in 7 th grade	0.07	0.26	332	0.07	0.25	213	0.01	0.77
Proportion in gifted program in 7 th grade	s	s	332	s	s	213	s	0.94
Standardized math score in 8 th grade	0.00	0.75	329	0.05	0.82	209	-0.05	0.49
Standardized reading score in 8 th grade	0.10	0.84	329	0.16	0.81	208	-0.05	0.47
GPA in 12 th grade	2.94	0.48	332	2.98	0.53	213	-0.04	0.36
Proportion with parent education less than high school	0.16	0.37	289	0.19	0.40	185	-0.04	0.32
Proportion with parent education of high school diploma	0.24	0.42	289	0.21	0.41	185	0.02	0.53
Proportion with parent education of some college or Associate's degree	0.44	0.50	289	0.42	0.49	185	0.02	0.62
Proportion with parent education of BA or greater	0.17	0.37	289	0.18	0.38	185	-0.01	0.73
Proportion with misdemeanor conviction prior to high school completion	0.04	0.20	332	s	s	213	0.02	0.14
Proportion with felony charge prior to high school completion	s	s	332	s	s	213	s	0.42
Proportion with misdemeanor charge prior to high school completion	0.05	0.21	332	0.02	0.15	213	0.02	0.14
Family income as percent of state MFI	62	1.41	332	67	1.45	213	-5	0.00

Notes:

Cells with an "s" are suppressed in accordance with reporting standards for cells with small samples.

Exhibit A32

Characteristics of Students Above and Below the 65% MFI Threshold First Attending 4-Year Institutions

Variable	CBS eligible 60%-65% of MFI			Ineligible for CBS 65%-70% of MFI			Difference	p-value
	Mean	SD	N	Mean	SD	N		
Proportion female	0.57	0.50	273	0.62	0.49	186	-0.05	0.32
Proportion White	0.36	0.48	273	0.39	0.49	186	-0.03	0.54
Proportion Black	0.08	0.28	273	0.11	0.31	186	-0.02	0.40
Proportion Hispanic	0.27	0.45	273	0.25	0.43	186	0.03	0.51
Proportion Asian	0.15	0.35	273	0.18	0.38	186	-0.03	0.38
Proportion American Indian/Alaska Native	s	s	s	s	s	s	s	s
Proportion other race	0.11	0.32	273	0.08	0.27	186	0.03	0.25
Proportion eligible for pledge based on FRL status in 7 th , 8 th , or 9 th grade	0.78	0.41	273	0.82	0.39	186	-0.03	0.38
Proportion free- or reduced-lunch in 12 th grade	0.63	0.48	273	0.58	0.50	186	0.06	0.21
Age in 7 th grade	17.83	0.35	273	17.82	0.35	186	0.01	0.80
Proportion English language learner	s	s	s	s	s	s	s	s
Proportion with primary language non-English	0.26	0.44	273	0.32	0.47	186	-0.06	0.15
Proportion with home language non-English	0.33	0.47	273	0.37	0.48	186	-0.04	0.36
Proportion with disability	s	s	s	s	s	s	s	s
Proportion who received academic assistance in 7 th grade	0.04	0.20	273	0.03	0.18	186	0.01	0.66
Proportion in gifted program in 7 th grade	0.05	0.23	273	0.04	0.19	186	0.02	0.40
Standardized math score in 8 th grade	0.46	0.78	268	0.49	0.84	184	-0.03	0.72
Standardized reading score in 8 th grade	0.47	0.73	268	0.55	0.82	183	-0.08	0.27
GPA in 12 th grade	3.34	0.41	273	3.37	0.42	186	-0.03	0.43
Proportion with parent education less than high school	0.14	0.35	246	0.17	0.38	164	-0.03	0.44
Proportion with parent education of high school diploma	0.20	0.40	246	0.22	0.42	164	-0.02	0.62
Proportion with parent education of some college or Associate's degree	0.45	0.50	246	0.41	0.49	164	0.04	0.47
Proportion with parent education of BA or greater	0.21	0.41	246	0.20	0.40	164	0.01	0.76
Proportion with misdemeanor conviction prior to high school completion	s	s	s	s	s	s	s	s
Proportion with felony charge prior to high school completion	s	s	s	s	s	s	s	s
Proportion with misdemeanor charge prior to high school completion	s	s	s	s	s	s	s	s
Family income as percent of state MFI	62.22	1.36	273	67.27	1.45	186	-5.05	0.00

Notes:

Cells with an "s" are suppressed in accordance with reporting standards for cells with small samples.

Some students included in the CBS eligible group do not receive CBS dollars, likely because they receive their full eligible award from other state aid programs. However, most CBS-eligible students receive CBS dollars—90% of eligible 2-year students receive CBS and 95% of eligible 4-year students. We also observe some students in the comparison group receiving CBS. This could represent measurement error in some variables. For example, we use OSPI data on GPA to determine whether a student satisfies the 2.0 GPA requirement, but we noticed some minor differences in our GPA variable from OSPI and the GPA variable

used by WSAC. In general, we find that students with family incomes above and below 65% MFI have similar average observable characteristics except for differences in a student's family income as a percent of the state MFI.

We observe few significant differences in outcomes for students with family incomes just above 65% of the state MFI and CBS-eligible students—most of whom are CBS recipients ([Exhibits A33](#) and [A34](#)). CBS eligibility may be associated with less credit accumulation or lower college completion rates, but none of these results are significant. At both 2-year and 4-year institutions, we find that CBS eligibility may be associated with the type of financial aid received with CBS-eligible students having significantly larger average SNG awards and total aid packages and smaller average loans in their first years of college.

Exhibit A33

Average Outcomes for Students Above and Below the 65% MFI Threshold First Attending 2-Year Institutions

Outcome	CBS eligible 60%-65% of MFI			Ineligible for CBS 65%-70% of MFI			Difference	p-value
	Mean	SD	N	Mean	SD	N		
Percent receiving CBS in 1 st year of college	0.90	0.30	332	0.13	0.33	213	0.78	0.00
Credits earned								
Cumulative credit hours earned through 1 st on-time year of college	23.68	14.77	332	25.30	15.56	213	-1.62	0.22
Cumulative credit hours earned through 2 nd on-time year of college	41.91	33.85	332	44.47	33.91	213	-2.56	0.39
Cumulative credit hours earned through 3 rd on-time year of college	43.34	46.21	211	51.81	45.27	134	-8.48	0.09
Cumulative credit hours earned through 4 th on-time year of college	37.48	51.29	107	33.99	52.61	67	3.49	0.67
Persistence								
Proportion enrolling in two consecutive years of college	0.74	0.44	332	0.76	0.43	213	-0.02	0.61
Proportion enrolling in three consecutive years of college	0.51	0.50	213	0.54	0.50	136	-0.04	0.50
Proportion enrolling in four consecutive years of college	0.31	0.46	107	0.22	0.42	69	0.09	0.19
Graduation								
Proportion who graduated with 2-year degree within two years of on-time HS completion	0.17	0.37	327	0.16	0.37	211	0.01	0.83
Proportion who graduated with 2-year degree within three years of on-time HS completion	0.31	0.46	229	0.39	0.49	145	-0.08	0.10
Proportion who graduated with 4-year degree within four years of on-time HS completion	S	s	107	s	s	67	s	0.24
Achievement								
GPA at end of 1 st year of college (2-year college)	2.55	0.96	329	2.66	0.89	208	-0.10	0.21
GPA at end of 2 nd year of college (2-year college)	2.72	0.81	231	2.83	0.73	152	-0.11	0.19
Financial aid receipt								
Loan dollars received in 1 st on-time year of college	\$442	\$1,349	332	\$682	\$1,946	213	-\$240	0.09
Need-based aid dollars received in 1 st on-time year of college	\$6,556	\$4,104	332	\$5,014	\$4,671	213	\$1,542	0.00
State Need Grant dollars received in 1 st on-time year of college	\$1,110	\$1,232	332	\$800	\$831	213	\$310	0.00
College Bound Scholarship dollars received in 1 st on-time year of college	\$2,453	\$1,696	332	\$294	\$891	213	\$2,159	0.00

Notes:

Cells with an "s" are suppressed in accordance with reporting standards for cells with small samples.

Exhibit A34

Average Outcomes for Students Above and Below the 65% MFI Threshold First Attending 4-Year Institutions

Outcome	CBS eligible 60%-65% of MFI			Ineligible for CBS 65%-70% of MFI			Difference (Eligible – Ineligible)	p- value
	Mean	SD	N	Mean	SD	N		
Percent receiving CBS in 1 st year of college	0.95	0.22	273	s	s	s	s	0.00
Credits earned								
Cumulative credit hours earned through 1 st on-time year of college	37.63	10.83	273	37.56	11.14	186	0.07	0.94
Cumulative credit hours earned through 2 nd on-time year of college	68.73	32.22	273	68.24	29.71	186	0.48	0.87
Cumulative credit hours earned through 3 rd on-time year of college	95.49	51.24	164	95.12	51.17	113	0.36	0.95
Cumulative credit hours earned through 4 th on-time year of college	115.19	74.91	83	118.04	80.25	64	-2.85	0.82
Persistence								
Proportion enrolling in two consecutive years of college	0.86	0.35	273	0.85	0.36	186	0.01	0.82
Proportion enrolling in three consecutive years of college	0.77	0.42	164	0.75	0.43	113	0.02	0.67
Proportion enrolling in four consecutive years of college	0.71	0.46	83	0.64	0.48	64	0.07	0.37
Graduation								
Proportion who graduated with 2-year degree within two years of on-time HS completion	0.10	0.30	269	0.09	0.28	184	0.01	0.63
Proportion who graduated with 2-year degree within three years of on-time HS completion	0.17	0.37	175	0.17	0.38	115	-0.01	0.86
Proportion who graduated with 4-year degree within four years of on-time HS completion	0.34	0.48	86	0.46	0.50	67	-0.13	0.12
Achievement								
GPA at end of 1st year of college (4-year college)	2.63	0.88	273	2.63	0.88	186	0.00	0.96
GPA at end of 2nd year of college (4-year college)	2.78	0.76	227	2.85	0.69	153	-0.06	0.42
Financial aid receipt								
Loan dollars received in 1 st on-time year of college	\$3,108	\$3,613	273	\$5,049	\$5,596	186	-\$1,941	0.00
Need-based aid dollars received in 1 st on-time year of college	\$19,003	\$5,684	273	\$17,402	\$6,510	186	\$1,602	0.01
State Need Grant dollars received in 1 st on-time year of college	\$2,668	\$2,849	273	\$261	\$978	186	\$2,407	0.00
College Bound Scholarship dollars received in 1 st on-time year of college	\$6,447	\$3,650	273	\$208	\$1,495	186	\$6,239	0.00

Notes:

Cells with an "s" are suppressed in accordance with reporting standards for cells with small samples.

V. Missing Data Examiner

For some students, we lacked data on characteristics important to our analyses. We handled missing values using case-wise deletion, excluding those students from our analyses. We excluded 46,281 students from our analysis of the pledge due to missing data (about 9.7%). We focus on missing data from the pledge analysis. We also used case-wise deletion in our scholarship analysis, but fewer students were excluded from these analyses due to missing data (about 4%-7%).

Of the 46,281 students dropped from our pledge analyses for missing student characteristic data, most (44,334) were missing 8th-grade mathematics test scores or missing 8th-grade reading test scores (44,094), and 211 were missing an indicator of their bilingual status. About 9.7% of students in our sample were dropped from our analyses due to missing data.

The sample of students dropped due to missing data differs from the sample of students with complete data ([Exhibit A35](#)). Students excluded from analyses due to missing data are more likely to be clearly pledge eligible. In terms of demographics, these students were less likely to be White and more likely to receive services for English language learners. They are also more likely to have a disability and less likely to be in a program for gifted students in 7th grade. Students with missing data were much more likely than other students to have been involved in the criminal justice system before 7th grade.

Students with incomplete data also had worse outcomes than other students. Only 36% completed high school on time, and only 15% enrolled in any college on time. Before completing high school, about 20% were convicted of a misdemeanor and about 9% were convicted of a felony.

Exhibit A35

Characteristics of Students with Incomplete Data

Variable	Students with incomplete data			Students with complete data		
	Mean	SD	N	Mean	SD	N
Pledge characteristics						
Proportion eligible for pledge based on FRL status in 7 th , 8 th , or 9 th grade	55%	0.50	46,045	45%	0.50	432,457
Proportion who signed the pledge	7%	0.26	46,045	16%	0.50	432,457
Demographic characteristics						
Proportion female	47%	0.50	46,045	49%	0.50	432,457
Proportion White	51%	0.50	46,045	65%	0.48	432,457
Proportion Black	7%	0.26	46,045	4%	0.21	432,457
Proportion Hispanic	16%	0.37	46,045	14%	0.35	432,457
Proportion Asian	5%	0.22	46,045	7%	0.26	432,457
Proportion American Indian/Alaska Native	3%	0.18	46,045	2%	0.13	432,457
Proportion other race	17%	0.38	46,045	8%	0.27	432,457
Age in 7 th grade	12.96	0.53	46,045	12.87	0.41	432,457
Proportion with primary language non-English	17%	0.38	46,045	14%	0.35	432,457
Proportion with home language non-English	20%	0.40	46,045	16%	0.37	432,457
Proportion migrant	3%	0.16	46,045	2%	0.14	432,457
Proportion with disability	16%	0.37	46,045	11%	0.32	432,457
Academic characteristics						
Proportion English language learner	8%	0.26	45,835	6%	0.23	432,457
Proportion who received academic assistance in 7 th grade	15%	0.36	46,045	16%	0.37	432,457
Proportion in gifted program in 7 th grade	2%	0.14	46,045	5%	0.21	432,457
Standardized test scores						
Proportion taking modified math test in 8 th grade	8%	0.28	1,941	3%	0.18	432,457
Proportion taking modified reading test in 8 th grade	9%	0.28	2,180	3%	0.17	432,457
Standardized math score in 8 th grade	-0.72	1.07	1,941	2%	1.00	432,457
Standardized reading score in 8 th grade	-0.74	1.16	2,180	1%	0.99	432,457
Criminal justice involvement						
Proportion with felony charge prior to 7 th grade	2%	0.15	46,045	1%	0.07	432,457
Proportion with misdemeanor charge prior to 7 th grade	6%	0.23	46,045	2%	0.13	432,457
Proportion with felony conviction prior to 7 th grade	1%	0.12	46,045	0%	0.05	432,457
Proportion with misdemeanor conviction prior to 7 th grade	4%	0.20	46,045	1%	0.11	432,457

We tested the Missing Completely at Random (MCAR) assumption in our data. This assumption states the event of missing data is independent of relevant covariates and outcomes. We also tested the Covariate-Dependent Missingness (CDM) assumption. Relaxing the MCAR assumption, the CDM assumption holds if, after conditioning on covariates, the conditional missingness is independent of other covariates and outcomes. The tests suggest the data do not satisfy MCAR or CDM.⁶⁶

A third, untestable claim is the Missing at Random (MAR) assumption. The MAR assumption, further relaxing MCAR and CDM, says data are MCAR conditional on all observed covariates, including those with missing values for some observations. In other words, the missingness depends only on observable data.

It is unlikely the MAR assumption holds in our data. Because only three student characteristics have missing values that led students to be dropped from our analysis, the CDM assumption can only be relaxed to the extent that the observed values in those three variables help explain the missing values among them. Eighth-grade math and reading test scores are nearly always missing simultaneously ($r=0.95$), leaving little variation for one to predict missingness in the other. The information available to us, from our data, indicates the MAR assumption is unreliable in the context of this study.

We considered using multiple imputation to account for missing values in our data. Multiple imputation can increase efficiency when data are MCAR by including observations that would otherwise be dropped and can reduce bias from systematic missingness when data satisfy CDM or MAR. When data are Missing Not at Random (MNAR), meaning the missing values cannot be predicted by observable characteristics, multiple imputation does not reduce bias. Multiple imputation can only reduce bias to the extent that missingness can be predicted.

If data are MNAR, multiple imputation can be more biased than case-wise deletion.⁶⁷ Case-wise deletion can produce unbiased regression estimates if predictor variables are MAR or MNAR, so long as missingness in the predictor variables is independent of the outcome variable. We must also assume the regression model is correctly specified. In this setting, missingness is analogous to measurement error or omitted variable bias.

The necessary independence of outcomes and predictor missingness is plausible in our data. Outcomes (typically) occur six or more years after 8th grade, weakening the potential for dependence on predictor missingness. And the outcomes are not censored for students based on the presence or values of 8th-grade test scores. Students can graduate from high school in Washington, for example, without completing 8th-grade standardized tests.

We believe multiple imputation is unlikely to reduce bias related to missing data in our sample, and case-wise deletion may be more robust in the context of this study. Nonetheless, we were also able to replicate findings by Goldhaber et al. (2017), which used multiple imputation, with our data, which used case-wise deletion, suggesting that missing data may not present a problem for our analysis.

⁶⁶ Little, R.J. (1988). A test of missing completely at random for multivariate data with missing values. *Journal of the American statistical Association*, 83(404), 1198-1202 and Li, C. (2013). Little's test of missing completely at random. *The Stata Journal*, 13(4), 795-809.

⁶⁷ Allison, P.D. (2001). *Missing data* (Vol. 136). Sage publications.

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