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The Effects of Washington's College Bound Scholarship Program on High School Grades, High School completion, and Incarceration

Dan Goldhaber
American Institutes for Research and University of Washington

Mark Long
University of Washington

Trevor Gratz
University of Washington

Jordan Rooklyn
University of Washington

The Center for Education Data & Research
University of Washington
3876 Bridge Way N. Ste. 201 Seattle, WA 98103
(206) 547-5585 cedr@u.washington.edu
www.cedr.us

Abstract:
An increasingly prevalent type of program designed to address college attainment gaps are state-based financial aid programs that offer low-income middle school students a promise of funding for college in exchange for making a pledge to do well in high school, be a good citizen and not be convicted of a felony, and apply for financial aid to college. Using a difference-in-differences specification, we estimate the effects of Washington State's College Bound Scholarship Program on high school grades, whether students graduate from high school, and incarceration in state prison during high school or during early adulthood. We find evidence that eligible students' high school grade point averages fell by 0.01 (from a pre-policy base of 2.38) and that the likelihood of being incarcerated fell by 0.1 percentage points (from a pre-policy base of 0.3 percentage points). These findings are robust to falsification exercises. Eligible students also experienced an increase in their rate of on-time high school graduation, but falsification tests show that this result is not due to the program, but rather due to broader secular improvement in graduation rates for disadvantaged youth.

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1. Early Commitment College Scholarship Programs

The past three decades have witnessed significant increases in the proportion of U.S. students enrolling and graduating from college, however, significant educational attainment gaps still exist between more advantaged, primarily white and high-income students, and disadvantaged students, who are often racial minorities and low-income students. For instance, between 1975 and 2010, bachelor's degree attainment increased 15 percentage points for Whites, but only 9 percentage points for Blacks, and 4 percentage points for Hispanics (Aud et al., 2011). In 2009, there was a 29-percentage point gap between students from low- and high-income families in the share attending either a two- or four-year college in the fall immediately after completing high school (Aud et al., 2011), and the gap in graduation from college is even higher, 45 percentage points (Kena, 2015). Consequently, it is vitally important that society focus on ways to increase college enrollment and success for disadvantaged students.

Empirical research has identified a variety of factors that contribute to the persistence of college enrollment gaps, but perhaps the most important is that disadvantaged students often lack the academic preparation necessary to succeed in college (Kirst, Venezia, and Antonio 2004; Jacob and Linkow, 2011; Rosenbaum 2001). Additionally, involvement in criminal activity also negatively affects educational attainment. Fumia (2013) finds that incarceration by age 18 reduces the probability of high school degree receipt by 22 percentage points and bachelor's degree receipt by 4 percentage points. Arrests have also been shown to reduce the likelihood of college enrollment (Apel and Sweeten, 2009; Kirk and Sampson, 2013) and college completion (Tanner, et al., 1999).

State legislators are trying to improve college readiness of low-income youth through a variety of policy changes. An increasingly prevalent type of program designed, in part, to address college attainment gaps are state-based financial aid programs that offer low-income students an early promise of funding for college in exchange for their making a pledge. Pledges, made during 7^{th}-9^{th} grades, have students promise to do well in high school, be a good citizen (e.g., by not committing a felony), and complete a FAFSA. These "early commitment programs" are hypothesized to directly help low-income students by making college more affordable and, importantly, the early promise of funding is thought to set them on the right path by creating a strong incentive for them to do well in high school, avoid criminal activity, and fulfill pledge requirements. Understanding whether these types of programs appear to work to increase student achievement and college readiness is immensely important, but much of the existing evidence of such programs is weak, primarily because prior studies have lacked data to identify whether students are program eligible.

Washington State's "*College Bound Scholarship Program*" (henceforth the "CBS") is an early commitment need-based scholarship program designed to encourage economically disadvantaged middle school students to "choose a path that will lead to educational success after high school." The goal of this paper is to evaluate whether this policy has met the legislative intent to improve the antecedent conditions required for low-income youth to successfully enter college. We estimate the effects of eligibility for CBS on high school grades; whether students graduate from high school; and incarceration during high school or during early adulthood.

Using a difference-in-differences identification strategy – the difference between eligible and non-eligible students before and after the availability of the CBS program – we find evidence that eligible students' high school grade point averages fell by 0.01 (from a pre-policy base of 2.38) and that the likelihood of being incarcerated fell by 0.1 percentage points (from a

pre-policy base of 0.3 percentage points). These findings are robust to falsification exercises. Eligible students also experienced an increase in their rate of on-time high school graduation, but falsification tests show that this result is not due to the program, but rather due to broader secular improvement in graduation rates for disadvantaged youth.

2. The CBS Program and Evidence on Other Early Commitment Programs

2.1 The Washington CBS Program

The College Bound Scholarship program was created by the Washington legislature in 2007 and was patterned on similar programs in Indiana (21st Century Scholars program initiated in 1990) and Oklahoma (Oklahoma's Promise initiated in 1996), but as we describe below, the Washington program has some features that differentiate it from similar early commitment programs. The program is designed to help low-income students get on a path toward college and provide them resources to make college attainable. The program works as follows. Students who are eligible to sign-up (primarily based on being from a low-income family) sign a pledge to: 1) do well in middle and high school; 2) be a good citizen and not be convicted of a felony; and 3) apply for financial aid to college. If they satisfy these requirements, and their family income remains below a threshold in their senior year, they are promised a scholarship that covers the tuition and some fees plus a small textbook allowance that are not covered by other state financial aid awards, to attend an eligible Washington State higher education institution.

A student is eligible to sign the CBS pledge if during 7th or 8th grade (or 9th grade for the first eligible cohort during 2008-09) any of the following applied: the student was eligible for free or reduced-price lunch (FRPL), the student's family received Temporary Assistance for Needy Families (TANF), the student was a foster youth, or the student's family income was below 185 percent of the poverty line (which would also qualify the student for FRPL).[1]

The text of the pledge read as follows[2]:

"Yes, I am college bound! I pledge that I will:

- Do well in middle school and high school, and graduate with a cumulative high school grade point average of 2.0 or higher on a 4.0 scale.[3]
- Be a good citizen in my school and my community and not commit a felony.
- Apply for financial aid by submitting the Free Application for Federal Student Aid (FAFSA) in a timely manner during my senior year of high school."

When the student enters her senior year, to be eligible for the financial aid the student's family income during that year must fall within 65 percent of the state's median family income.[4]

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[1] Note that for the first cohort, for a family of four, 185 percent of the poverty line equaled $39,220 in 2008.

[2] This was the pledge for the cohorts in our sample. The current pledge (as of 2017) reads "Graduate with a cumulative high school grade point average of 2.0 or higher. Be a good member of my community, and not be convicted of a felony. Apply for financial aid by completing the FAFSA or WASFA."

[3] These "early commitment" programs are similar to merit scholarship programs that are available in several states (Georgia's HOPE Scholarship Program is particularly well-known), in that they require students to earn a certain high school GPA to be eligible for receipt of the funds, but they differ from merit scholarship programs in that they are income-contingent (i.e., available only to low-income students) and require the signing of a pledge in the early high school grades.

The fact that the CBS is contingent on family income during a student's senior year somewhat weakens the clarity of what rewards will follow from signing and fulfilling the pledge, but the increase in the income threshold for qualifying (e.g., rising from $39,220 in 8th grade to $53,000 in 12th grade for a family of four in the first cohort) implies that a great many of those students who initially sign up for the program will be eligible when it comes time to make college-going decisions.[5]

Should the student remain income-eligible in their senior year, the guaranteed aid is both generous and transparent, completely covering tuition and service/activity fees.[6] Students attending private institutions of higher education in Washington receive an amount equal to what the average student receives attending a comparable public institution in the state (typically the average award given at the University of Washington and Washington State University). CBS covers 8 semesters (12 quarters) so long as the student maintains Satisfactory Academic Progress as determined by the college, must be used within five years of high school graduation, and cannot be used for graduate school.

2.2 Empirical Evidence on Early Commitment Programs

There is a large and growing literature on the effects of merit-based financial aid (e.g. Cornwell et al., 2006; Dynarski, 2008, Sjoquist and Winters, 2012), but early commitment college programs – distinct from more general forms of merit-based aid – have received very limited empirical scrutiny despite significant public attention. Early commitment programs offer promise as they are designed to address some of the hurdles confronting low-income students. Studies show that students and parents often misestimate college costs (Ikenberry & Hartle 1998; Usher 2005), and that low-income families are particularly inaccurate in their estimation (Avery & Kane 2004; Grodsky & Jones 2007; Horn et al., 2003; Jacob and Linkow, 2011). This fact likely contributes to low-income families being more likely to have inadequate funding (Long & Riley 2007). However, even when adequate funding is available (e.g. through scholarships, etc.), low-income students and families are less likely to know about funding options, or at least to apply for assistance (Bettinger et al., 2009; Hahn & Price 2008; Long & Riley 2007). Dynarski and Scott-Clayton (2006) argue that complexities in the financial aid system, particularly in completing the Free Application for Federal Student Aid (FAFSA) "may prevent the subsidies

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[4] Like all need-based government policies, this feature of the program gives an incentive for families to stay low-income. If parents respond to this adverse incentive, it could have longer-term negative effects on students.

[5] The language surrounding Washington's CBS implies a contractual bond between the student and the state. The "College Bound Scholarship Program… *promises* annual college tuition and a small book allowance" [http://www.wsac.wa.gov/sites/default/files/2011-12_Q&A.pdf, emphasis added]. Moreover, given that the student is required to do well in school, be a good citizen, and not commit a felony, it appears that it would be politically hard to break the promise if the student does these things. As State Representative Reuven Carlyle, noted that the state has "a moral responsibility to fund [the CBS]. There's no way we can break that social contract" (Long, 2012). Thus, these types of pledge programs may bind future legislatures to fund the programs given the promise of funding. These kinds of pledge policies may be appealing to legislatures given their transparency to students and the ability of current legislatures to bind the actions of future legislators.

[6] Specifically, the CBS documentation states: "The scholarship amount will be based on tuition rates at Washington public colleges and universities. It will cover the tuition and fees (plus a small book allowance) that are not covered by other state financial aid awards such as the State Need Grant. You will receive your scholarship through your college or university as part of your financial aid award" (WHECB, 2012c).

from having their intended effect of inducing students into college" (p. 319). Correspondingly, Bettinger et al. (2009) find that low- and moderate-income families who "received assistance with the FAFSA" during tax preparation at H&R Block "and information about aid were substantially more likely to submit the aid application, enroll in college the following fall, and receive more financial aid" (abstract).

It is not surprising that there are numerous programs designed to increase years of postsecondary schooling, particularly for disadvantaged students. In fact, there are over a thousand programs administered by federal and state government, universities, nonprofits, and community groups designed to address the attainment gap through a variety of approaches including mentoring, counseling, parental involvement, academic preparation, personal enrichment, and financial assistance (Gándara & Bial, 2001). Unfortunately, our knowledge about the efficacy of specific programs designed to increase the likelihood of high school graduation, college-going, and college completion is poor. While many studies have evaluated the effectiveness of "early intervention" or "pipeline" programs, few are designed in a way that reliably measure program effects; only a handful of studies employ experimental or quasi-experimental methods with comparison groups even though this approach is usually necessary to determine whether student gains are the result of the program or the result of how students were selected into the program or some other unobserved factor (Domina, 2009).[7]

Several states have early commitment programs that are similar to the CBS in that they meet Blanco's (2005) "three core criteria for early commitment programs: that they make a guarantee of aid; that aid is designated only for economically disadvantaged students; and that students are identified in elementary, middle school, or early high school" (p. 9).[8] But unlike the literature on state merit aid scholarship programs, there is no experimental or quasi-experimental research on state-administered "early commitment" scholarship programs like the CBS. The lack of empirical evidence on early commitment pledge programs is likely due to the lack of data needed to form appropriate comparison groups for those students who are eligible to participate in these programs. For instance, St. John et al. (2001, 2003, 2004, 2005, 2008) investigate the possible impact of Indiana's 21st Century Scholars Program—which enrolls 6th, 7th, and 8th graders in a program similar to the CBS—on student-level outcomes. The studies find significant positive associations between completion of the pledge in Indiana, the likelihood that students completed an advanced high school curricula, and enrollment in both two- and four-year colleges.

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[7] For experimental or quasi-experimental studies of such programs (e.g., Quantum Opportunity Program, Upward Bound, and Talent Search) see Schirm et al. (2006), Myers et al. (2004), and Constantine et al. (2006).

[8] But there are two important programmatic differences between Washington's program and those in other early commitment states, such as Indiana and Oklahoma. First, until recently, the programs in Indiana and Oklahoma had no income requirement at the time that the student attended college. Heller (2006) noted, "(t)he distinguishing characteristic of these two programs from that of other publicly funded aid programs is that once students are accepted into the program while in middle school, they will not be removed even if their family's economic circumstances change" (p. 1276). Washington, in contrast, was designed at its inception with a restriction that the students' family income could not rise above 65% of the state's median family income ($53,000 for a family of four in 2012-13). This senior year income cap makes the CBS "promise" much more uncertain. Second, the programs in Indiana and Oklahoma require students to take certain college-appropriate coursework while in high school to be eligible. CBS, in contrast, places no coursework restrictions and merely has a relatively weak 2.0 grade point average as its only performance requirement.

The St John et al. studies, however, do not rely on data on cohorts of students *before the introduction of the pledge program*, and lack information needed to identify if a student was *eligible for the program.* Thus, they were forced to compare students who *signed the pledge*, to a comparison group of students who may or may not have been eligible. St. John et al. (2004), for example, use students who attended high-poverty schools, but who did not sign the pledge, as the control group. By using students who did not sign the pledge as the comparison group any estimated program effects are likely confounded by unobserved variables that are correlated both with the likelihood of a student signing the pledge and with the likelihood of a student attending college as students who enroll in the program are probably more likely to attend college (holding observable student characteristics constant) given their unobserved motivation.[9]

3. Data and Analytic Approach

3.1 Data

The primary data we utilize for this research are collected and aggregated by Washington State's Education Research & Data Center (ERDC). ERDC maintains individual student level K-12 records for all public-school students in the state that are linked to information about CBS sign up and eligibility maintained by the Washington State Achievement Council (WSAC).[10] In particular, the ERDC information includes student academic performance (GPA, performance on state assessments, etc.) while in high school, and whether students graduate from high school.

The ERDC data includes K-12 student information dating back to the 2005-06 school year. This means that, unlike prior studies of early commitment financial aid programs, we have data on two cohorts of students who did not have the opportunity to receive a CBS (those who

[9] There are also two on-going experimental studies of locally-administered early commitment scholarship programs. (Additionally, the Wisconsin Scholars Longitudinal Study is large-scale financial aid experiment, but does not include an early commitment component.) These programs differ in important ways from current state-administered programs. In 2011, eighteen Milwaukee schools were randomly selected as beneficiaries of the "The Degree Project®". The program was "announced at surprise assemblies to the students that if they graduate from high school and meet certain eligibility requirements, they'll earn scholarships worth up to $12,000" (Finkelmeyer, 2011). The study includes roughly 2,600 treatments students and roughly 4,700 controls. The requirements include graduating from a Milwaukee Public School with a regular diploma, a 2.5 GPA, and a 90% attendance rate. Notably, students did not need to sign-up in 9th grade, there are no income requirements for students either at entry nor during the student's senior year, students are making no "pledge" at entry, and there is no requirement that the student to be a "good citizen" nor not commit a felony (Degree Project, undated). The announcement of this program in school assemblies likely increases the impact of the promise as it makes it salient and exciting. In 2007, the "Future to Discover" study was launched in 30 New Brunswick and 21 Manitoba high schools. This study contained two interventions, "Explore Your Horizons" (a career exploration program) and "Learning Accounts" (an early commitment scholarship program, which was only available in New Brunswick schools). Grade 9 students were automatically enrolled (less than 0.5 percent opted out) and "A total of 1,097 participants from lower-income families were randomly assigned to receive Learning Accounts either by itself or in combination with Explore Your Horizons" (Social Research and Demonstration Corporation, 2007). To be eligible, students must have household income during 9th grade below the provincial median (a much less restrictive income-cap than CBS). There is no senior year income cap. Participants receive up to $8,000 (Canadian) to be used at a recognized post-secondary education program. Thus, unlike CBS, this program does not guarantee a scholarship that will pay the full tuition at a public university. Neither study has produced their final report on outcomes, though analysis to date suggests positive effects on postsecondary enrollment (e.g. Ford and Kwakye, 2016).

[10] WSAC administers the CBS program. WSAC is a cabinet-level agency in Washington; for more information on the responsibilities of WSAC, see http://www.wsac.wa.gov/what-we-do.

were in 8th grade in 2005-06 and 2006-07). We define cohort one as students who were enrolled in 8th grade in 2005-06, cohort two as students who were enrolled in 8th grade in 2006-07, cohort three as students who were enrolled in 8th grade in 2007-08, and so forth for all five cohorts. Hence, cohorts one and two are the cohorts that were not eligible to receive the CBS.

Additionally, through an agreement with the Department of Corrections, we have access to the census of all individuals who are incarcerated in Washington State prisons at any point between January 1, 2009 and November 2014. This information was linked to the ERDC data (through social security numbers), de-identified and returned to us to be used to assess the likelihood of incarceration (while in high school or as a young adult). While some individuals are incarcerated in State prisons for misdemeanors, the overwhelming number of individuals incarcerated in State prisons are for more serious crimes, such as felonies. Indeed, 98.9% of students in our DOC data were convicted of at least one felony. Our outcome measure is whether the student was incarcerated in a Washington State prison before two years after on-time high school graduation[11]. Due to the limited span of time included in our DOC data, we can only compute this outcome for our second and third cohorts (i.e., students in the cohorts immediately before and after the introduction of the CBS program). As a consequence, we will only be able to use the second of our two falsification checks described below.

We do not have access to data on county jails or juvenile detentions. Many misdemeanors and minor crimes are handled by county jails, rather than state prisons. Furthermore, in Washington State the Department of Social and Health Services (DSHS) oversees juvenile detention rather than the Department of Corrections. For these two reasons, our outcome measure of incarceration mainly reflects serious crimes of adults (18 years of age or higher)[12]. In our sample, only 0.154% of individuals between 18 and 20 are incarcerated at least once. For comparison, the national incarceration rate in state prisons for individuals between the ages of 18 and 19 in 2014 was 0.146% (Carson, 2015).

Our data include 443,315 individual student records for the five cohorts, but we drop from these data foreign exchange students, observations with missing ID codes, observations with multiple IDs and irreconcilable birthdates, students enrolled part time in public high school, and students who were not identified in a school in 8th grade. These restrictions reduce the number of unique student observations to 415,384 unique students, including 169,887 in the pre-policy cohorts 1 and 2, and 245,497 in the post-policy cohorts 3, 4, and 5. Nearly half of the students in the post-policy cohorts, 114,612, were clearly eligible for the CBS program (as a result of being enrolled in foster care or FRPL eligible in 8th or 9th grade (cohort 3) or 7th or 8th grade (cohorts 4 and 5). Similarly, nearly half of the students in the pre-policy cohorts, 76,496, were enrolled in foster care or were FRPL eligible in 7th or 8th grade – yet, these disadvantaged youth were ineligible for the CBS scholarship. Since these students would have been eligible to apply for the CBS scholarship had the CBS been implemented one or two years earlier, we refer to them as "placebo-eligible".

Note that we define a student as "eligible" for the CBS program if the student is enrolled in foster care or is known to be eligible for FRPL.[13] Unfortunately, this is an imperfect definition

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[12] www.dshs.wa.gove/ra/office-juvenile-justice/juveniles-transferrred-or-waived-adult-criminal-court-system

[13] By 2008-09, all school districts in the U.S. were required by the 2004 Child Nutrition and WIC Reauthorization Act to "directly certify" recipients of SNAP and FDPIR as eligible for free meals under the National School Lunch Program. Thus, all SNAP and FDPIR recipients should be coded as a FRPL-eligible in our administrative data. In

and it is not possible with the administrative data made available to us to construct a perfect measure of whether the student is eligible to sign up for the CBS in middle school as we do not have information on students who may be income eligible despite not receiving FRPL, the Supplemental Nutrition Assistance Program (SNAP), the Food Distribution Program on Indian Reservations (FDPIR), or TANF. Who we will fail to identify as eligible are those who are income-eligible for CBS, but not a recipient of FRPL, SNAP, FDPIR, or TANF; we estimate that our definition of "eligible" will miss only 13.4 percent of students who are actually eligible.[14, 15]

Because of the misidentification of students as ineligible who come from income-eligible-only families, our estimated effects may be biased, yet we can bound the true effect as lying in the interval between $0.866 \times \hat{\beta}_1$ and $1.10 \times \hat{\beta}_1$ (with a corresponding confidence interval around these extremes), where $\hat{\beta}_1$ is the estimated treatment effect given by Equation 1, described below. If we assume that income-eligible-only students respond to the CBS exactly as the students we correctly identify as eligible, then our estimate of the effect would be biased downwards (akin to random measurement error) and we can easily recover an unbiased estimate of the effect by inflating the downwardly biased estimate.[16] If, on the other hand, we assume that income-eligible-only students do not respond to the CBS, then our estimates will biased upward (as we would miss the zero effect on these students), and we can again easily recover an unbiased estimate by deflating the upwardly biased estimate.[17, 18]

Washington in 2007-08, 76 percent of children in SNAP households were directly certified for free school meals (USDA, 2008). Washington began direct certification of children in TANF households in 2003-04 (Neuberger, 2006).

[14] This figure is based on 3,245 SIPP youth aged 12-14 who were CBS eligible. If we restrict the analysis to Washington youth (only 93 observations), we find a comparable rate of youth eligible for CBS based solely on family income (17.7 percent), which is not significantly different than the full sample given the small sample size.

[15] Recipients of the Food Distribution Program on Indian Reservations (FDPIR) are directly certified as eligible for free lunches, but SIPP does not collect data on FDPIR participation. Since we capture these youth as FRPL-eligible from school administrative data, our estimate of the fraction that we miss, 13.4 percent, is an upper-bound estimate. Nationally (in Washington) we estimate (based on data in Usher, Shanklin, and Wildfire (1990), Snyder and Dillow (2011), and USDA (2012)) that 0.05 percent (0.10 percent) of 8th grade students participate in FDPIR

[16] A simple algebraic manipulation can show that an unbiased estimate of β_1 (from Equation 1, below) is equal to $\frac{A+B}{A}\hat{\beta}_1$, where *A* and *B* respectively represent the numbers of truly ineligible and income-only-eligible students in the post-policy period. Based on the figures we report earlier, we predict the multiplier on $\hat{\beta}_3$ to equal 1.10.

[17] There are two reasons to believe that income-only-eligible students have a lower responsiveness to the CBS than the students we correctly identify as eligible. First, such students may come from families who do not feel comfortable relying on government aid or are from families who are generally unaware of available need-based aid programs. If so, and if these preferences and/or lack of knowledge applied to college financial aid, then this group might be less responsive. Second, based on our analysis of SIPP youth, income-only-eligible students appear to come from families with lower income and higher poverty than students that we correctly identify as eligible. (Those who we identify as eligible have higher median family incomes ($30,280 versus $25,711), larger mean family sizes (4.7 versus 4.1), and higher income-to-poverty threshold ratios (1.31 to 1.24) than those who are foster / income-eligible-only, based on these SIPP youth.) Such lower income families are likely to have greater amounts of support from Pell Grants and State Need Grants, and thus receive smaller amounts of net financial aid support from the CBS. If they receive less marginal funding from CBS, they may be less responsive to the program. If we assume foster/income-eligible students have no responsiveness to the CBS, then the true value of β_3 would be a weighted average of the effect observed for those who we identify as eligible ($\hat{\beta}_3$) and the zero effect for the income-only-eligible students, with the weights equal to their relative shares of eligible students (0.866 and 0.134 based on our SIPP analysis), yielding $0.866 \times \hat{\beta}_3$ as the lower bound estimate.

[18] When students (and their parents) sign the pledge in middle school, they are asked to check all of the following that apply: "Student is eligible for the federal free- or reduced-price lunch program; Student's family receives basic

The primary GPA measure we use is the student's cumulative high school GPA through 12th grade. Approximately, 22.9% of students are missing cumulative GPAs in 12th grade because they either drop out prior to the end of high school, transfer out of state, or do not have a record of their cumulative GPA in 12th grade. Thus, we also report effects on student's cumulative high school GPA through 10th grade.[19] High school graduation is determined by using school withdrawal reason codes[20] and the expected year of graduation. If a student has a withdrawal code indicating that they received a high school diploma by September 1st of a student's expected graduation year, we consider this student as a high school graduate. If a student transfers out of Washington State prior to the end of their expected graduation year, they are removed from the on-time graduation models described below.

Table 1 provides sample statistics for selected student variables before and after the implementation of the CBS program and broken out by program eligibility status. As is readily apparent from looking at the mean differences between the treatment eligible (or "placebo eligible" in the pre-CBS program years) and not eligible students, treatment eligible students are far more likely to be underrepresented minority students, disabled, from a household where English is not the primary language, or homeless. Furthermore, the eligible students have significantly poorer high school outcomes than non-eligible students. It is not surprising that there is a large gap in 7th grade math (baseline) test scores between eligible and not eligible students, but somewhat reassuring is the fact that the magnitude of the gap changes little between students in the pre-policy and post- policy years[21]. Interestingly, however, the on-time high school graduation gap between eligible and not eligible students shrinks considerably (by just over 3 percentage points from -.26 to -.23), suggestive of a policy effect (although we reject that interpretation using the falsification tests described below).

[Insert Table 1]

3.2 Analytic Approach

The data we utilize allows us identify cohorts of students who met the CBS eligibility requirements before and after the introduction of the program. Hence, our primary analytic strategy is to utilize a differences-in-differences analysis using student-level data to compare

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food/TANF benefits; Our 2010 family income from all sources (taxable and nontaxable) was less than or equal to the amounts in the chart; Student is currently in foster care and is automatically eligible to apply" (based on the 2011-12 form). We evaluate the differences in outcomes of students who only checked the foster or income-eligible-only boxes to other eligible students. If the outcomes prove to be similar across these groups, it would provide limited support for the assumption that the two groups respond similarly to the CBS and thus would support the assumption that the value of β_3 is closer to $1.10 \times \hat{\beta}_3$.

[19] We use multiple imputation with 10 iterations to impute the following variables when missing: 7th grade test scores, 12th and 10th grade GPAs, and high school unemployment rates. After creating the 10 imputed data sets, we follow the recommendation of Von Hippel (2007) to drop observations with missing dependent variables (e.g., missing high school GPA) from the difference-in-differences regressions in order to improve efficiency. We combine estimates from the 10 imputed datasets using Rubin's (1987) rules.

[20] Withdrawal codes allow us to determine whether a student left school for a number of reasons, including but not limited to, transferring out of state or dropping out.

[21] We use "reassuring" here in the sense that proper identification of a treatment effect relies on similar comparison groups. Had these baseline measures changed substantially it would indicate our comparison groups were changing across time.

differences in outcomes (e.g. the likelihood of high school graduation) of those who meet the CBS eligibility requirements in cohorts before (cohorts one and two) and after (cohorts three through five) the introduction of the implementation of the CBS program (the first difference), and compare this to cross-cohort differences in outcomes for students who do not meet the eligibility requirements (the second difference). This is expressed in equation 1:

$$(1) \qquad Y_{imt} = \beta_m + \beta_1 Post_t \times Eligible_i + \beta_2 Post_t + \beta_3 Eligible_i + \beta_4 X_i + \varepsilon_{imt}$$

Y_{imt} is the outcome for student i attending middle school m in cohort t, β_m are middle school fixed effects based on the student's enrollment during the fall of 8th grade, $Post_t$ is an indicator that equals one if the student is in a cohort of students who graduate from high school in 2012, 2013, or 2014 (i.e. cohorts 3,4, and 5), $Eligible_i$ is an indicator for being eligible (or pseudo-eligible) for the CBS program as described above, and X_i is a vector of individual student characteristics (e.g. 7th grade math achievement[22], gender, homeless status), and ε_{imt} is the error term.[23] We also estimate alternative specifications that (a) exclude middle school fixed effects, (b) add the time-varying share of the middle school population that is CBS-eligible (so as to capture peer effects), and (c) include both share eligible and fixed effects. These models are estimated by ordinary least squares for all outcomes.

We also conduct subgroup analysis by race and gender. The subgroup analysis is conducted by doing the differences-in-differences analysis for each subgroup separately. The race/ethnicity analysis is motivated by the fact that while African Americans and Hispanics have lower raw rates of college enrollment than non-Hispanic whites and Asian Americans, there are a number of authors who have found higher rates of attendance by African Americans and Hispanics relative to non-Hispanic whites conditional on measures of socioeconomic status and school performance (e.g., Perna, 2000; Cameron and Heckman, 2001; Black and Sufi, 2002; Bennett & Xie, 2003; and Jez, 2008). Our analysis of gender differences is motivated by the raw gap in college attendance: boys' share of first-time freshmen fall enrollment was only 46.3 percent in 2010 and their share of bachelor's degrees awarded in 2009-10 was 42.8 percent (Snyder and Dillow, 2011). Our interest is in whether low-income boys have correspondingly smaller responses to the CBS than low-income girls do.

The key policy variable upon which we focus is "$Post \times Eligible$."[24] If the second term of Equation 1 ($\boldsymbol{\beta_2} Post_i$) captures the true counterfactual time trend in outcomes for eligible students (i.e., the change in outcomes that would have occurred for eligible students if the CBS program had not been enacted) then $\boldsymbol{\beta_1}$reveals the effect of the policy. As with all difference-in-

[22] 7th grade math and reading scores have been standardized within grade and year.

[23] We use a linear probability model (with robust standard errors clustered at the middle school level) when the outcome is dichotomous. Using a linear probability model is preferred in this context (over a logit or probit specification) given the fact that the central part of Equation 3, reflected in the first four terms, is essentially a comparison of conditional means. Further, given the complexities of interpreting interaction terms in non-linear models (Ai and Norton, 2003), we prefer a linear probability model for its ease of interpretation.

[24] We use robust standard errors here and for all specifications. However, note that by collapsing the data into pre- and post-policy periods, we avoid the problems caused by serial correlation which, as shown by Bertrand et al. (2004), to lead to downwardly biased estimates of standard errors. Using a simulation that included serial correlation, Bertrand et al. show that data collapsed into pre- and post-policy periods yield the expected false positive rate of roughly 5% (using a 95% confidence interval).

differences analyses, the internal validity of the estimate as revealing the true causal effect of the policy relies on this parallel trends assumption.

The identifying assumption for our differences-in-differences design is that changes in outcomes across cohorts for those who were ineligible for the CBS are a reasonable proxy for changes in outcomes that would have been observed for the CBS-eligible population in the absence of the program. For this assumption to be valid there must be no factors that influence the student outcomes that shift concurrently with the implementation of the CBS and that differentially affect students who do or do not meet the eligibility requirements.

One concern with this identification strategy is that the unemployment rate in Washington had been falling during the period when these students would be making college enrollment decisions (from 9.8 percent (9/09) to 9.8 percent (9/10) to 9.0 percent (9/11) to 7.4 percent (9/12) to 6.8 percent (7/13)) and it is reasonable to believe that this improving labor market might differentially affect the college enrollment prospects of low-income youth relative to middle and high-income youth, which would weaken the validity of our approach.[25] Potentially offsetting any positive effect of the improving economy, state funding for higher education fell dramatically during this same period, falling 25.5 percent between the state's 2007-09 and 2011-13 biennium budgets, and these changes are likely to have disproportionate negative impacts on the enrollment decision of low-income students (WHECB, 2012b). To attenuate some of these concerns, we include the county unemployment rate by cohort and grade for each high school in all of models.

Furthermore, we can do falsification checks to see whether economic or funding changes appear to be the driver of our findings from Equation 1. First, we attempt to assess whether there are any pre-existing time-trends that might affect the relative achievement of different cohorts of FRPL students. To do this, we focus on the relative performance of cohorts 1 (8th graders in 2005-06) and 2 (8th graders in 2006-07), neither of which were eligible for the CBS program. We compare the relative (to non-FRPL/Foster students) outcomes of FRPL/Foster students in cohort 1 to the relative outcomes in cohort 2. Since students in these cohorts are not enrolling in the CBS program, any improvement in the relative outcomes of students in cohort 2 compared to cohort 1 would be indicative of a time trend that could also bias our estimates (from Section 4) of the CBS program effect. Specifically, we estimate a regression that adds to Equation (1) indicators for cohort 2 and an interaction between cohort 2 and FRPL/Foster care eligibility in 7th or 8th grade as given by Equation 2:

$$(2) \qquad Y_{imt} = \beta_m + \beta_1 Post_t \times Eligible_i + \gamma Cohort2_t \times Eligible_i + \beta_2 Post_t + \beta_3 Eligible_i + \beta_4 X_i + \varepsilon_{imt}$$

The coefficient on this interaction term, γ, shows whether the performance of disadvantaged students in cohort 2 improves relative to similarly disadvantaged students in cohort 1. The advantage of this first falsification test is that we are keeping the definition of eligibility consistent with the CBS program in terms of the grades where eligibility is defined.

[25] It is easy to imagine that improving economic conditions might differentially affect the schooling outcomes of low-income youth relative to middle and high-income youth (Kalil, 2013). The improving labor market could disproportionately improve the parental resources of low-income youth, thereby facilitating their college enrollment. Or, the improving labor market could disproportionately pull low-income youth into the labor market and away from college.

There are three main limitations of this test. First, we only have data on outcomes for two pre-treatment cohorts, which provides limited data with which to identify existing trends. Second, this test is not sensitive to any economic or policy changes that are coincident with the introduction of the CBS. For instance, an acceleration of the economic recovery following the introduction of the policy may not manifest itself during the pre-treatment period. Third, this falsification data cannot be implemented for our incarceration outcome, as that variable is only available for cohorts 2 and 3.

The second way we attempt to falsify the findings is to test whether students that are plausibly as disadvantaged as CBS program-eligible students, but who are identified as being disadvantaged (i.e. through the FRPL program) in grades that don't make them eligible, appear to have similar gains to those students who are actually CBS program-eligible. To do this we must identify these "plausibly as disadvantaged" students. Here we draw on the fact that recent evidence (Michelmore and Dynarski, 2016) from Michigan shows that there is considerable intertemporal volatility in students FRPL status, i.e. there are some years that students are enrolled in FRPL programs and some years they are not. We find this is also true in Washington State; for instance, 41% of students FRPL eligible at least once between grades 6 and 9 were also ineligible in at least one of these grades.

As we show in Figure 1, certain cohorts of students are eligible for the CBS program in certain years (yellow shading), but there are students who are, for instance, FRPL recipients in a border grade (blue shading) but not FRPL recipients in any CBS eligibility grade. For example, a student in cohort 5 who was FRPL eligible in 6th grade only could not receive the CBS. They are as relatively disadvantaged as a student in cohort 5 who was FRPL eligible in 7th grade only, but who is eligible to receive the CBS. Thus, students who are FRPL eligible only in these border grades form a plausibly as disadvantaged group as the CBS program-eligible group, but they are not actually treated.

[Insert Figure 1]

In this second falsification test, we assess whether these border FRPL eligible only students appear to have better relative outcomes after the implementation of the CBS program. Specifically, we estimate a model that includes placebo "border-eligible" students (i.e., those who are FRPL/foster eligible in the wrong grades to participate in the CBS program), and interact this placebo eligibility with the timing of the CBS implementation. The model also includes a vector of controls for students' overall patterns of FRPL/foster eligibility in each grade (P_i)[26], and is given by Equation 3:

$$(3) \qquad Y_{imt} = \beta_m + \beta_1 Post_t \times Eligible_i + \delta Post_t \times Border\text{-}Eligible_i + \beta_2 Post_t + \beta_3 X_i + \beta_4 P_i + \varepsilon_{imt}$$

The identification therefore comes from changes in the outcomes of border-eligible students between the pre-CBS and CBS cohorts relative to changes in the outcomes of students who are non-FRPL recipients in between the pre-CBS and CBS cohorts. Since the placebo students cannot participate in the CBS program, any positive findings associated with the implementation of the program would undermine the argument that it is the program itself that is driving the

[26] P_i includes the full set of possible patterns of FRPL eligibility during grades 6, 7, 8, 9, and 10 (Just 6, Just 7, Just 8, Just 9, Just 10, 6 & 7, 6 & 8, etc. etc., and eligibility in all five grades).

positive outcomes found using Equation 1. That is, if the estimated values of β_1 and δ from Equation 3 are similar, it would indicate a secular time trend affecting disadvantaged youth rather than an effect of the CBS program *per se*.

The main threat to validity of this falsification test is the possibility that border-eligible students respond differently to time trends in unobservable ways from CBS-eligible students. Note that by definition border FRPL eligible only students are not chronically FRPL eligible (because we know they are not eligible in the CBS program-qualifying grades). On the other hand, the group of students that are FRPL eligible in the right grades (making them CBS eligible) include both chronically FRPL eligible and transitory FRPL eligible students. To the extent that the number of years spent being FRPL eligible is a good proxy for lower socioeconomic status (see Michelmore and Dynarski (2016)), this likely makes the border eligible students slightly less poor than the CBS eligible students. In other words, the threat to validity in this falsification test is that poorer students (again, likely CBS-qualifying) may respond differently to time trends unassociated with the CBS than slightly less poor students.

4. Results

The estimated coefficients for Equation 1 are reported in Table 2. Columns 1-4 are the findings for student's high school GPA as an outcome,[27] Columns 5-8 are the findings for having an on-time high school diploma, and Columns 9-12 are the findings for the probability of incarceration.

[Insert Table 2]

Prior to focusing on the variables of interest, it is worth noting that the findings on the individual student control variables are quite consistent across specifications with our expectations based on prior research. For instance, we find that students who perform better on math and reading tests are predicted to have significantly higher high school GPAs (Kobrin, Camara, and Milewski, 2002) and are much more likely to graduate on time (Neild, Stoner-Eby, and Furstenberg, 2008). Asian students have higher GPAs than White students (the excluded category) , whereas Hispanic students have lower GPAs (Nord et al., 2011); similar patterns exist for female relative to male students (the excluded group) (Fortin, Oreopoulos, and Phipps, 2015). African American students have higher GPAs relative to White students after controlling for CBS eligibility, the parameterization of which relies heavily on FRPL eligibility. However, if CBS eligibility is removed from the 12th grade GPA models, African Americans have lower GPAs relative to White students. Similarly, disabled students have higher GPAs than non-disabled students after controlling for 7th grade test scores. However, if 7th grade test scores are removed from the models they are more likely to have lower GPAs than non-disabled students. In terms of incarceration, we observe that Hispanic and African American students are more likely to be incarcerated, female students are significantly less likely to be incarcerated (Chesney-Lind and Shelden, 2013), and students with higher baseline test scores are marginally less likely to be incarcerated (Zahn et al., 2010).

[27] While not reported, our findings are broadly similar if instead we use cumulative GPA through the 10th grade. We also experiment with linear probability models of students achieving a 2.0 or higher and find that the CBS has no effect on increasing or decreasing the likelihood of achieving a 2.0 or higher. Results are available upon request.

Students in pre-policy cohorts who are placebo-eligible for the CBS program have substantially lower high school GPAs (as shown by the "FRPL/Foster Eligible/Pseudo-Eligible" coefficients). The standard deviation of high school GPA is 0.85 (as shown in Table 1), so the magnitude of the coefficient on eligibility, -0.25 (shown in Column 1 of Table 2), suggests that students eligible for the program have a cumulative GPA that is more than one-quarter of a standard deviation lower than non-eligible students (controlling for other factors). This result is not terribly surprising since eligible students are economically or otherwise (e.g. in foster care) disadvantaged. Indeed, the magnitude of the GPA differential is roughly what prior research finds (Reardon, 2013; Duncan and Murnane, 2011). Likewise, there is a strong negative relationship between eligibility for the CBS and the likelihood of on-time graduation: pre-policy pseudo-eligible students are estimated to have a 17-percentage point lower probability to graduate on time than non-eligible students (Column 5). Given a mean graduation rate of 74% (across all cohorts), this represents about a 20% lower probability of on-time graduation. Finally, pre-policy pseudo-eligible students are far more likely than non-eligible students to be incarcerated within two years after on-time high school graduation. The overall likelihood of incarceration is small; only about 0.15 percent of the students in our sample are incarcerated in this time. Yet, pre-policy pseudo-eligible students are estimated to be about 0.17 percentage points more likely to be incarcerated (i.e., more than double the rate for non-eligible students).

4.1 Effects on High School GPA

The primary variable of interest, shown in the bolded first row of Table 2, is the interaction between FRPL/foster eligible and post. The coefficient on this interaction term provides the difference-in-differences estimates of the effect of the CBS program on high school GPA. This term is consistently negative and statistically significant whether we estimate it: using within and between school variation (Column 1); including an 8th grade school fixed effect so estimate it based on within 8th grade school variation (Column 2); including peer effects via the proportion of students in a school that are eligible or placebo eligible to sign up for the CBS program (Column 3); or including both school fixed effects and peer effects (Column 4). The magnitude of the interaction term is quite consistent across these specifications and is relatively small (ranging from -0.012 to -0.020).[28]

The insignificant coefficient on "post" suggests there is little evidence of a time-related change in GPA (e.g. due to changes in the achievement of cohorts of students or GPA standards) across cohorts of students before and after CBS program implementation.

It is important to note that while the findings suggest a negative effect of the CBS program on student performance, as measured by GPA, this should not necessarily be seen as an indictment of the program. One possibility, for instance, is that students who sign up for the CBS program take a more rigorous set of courses than they otherwise would resulting in poorer grades, but better preparation for college.[29]

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[28] We also estimated a variant of this model where the dependent variable is whether students have a cumulative GPA of 2.0 or higher (corresponding with the CBS program requirement). The interaction terms in these models are never statistically significant.

[29] In principal, we could test this hypothesis given that the CEDARS data system, which started in the 2009-10 school year, is designed to collect high school transcript data, including all previous course work. Unfortunately, however, the compliance with the CEDARS data system was quite poor in the early years. For instance, when we check course data against enrollment records we find that approximately 22% of high school enrollment spells lack

4.2 Effects on On-Time Graduation

Columns 5-8 of Table 2 are the findings for on-time high school graduation, and as was the case for GPA, the results are remarkably consistent across specifications. The "FRPL/Foster Eligible × Post" coefficient is consistently significant and positive across the various model specifications. These results suggest that the CBS program has increased the likelihood of on-time graduation for the disadvantaged students eligible to participate in the program by 3.2 to 3.5 percentage points. This is a large effect and reduces the baseline disparity between eligible and ineligible students (i.e., 14-17 percentage points, as discussed above) by roughly one-quarter. However, as we show below using the falsification test, this apparent effect appears to be driven by broader secular change and not a result of the CBS program.

The findings on "post" are sensitive to model specification; the estimates, which are significant and negative in the absence of 8th grade school fixed effects (Columns 5 and 7) are significant and positive when fixed effects are included in the model (Columns 6 and 8). One explanation for this result is the importance of school culture in influencing student outcomes. A number of studies, for instance, finds that the high schools play and important role in influencing graduation (Dobbie and Fryer, 2009), and in explaining both the quality of the college in which postsecondary students enroll (Darolia and Koedel, 2017 and performance in college (Black et al., 2015; Fletcher and Tienda, 2010; Long et al., 2009). We include 8th grade effects to account for unobserved middle school factors that might influence both the identification of student eligibility for the CBS program and a student's academic trajectory.[30] However, one might reasonably argue that high school factors are more important. Thus, we also estimate variants of the model where we either substitute 9th grade school indicators for 8th grade school indicators or include both simultaneously.[31] The findings on the coefficient on "post" are scarcely affected by the choice of 8th or 9th grade fixed effects.[32]

4.3 Effects on Incarceration

The key incarceration results (Columns 9-12 of Table 2) are not sensitive to model specification. As was the case with on-time graduation, the difference-in-differences estimates ("FRPL/Foster Eligible × Post") suggest benefits of the CBS program: a 0.11 percentage point lower probability of incarceration as a result of the program. Again, while this effect is a small reduction in absolute terms, it represents a substantial reduction in the likelihood of state prison

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course data for early cohorts. Moreover, the missing data does not appear to be random. When we estimate the probability that student transcript data is missing against student characteristics and school by year fixed effects, a number of the student characteristics are highly predictive of missingness. In particular, we find that being FRPL eligible, homeless, having a disability, and transferring schools within the year strongly predict whether or not a student will be missing course data. Thus, we conclude that the non-random patterns of missing data suggest we cannot accurately model course-taking in high school.

[30] See Goldhaber, Long, Person, and Rooklyn (2016) for more on the factors that might influence whether students' sign-up for the CBS program.

[31] We do not include 12th grade indicators because some students have dropped out of high school by that time.

[32] While 83% of students change schools between 8th and 9th grade, these results are not too surprising given that most middle school students in the same school are funneled to the same high school. These results are available from the authors upon request.

incarceration in the years following 10th grade – nearly a one-third reduction in eligible students' pre-policy incarceration rate and more than a two-fifths reduction in the disparity in incarceration rates between eligible and ineligible students. Put differently, we estimate that roughly 42 fewer students per cohort became incarcerated in a state prison because of the CBS program.[33,34]

As we discuss below (in Section 5), the incarceration findings do hold up to falsification tests, but there is some ambiguity about how to interpret this finding. On the one hand this finding is perfectly consistent with the portion of the CBS pledge that requires pledge-takers to "Be a good citizen in my school and my community and not commit a felony" implying that students' behavior might be changed as a consequence of the early commitment program. However, an alternative possibility is that the eligibility for the scholarship (and the contingency associated with committing a felony) affects the likelihood of prosecution or sentencing for (alleged) crimes. Prosecutors, for instance, might opt to go easier on students knowing that conviction for a felony would result in the loss of scholarship eligibility. (See Jain, 2016 for a discussion of prosecutors using discretion to avoid, or increase, the likelihood of civil penalties such as deportation and licensing). Unfortunately, the data we have from DOC on incarcerations does not allow us to assess whether CBS eligibility affects the likelihood of being charged with a particular type of crime. Thus, we cannot distinguish whether the incarceration findings are driven by students' behavioral changes or prosecutorial discretion.

4.4 Effects of CBS by Race and Gender

Table 3 re-estimates Equation 1 for racial and gender subgroups. We find that the negative effect of CBS on student's high school GPA was experienced by Hispanic youth (a reduction in GPA of 0.06), but not significantly by other groups.

We find positive significant apparent effects on on-time high school graduation for every subgroup. With regard to incarceration, we do not find significant effects of CBS when restricted to racial subgroups. But, we do see large differences in the effect of CBS on incarceration between females and males, where the estimated effects are -0.0005 and -.0018 respectively (i.e. the effect of CBS on incarceration is more than three times greater for males).

[Insert Table 3]

5. Falsification Tests

Table 4 presents the results of our first falsification test, with the "FRPL/Foster Eligible/Pseudo-Eligible × Cohort 2" rows bolded. If the results in this row are statistically significant, it would indicate important pre-policy trends, that if continued could explain our main findings in Table 2.

[Insert Table 4]

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[33] As the "Post" coefficients are insignificant, there is no evidence of significant changes in incarceration probability across cohorts.

[34] For comparison, Lochner and Moretti (2004) find that each additional year of schooling lowers the likelihood of incarceration for males aged 20 to 60 in 1960, 1970, and 1980 by 0.10 percentage points for whites and 0.37 percentage points for blacks.

In Columns 1-4 of Table 4, we see insignificant coefficients on "FRPL/Foster Eligible/Pseudo-Eligible × Cohort 2", which suggests that there was no pre-policy differential trend in outcomes between disadvantaged and advantaged kids with respect to GPA. Moreover, if anything, the point estimates suggest an upward trend that was interrupted by the CBS program.

The more critical findings of Table 4 are in Columns 5-8. Here we see that on-time high school graduation increased by 2.1 to 2.6 percentage points between Cohort 1 to Cohort 2. If this trend persisted into cohorts 3, 4, and 5, it could explain the apparent effect of CBS on graduation that was shown in Table 2. This is the first piece of evidence that the findings in Table 2 with regard to high school graduation are spurious.

Table 5 presents the results of our second falsification test, with the "FRPL/Foster Pseudo-Eligible in Border Grades × Post" rows bolded. In Columns 1-4 of Table 5, we see insignificant positive coefficients on "FRPL/Foster Pseudo-Eligible in Border Grades × Post", which suggests that there was no general downward trend in high school GPA for disadvantaged youth. Thus, our findings in Table 2 with regard to high school GPA (i.e., that CBS caused a mild reduction in high school GPA of 0.01 to 0.02) are robust to these falsification tests. Since the (insignificant) point estimates on Pseudo-Eligible in Border Grades × Post are positive, it suggests that GPAs of eligible students would have likewise risen absent the CBS program – if so, the Table 2 results would be an underestimate of the negative effect of the CBS program on eligible students' grades.

[Insert Table 5]

In Columns 5-8 of Table 5, we see positive significant "FRPL/Foster Pseudo-Eligible in Border Grades × Post" coefficients on high school graduation. These coefficients are nearly the same magnitude (around 3.5 percentage points) as the "FRPL/Foster Eligible/Pseudo-Eligible × Post" results in Table 2. This result, particularly when combined with the first falsification test, *strongly suggests that the association between CBS eligibility and high school graduation is spurious and simply reflects a generally improving rate of high school graduation of disadvantaged youth relative to advantaged youth, and not an effect of the CBS program.*

Finally, in columns 9-12 of Table 5, we see small insignificant "FRPL/Foster Pseudo-Eligible in Border Grades × Post" coefficients on incarceration. This result suggests that the negative impact of CBS on likelihood of incarceration is robust.

6. Discussion and Conclusions

The importance of success in high school to society cannot be overstated. Of particular interest are significant high school achievement gaps between advantaged and traditionally disadvantaged students, and these gaps are a significant factor in explaining the associated disparities in college access and success.

Legislators in Washington State attempted to close these achievement gaps for low-income students through the use of an early commitment need-based scholarship pledge: the College Bound Scholarship. A key aspect of this new policy is to provide an early promise that should encourage low-income students to fulfill pledge requirements to stay out of trouble and academically prepare for college while in high school. To our knowledge, this is the first study of

an early-commitment needs-based scholarship that uses quasi-experimental methods and eligibility for the program to identify whether the pledge program has an effect on students' academic preparation or involvement with serious criminal activity.

We find mixed effects. High school GPAs for students *fall* slightly, and this finding is robust to various falsification tests. But it is important to be cautious in interpreting this finding. For instance, the slight drop in GPA (driven mainly by Hispanic students) may be consistent with the CBS program incentivizing disadvantaged students to take more challenging courses that better prepare them for college.

We also find evidence of large increases in on-time high school graduation rates for CBS eligible cohorts of students. This finding is encouraging, but it appears to not to be driven by the CBS program as two falsification tests indicate that this result is likely to be due to a time trend positively impacting low-income students relative to higher-income students. It is outside the scope of this study to determine what might have yielded better graduation results for disadvantaged students, but the CBS program was implemented during a period of dramatic economic change i.e. at the start of the Great Recession, and it is a reasonable to hypothesize that the economic recovery affected lower income students in different ways than higher income students.

A comprehensive evaluation of the effects of this pledge program will require an analysis of the effects on rates of college entry and persistence for eligible low-income youth. We plan to evaluate such effects as data becomes available on these outcomes. However, our results to this point suggest some cautious optimism about the program. A key motivation for the early commitment of college funds is the idea that it will lead to behavioral changes, and our findings indicate a drop off in the likelihood of incarceration associated with CBS eligibility. This is consistent with the pledge students take to stay out of trouble, and, importantly, these findings are robust to our falsification tests.

Given the high social and financial costs to society of incarcerating young adults, even modest causal effects can have large societal benefits. Our estimates suggest that the CBS program is having a causal impact on the likelihood of incarceration for eligible students, lowering incarceration rates by 0.11 percentage points, or about 42 fewer incarcerated young adults per cohort. We use this figure combined with evidence from the Washington State Institute for Public Policy (Aos, 2003) and Lochner and Moretti (2004) to obtain a crude estimate of the social value of this reduction in incarceration.

Aos estimates the cost per year of prison of $41,302 in 2012 dollars, inclusive of prison operating and capital costs, but exclusive of victim costs. Lochner and Moretti estimate that each arrest has an average social benefit, inclusive of victim and incarceration costs, of $195,302 in 2012 dollars. Using these estimates as bounds, and multiplying by 42, we find that the CBS program has a social benefit of $1.7 to $8.2 million per cohort, or $45 to $215 per eligible student.

The average scholarships received by 2012-13 CBS recipients (note this is distinct from the cost of the CBS program as we discuss below) was $8,370 and the total cost of the scholarships was just over $39 million.[35] This figure suggests that the incarceration reduction itself does not fully pay for the scholarships received, but, viewed another way, any positive behavioral changes, observed or otherwise, associated with the CBS program are likely to be cost-effective given that CBS eligible students are also eligible for Washington's State Need

[35] These figures are derived from Figure 8 in WSAC (2017).

Grant. The way the CBS and State Need Grant programs are administered (as of 2015-16) is such that eligible students first receive the maximum allowable Need Grant funding from the state and then any supplement due to the CBS program (WSAC, 2015). Thus, the majority of funds received by CBS recipients likely represents funding that they would have received under the State Need Grant even in the absence of the CBS program.[36] In other words, the early promise of CBS funds may have little effect on state outlays, while yielding positive benefits.[37]

There is some question, however, about horizontal equity as a result of the CBS program. Funding for this pledge program may siphon off other state-based financial aid that would otherwise go to low-income students who failed to sign-up for the program in middle school, were poor in the wrong year (e.g., income-eligible in 6th or 9th grade, but not in 7th or 8th grade when the pledge can be signed), or moving into the state during high school and thus not able to sign the pledge in middle school. Since these pledge programs are, in effect, a promise made by the state, it is hard to not fully fund such promises. Yet, in contrast, Washington State's older mechanism for providing funding for low-income college students, the State Need Grant, has been underfunded; "every year since 2009, at least a quarter of eligible students have not received grants due to lack of state funding" (Cauce, Sundborg, and Pan, 2017). Given that there is therefore the potential for a the tradeoffs in terms of which students receive college aid under Washington's different programs, it is worthwhile to investigate the extent to which the early commitment element of the CBS program may influence whether other needy, but non-CBS qualifying, students fail to receive state aid when it comes time to enroll in college.

[36] In 2015-16, for instance, CBS recipients, on average, received $7,085 from Washington state to pay for college, but only $1,343 came from the funding designated for the College Bound Scholarship; the remaining 81% ($5,742) was State Need Grant funds (WSAC, 2017).

[37] There is, of course, some cost associated with administering the CBS program, but this is likely small relative to the cost of scholarships.

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Figure 1: Definition of Placebo "Border-Eligible" Students

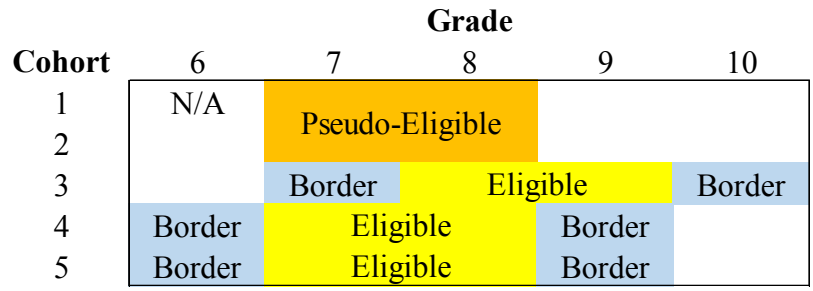

Table 1: Student Characteristics and Student Outcomes by Eligibility Status, Pre- and Post-Policy

	Pre-Policy			Post-Policy		
	Placebo-Eligible	Not Eligible	Difference	Eligible	Not Eligible	Difference
Panel A: Student Characteristics						
Proportion Female	0.48	0.48	0.00	0.49	0.49	0.00
Proportion Migrant	0.08	0.00	0.08	0.09	0.00	0.08
Proportion Bilingual	0.15	0.01	0.13	0.21	0.03	0.19
Proportion Gifted	0.03	0.10	-0.07	0.05	0.16	-0.10
Proportion Homeless	0.09	0.02	0.08	0.13	0.02	0.11
Proportion Disabled	0.21	0.11	0.09	0.23	0.15	0.09
Proportion Home Language not English	0.24	0.04	0.20	0.30	0.07	0.23
Proportion White	0.48	0.79	-0.30	0.44	0.75	-0.31
Proportion Hispanic	0.27	0.05	0.22	0.31	0.07	0.25
Proportion African American	0.07	0.03	0.05	0.07	0.02	0.04
Proportion Asian	0.06	0.07	-0.01	0.06	0.07	-0.02
Proportion Other Race	0.09	0.06	0.03	0.11	0.08	0.03
7th Grade Math Test*	-0.45	0.28	-0.73	-0.41	0.29	-0.70
(st. dev.)	(0.95)	(0.95)		(0.93)	(0.95)	
7th Grade Reading Test*	-0.42	0.26	-0.68	-0.35	0.25	-0.61
(st. dev.)	(0.97)	(0.95)		(0.99)	(0.93)	
Panel B: Student Outcomes						
12th Grade GPA**	2.38	2.92	-0.55	2.41	2.99	-0.58
(st. dev.)	(0.88)	(0.79)		(0.86)	(0.75)	
Proportion Graduation on Time**	0.59	0.84	-0.25	0.65	0.87	-0.22
Proportion Incarcerated in State Prison ***	0.0034	0.0008	0.0026	0.0020	0.0003	0.0017
Unique Students	76,496	93,391		114,612	130,885	

* When 7th grade math or reading scores are missing, we have imputed them using multiple imputations. The summary statistics provided here have been combined via Rubin's rule.

** Students who transferred out of Washington State public schools have been removed from the graduation rate calculation and therefore are a subset of the "Unique Students" displayed here. Similarly, students who drop out, transfer, or have unreported GPAs do not contribute to the 12th Grade GPA calculation.

*** Due to data limitations, the felony rates are based off of the cohort just prior to implementation of CBS and just after implementation. Therefore, these rates are based off of a subset of the "Unique Students" shown above.

Table 2: Estimated Effect of Washington's College Bound Scholarship Program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Independent Variable	12th Grade GPA				On Time HS Diploma				Felony Conviction			
FRPL/Foster Eligible/Pseudo-Eligible × Post	**-.018****	**-.012+**	**-.020****	**-.012+**	**.035*****	**.032*****	**.033*****	**.032*****	**-.0010***	**-.0011****	**-.0010***	**-.0011****
	(.007)	**(.006)**	**(.007)**	**(.006)**	**(.004)**	**(.003)**	**(.004)**	**(.003)**	**(.0004)**	**(.0004)**	**(.0004)**	**(.0004)**
Post-Policy	.017	-.016	.020	-.016	-.036***	.018+	-.033***	.018+	0.0010	0.0017	.0009	.0017
	(.017)	(.018)	(.017)	(.018)	(.009)	(.011)	(.009)	(.011)	(.0010)	(.0013)	(.0010)	(.0013)
FRPL/Foster Eligible/Pseudo-Eligible †	-.250***	-.234***	-.230***	-.234***	-.152***	-.127***	-.134***	-.127***	.0017***	.0016***	.0015***	.0016***
	(.007)	(.006)	(.006)	(.006)	(.004)	(.003)	(.003)	(.003)	(.0004)	(.0004)	(.0004)	(.0004)
Hispanic	-.149***	-.136***	-.138***	-.136***	-.033***	-.028***	-.023***	-.028***	.0006+	.0004	.0005	.0004
	(.009)	(.007)	(.008)	(.007)	(.004)	(.003)	(.004)	(.003)	(.0004)	(.0003)	(.0004)	(.0003)
African American	.021*	.029**	.041***	.029**	.037***	.046***	.053***	.046***	.0020*	.0023**	.0019*	.0023**
	(.011)	(.009)	(.011)	(.009)	(.006)	(.005)	(.006)	(.005)	(.0008)	(.0008)	(.0008)	(.0008)
Asian	.206***	.202***	.212***	.202***	.072***	.067***	.077***	.067***	.0002	.0004	.0001	.0004
	(.008)	(.008)	(.008)	(.008)	(.005)	(.005)	(.006)	(.005)	(.0003)	(.0003)	(.0003)	(.0003)
Other Race	-.106***	-.098***	-.099***	-.098***	-.048***	-.042***	-.041***	-.042***	.0004	.0006	.0004	.0006
	(.007)	(.006)	(.007)	(.006)	(.004)	(.003)	(.004)	(.003)	(.0004)	(.0004)	(.0004)	(.0004)
Female	.266***	.267***	.266***	.267***	.044***	.042***	.043***	.042***	-.0020***	-.0019***	-.0020***	-.0019***
	(.003)	(.003)	(.003)	(.003)	(.002)	(.002)	(.002)	(.002)	(.0002)	(.0002)	(.0002)	(.0002)
Disability Status	.142***	.136***	.138***	.136***	-.033***	-.038***	-.037***	-.038***	.0005	.0005	.0005	.0005
	(.007)	(.007)	(.007)	(.007)	(.004)	(.004)	(.004)	(.004)	(.0004)	(.0004)	(.0004)	(.0004)
Standardized 7th grade math score	.309***	.303***	.306***	.303***	.070***	.065***	.066***	.065***	-.0005**	-.0005*	-.0005*	-.0005*
	(.004)	(.004)	(.004)	(.004)	(.002)	(.002)	(.002)	(.002)	(.0002)	(.0002)	(.0002)	(.0002)
Standardized 7th grade reading score	.131***	.130***	.131***	.130***	.042***	.040***	.042***	.040***	-.0003	-.0002	-.0003	-.0002
	(.003)	(.003)	(.003)	(.003)	(.001)	(.001)	(.001)	(.001)	(.0002)	(.0002)	(.0002)	(.0002)
Controlling for:												
Middle School Fixed Effect		X		X		X		X		X		X
Middle School Eligible Rate			X	X			X	X			X	X
Observations	320,293	320,293	320,293	320,293	364,537	364,537	364,537	364,537	164,517	164,517	164,517	164,517

Note: †: For cohorts 1,2,4, and 5 the grades used to determine FRPL/foster eligibility are 7th and 8th grade. For cohort 3, the first cohort of policy implementation, the grades used are 8th and 9th. Note that the policy relevant omitted group of students is non-FRPL/foster eligible foster students in grades 7 and 8, in cohorts 1 and 2. p-values from two-sided t-test: +p<0.10, *p<0.05, **p<0.01, ***p<0.001. Models control for race/ethnicity, gender, age in 8th grade, school region, 7th grade math WASL scores, modified test status, out-of-grade level test status, disability status, bilingualism, housing status, migrant status, English Language Learning status, and highly capable/gifted program participation. Reference categories are White for race, male for gender, and non-disabled. Standard errors are clustered at the middle school level.

Table 3: Estimated Effect of Washington's College Bound Scholarship Program for Race and Gender Subgroups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample Restricted to:							
Whites	X						
Hispanics		X					
African Americans			X				
Asian Americans				X			
Multiple or Other Race					X		
Females						X	
Males							X
Independent Variable	Panel A: 12th Grade GPA						
FRPL/Foster Eligible/Pseudo-Eligible × Post	**-.003**	**-.060****	**-.006**	**.014**	**-.002**	**-.011**	**-.014**
	(.007)	**(.019)**	**(.025)**	**(.022)**	**(.018)**	**(.008)**	**(.008)**
Post-Policy	.007	-.054+	.011	.010	.058	-.017	-.013
	(.019)	(.031)	(.069)	(.057)	(.039)	(.020)	(.021)
FRPL/Foster Eligible/Pseudo-Eligible †	-.260***	-.165***	-.190***	-.121***	-.242***	-.217***	-.251***
	(.006)	(.016)	(.019)	(.018)	(.017)	(.007)	(.008)
Hispanic						-.147***	-.124***
						(.008)	(.009)
African American						.022*	.037**
						(.011)	(.013)
Asian						.185***	.219***
						(.009)	(.010)
Other Race						-.119***	-.077***
						(.008)	(.007)
Female	.274***	.269***	.278***	.231***	.245***		
	(.004)	(.007)	(.013)	(.009)	(.009)		
Disability Status	.121***	.204***	.145***	.083***	.175***	.134***	.139***
	(.008)	(.013)	(.025)	(.020)	(.015)	(.009)	(.008)
Standardized 7th grade math score	.309***	.296***	.248***	.284***	.302***	.308***	.298***
	(.004)	(.008)	(.014)	(.008)	(.008)	(.004)	(.005)
Standardized 7th grade reading score	.132***	.131***	.104***	.127***	.128***	.129***	.132***
	(.003)	(.006)	(.011)	(.006)	(.007)	(.003)	(.004)
Controlling for:							
Middle School Fixed Effect	X	X	X	X	X	X	X
Observations	205,799	49,995	12,028	22,469	30,002	158,162	162,131

Note: †: For cohorts 1,2,4, and 5 the grades used to determine FRPL/foster eligibility are 7th and 8th grade. For cohort 3, the first cohort of policy implementation, the grades used are 8th and 9th. Note that the policy relevant omitted group of students is non-FRPL/foster eligible foster students in grades 7 and 8, in cohorts 1 and 2. p-values from two-sided t-test: +p<0.10, *p<0.05, **p<0.01, ***p<0.001. Models control for race/ethnicity, gender, age in 8th grade, school region, 7th grade math WASL scores, modified test status, out-of-grade level test status, disability status, bilingualism, housing status, migrant status, English Language Learning status, and highly capable/gifted program participation. Reference categories are White for race, male for gender, and non-disabled. Standard errors are clustered at the middle school level.

Table 3 is continued on the next page.

Table 3: Estimated Effect of Washington's College Bound Scholarship Program for Race and Gender Subgroups

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Sample Restricted to:							
Whites	X						
Hispanics		X					
African Americans			X				
Asian Americans				X			
Multiple or Other Race					X		
Females						X	
Males							X
Independent Variable	Panel B: On Time HS Diploma						
FRPL/Foster Eligible/Pseudo-Eligible × Post	**.025*** **	**.017+**	**.037***	**.041*** **	**.023***	**.033*** **	**.031*** **
	(.004)	**(.009)**	**(.018)**	**(.011)**	**(.010)**	**(.004)**	**(.004)**
Post-Policy	.014	-.007	.003	.049	.058**	.013	.024+
	(.012)	(.018)	(.049)	(.035)	(.022)	(.011)	(.013)
FRPL/Foster Eligible/Pseudo-Eligible †	-.133***	-.095***	-.102***	-.067***	-.137***	-.112***	-.141***
	(.004)	(.008)	(.012)	(.009)	(.009)	(.004)	(.004)
Hispanic						-.032***	-.023***
						(.004)	(.004)
African American						.044***	.049***
						(.006)	(.007)
Asian						.061***	.073***
						(.005)	(.006)
Other Race						-.048***	-.037***
						(.004)	(.004)
Female	.037***	.060***	.066***	.041***	.039***		
	(.002)	(.004)	(.009)	(.004)	(.005)		
Disability Status	-.050***	.008	-.016	-.061***	-.019*	-.036***	-.036***
	(.004)	(.007)	(.014)	(.012)	(.008)	(.005)	(.004)
Standardized 7th grade math score	.060***	.080***	.074***	.051***	.077***	.060***	.068***
	(.002)	(.004)	(.007)	(.005)	(.005)	(.002)	(.002)
Standardized 7th grade reading score	.034***	.054***	.054***	.032***	.047***	.037***	.044***
	(.001)	(.003)	(.006)	(.004)	(.005)	(.002)	(.002)
Controlling for:							
Middle School Fixed Effect	X	X	X	X	X	X	X
Observations	229,169	60,373	14,795	24,225	35,975	178,258	186,279

Note: †: For cohorts 1,2,4, and 5 the grades used to determine FRPL/foster eligibility are 7th and 8th grade. For cohort 3, the first cohort of policy implementation, the grades used are 8th and 9th. Note that the policy relevant omitted group of students is non-FRPL/foster eligible foster students in grades 7 and 8, in cohorts 1 and 2. p-values from two-sided t-test: +p<0.10, *p<0.05, **p<0.01, ***p<0.001. Models control for race/ethnicity, gender, age in 8th grade, school region, 7th grade math WASL scores, modified test status, out-of-grade level test status, disability status, bilingualism, housing status, migrant status, English Language Learning status, and highly capable/gifted program participation. Reference categories are White for race, male for gender, and non-disabled. Standard errors are clustered at the middle school level.

Table 3 is continued on the next page.

Table 3: Estimated Effect of Washington's College Bound Scholarship Program for Race and Gender Subgroups

	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Sample Restricted to:							
Whites	X						
Hispanics		X					
African Americans			X				
Asian Americans				X			
Multiple or Other Race					X		
Females						X	
Males							X
Independent Variable	Panel C: Incarcerated in State Prison						
FRPL/Foster Eligible/Pseudo-Eligible × Post	**-.0009**	**.0000**	**-.0000**	**-.0010**	**-.0013**	**-.0005+**	**-.0018***
	(.0006)	**(.0011)**	**(.0030)**	**(.0008)**	**(.0015)**	**(.0003)**	**(.0008)**
Post-Policy	.0013	.0009	.0147	-.0023	.0015	-.0008	.0041+
	(.0014)	(.0032)	(.0181)	(.0037)	(.0050)	(.0008)	(.0024)
FRPL/Foster Eligible/Pseudo-Eligible †	.0014***	.0010	-.0008	.0010	.0021+	.0003	.0028***
	(.0004)	(.0012)	(.0028)	(.0010)	(.0012)	(.0002)	(.0007)
Hispanic						-.0002	.0010
						(.0002)	(.0007)
African American						-.0000	.0044**
						(.0003)	(.0015)
Asian						-.0002	.0008
						(.0001)	(.0005)
Other Race						.0003	.0010
						(.0003)	(.0008)
Female	-.0012***	-.0031***	-.0066***	-.0008*	-.0028***		
	(.0002)	(.0005)	(.0015)	(.0004)	(.0007)		
Disability Status	.0002	.0010	.0008	.0008	-.0001	-.0007**	.0009
	(.0004)	(.0011)	(.0029)	(.0010)	(.0014)	(.0002)	(.0006)
Standardized 7th grade math score	-.0004*	-.0002	-.0021	-.0003	-.0003	-.0001	-.0007+
	(.0002)	(.0006)	(.0016)	(.0003)	(.0007)	(.0001)	(.0004)
Standardized 7th grade reading score	-.0002	-.0006	.0010	.0003	-.0007	-.0001	-.0004
	(.0002)	(.0005)	(.0017)	(.0002)	(.0007)	(.0001)	(.0004)
Controlling for:							
Middle School Fixed Effect	X	X	X	X	X	X	X
Observations	102,178	27,635	7,341	10,366	16,997	79,919	84,598

Note: †: For cohorts 1,2,4, and 5 the grades used to determine FRPL/foster eligibility are 7th and 8th grade. For cohort 3, the first cohort of policy implementation, the grades used are 8th and 9th. Note that the policy relevant omitted group of students is non-FRPL/foster eligible foster students in grades 7 and 8, in cohorts 1 and 2. p-values from two-sided t-test: +p<0.10, *p<0.05, **p<0.01, ***p<0.001. Models control for race/ethnicity, gender, age in 8th grade, school region, 7th grade math WASL scores, modified test status, out-of-grade level test status, disability status, bilingualism, housing status, migrant status, English Language Learning status, and highly capable/gifted program participation. Reference categories are White for race, male for gender, and non-disabled. Standard errors are clustered at the middle school level.

Table 4: Falsification Test 1: Is There a Pre-Policy Trend?

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	12th Grade GPA				On Time HS Diploma			
FRPL/Foster Eligible/Pseudo-Eligible × Post	-.0152+	-.0092	-.0184*	-.0092	.0475***	.0426***	.0446***	.0427***
	(.0082)	(.0078)	(.0084)	(.0078)	(.0045)	(.0041)	(.0047)	(.0041)
FRPL/Foster Eligible/Pseudo-Eligible × Cohort 2	**.0056**	**.0061**	**.0034**	**.0061**	**.0257*** **	**.0209*** **	**.0233*** **	**.0210*** **
	(.0089)	**(.0082)**	**(.0089)**	**(.0082)**	**(.0049)**	**(.0047)**	**(.0051)**	**(.0047)**
Post-Policy	.0171	-.0149	.0241	-.0148	-.0338**	.0201+	-.0266*	.0197+
	(.0212)	(.0198)	(.0203)	(.0198)	(.0117)	(.0117)	(.0114)	(.0118)
Cohort 2	.0000	.0024	.0073	.0024	.0053	.0032	.0118	.0029
	(.0187)	(.0161)	(.0189)	(.0161)	(.0107)	(.0086)	(.0104)	(.0086)
FRPL/Foster Eligible/Pseudo-Eligible †	-.2526***	-.2375***	-.2316***	-.2374***	-.1651***	-.1374***	-.1455***	-.1378***
	(.0081)	(.0072)	(.0079)	(.0073)	(.0047)	(.0040)	(.0042)	(.0040)
Hispanic	-.1493***	-.1357***	-.1376***	-.1357***	-.0335***	-.0276***	-.0227***	-.0276***
	(.0090)	(.0069)	(.0083)	(.0069)	(.0043)	(.0034)	(.0042)	(.0034)
African American	.0212*	.0295**	.0406***	.0295**	.0375***	.0462***	.0534***	.0463***
	(.0108)	(.0095)	(.0109)	(.0095)	(.0062)	(.0052)	(.0061)	(.0052)
Asian	.2064***	.2017***	.2125***	.2017***	.0724***	.0669***	.0774***	.0669***
	(.0082)	(.0077)	(.0084)	(.0077)	(.0052)	(.0047)	(.0059)	(.0047)
Other Race	-.1060***	-.0979***	-.0989***	-.0979***	-.0475***	-.0421***	-.0411***	-.0421***
	(.0067)	(.0061)	(.0067)	(.0061)	(.0044)	(.0035)	(.0042)	(.0035)
Female	.2665***	.2666***	.2663***	.2666***	.0436***	.0420***	.0433***	.0420***
	(.0034)	(.0034)	(.0034)	(.0034)	(.0019)	(.0018)	(.0019)	(.0018)
Disability Status	.1422***	.1361***	.1380***	.1361***	-.0330***	-.0381***	-.0370***	-.0381***
	(.0070)	(.0070)	(.0070)	(.0070)	(.0039)	(.0036)	(.0039)	(.0036)
Standardized 7th grade math score	.3093***	.3029***	.3055***	.3029***	.0699***	.0647***	.0664***	.0647***
	(.0038)	(.0035)	(.0037)	(.0035)	(.0017)	(.0018)	(.0018)	(.0018)
Standardized 7th grade reading score	.1315***	.1301***	.1311***	.1301***	.0422***	.0399***	.0417***	.0399***
	(.0032)	(.0027)	(.0031)	(.0027)	(.0015)	(.0014)	(.0015)	(.0014)
Controlling for:								
Middle School Fixed Effect		X		X		X		X
Middle School Eligible Rate			X	X			X	X
Observations	320,293	320,293	320,293	320,293	364,537	364,537	364,537	364,537

Note: †: For cohorts 1,2,4, and 5 the grades used to determine FRPL/foster eligibility are 7th and 8th grade. For cohort 3, the first cohort of policy implementation, the grades used are 8th and 9th. Note that the policy relevant omitted group of students is non-FRPL/foster eligible foster students in grades 7 and 8, in cohort 1. p-values from two-sided t-test: +p<0.10, *p<0.05, **p<0.01, ***p<0.001. Models control for race/ethnicity, gender, age in 8th grade, school region, 7th grade math WASL scores, modified test status, out-of-grade level test status, disability status, bilingualism, housing status, migrant status, English Language Learning status, and highly capable/gifted program participation. Reference categories are White for race, male for gender, and non-disabled. Standard errors are clustered at the middle school

Table 5: Falsification Test 2: Is There a Post-Policy Change in Outcomes for Ineligible, Yet Similarly Disadvantaged, Youth?

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	12th Grade GPA				On Time HS Diploma				Felony Conviction			
FRPL/Foster Eligible/Pseudo-Eligible × Post	-.0109	-.0043	-.0127+	-.0044	.0269***	.0255***	.0253***	.0255***	-.0012**	-.0014***	-.0013**	-.0014***
	(.0069)	(.0067)	(.0070)	(.0067)	(.0036)	(.0032)	(.0036)	(.0032)	(.0004)	(.0004)	(.0004)	(.0004)
FRPL/Foster Pseudo-Eligible in Border Grades × Post	**.0099**	**.0158**	**.0096**	**.0159**	**.0348*** **	**.0346*** **	**.0342*** **	**.0345*** **	**-.0002**	**-.0002**	**-.0002**	**-.0001**
	(.0120)	**(.0114)**	**(.0123)**	**(.0114)**	**(.0070)**	**(.0064)**	**(.0071)**	**(.0064)**	**(.0006)**	**(.0007)**	**(.0006)**	**(.0007)**
Hispanic	-.1422***	-.1302***	-.1323***	-.1302***	-.0345***	-.0287***	-.0245***	-.0287***	.0006	.0004	.0005	.0004
	(.0091)	(.0070)	(.0085)	(.0070)	(.0042)	(.0034)	(.0041)	(.0034)	(.0004)	(.0003)	(.0004)	(.0003)
African American	.0277**	.0342***	.0443***	.0342***	.0368***	.0446***	.0517***	.0446***	.0020*	.0022**	.0018*	.0022**
	(.0107)	(.0094)	(.0107)	(.0094)	(.0061)	(.0051)	(.0060)	(.0051)	(.0008)	(.0008)	(.0008)	(.0008)
Asian	.2083***	.2035***	.2135***	.2035***	.0721***	.0668***	.0769***	.0668***	.0002	.0004	.0001	.0004
	(.0082)	(.0078)	(.0084)	(.0078)	(.0051)	(.0046)	(.0057)	(.0046)	(.0003)	(.0003)	(.0003)	(.0003)
Other Race	-.0997***	-.0933***	-.0937***	-.0933***	-.0465***	-.0417***	-.0405***	-.0417***	.0004	.0006	.0003	.0006
	(.0066)	(.0060)	(.0066)	(.0060)	(.0043)	(.0034)	(.0041)	(.0034)	(.0004)	(.0004)	(.0004)	(.0004)
Female	.2671***	.2672***	.2669***	.2672***	.0438***	.0422***	.0435***	.0422***	-.0020***	-.0019***	-.0020***	-.0019***
	(.0034)	(.0034)	(.0034)	(.0034)	(.0019)	(.0018)	(.0019)	(.0018)	(.0002)	(.0002)	(.0002)	(.0002)
Disability Status	.1418***	.1365***	.1380***	.1365***	-.0352***	-.0397***	-.0391***	-.0397***	.0005	.0005	.0005	.0005
	(.0070)	(.0070)	(.0070)	(.0070)	(.0039)	(.0036)	(.0039)	(.0036)	(.0004)	(.0004)	(.0004)	(.0004)
Standardized 7th grade math score	.3060***	.3006***	.3028***	.3006***	.0688***	.0640***	.0656***	.0640***	-.0005**	-.0005*	-.0005*	-.0005*
	(.0037)	(.0035)	(.0037)	(.0035)	(.0017)	(.0018)	(.0018)	(.0018)	(.0002)	(.0002)	(.0002)	(.0002)
Standardized 7th grade reading score	.1302***	.1289***	.1299***	.1289***	.0416***	.0395***	.0412***	.0395***	-.0003	-.0002	-.0003	-.0002
	(.0031)	(.0027)	(.0031)	(.0027)	(.0015)	(.0014)	(.0015)	(.0014)	(.0002)	(.0002)	(.0002)	(.0002)
Controlling for:												
Cohort Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X
Patterns in FRPL/Foster Eligibility	X	X	X	X	X	X	X	X	X	X	X	X
Middle School Fixed Effect		X		X		X		X		X		X
Middle School Eligible Rate			X	X			X	X			X	X
Observations	320,293	320,293	320,293	320,293	364,537	364,537	364,537	364,537	164,517	164,517	164,517	164,517

Note: p-values from two-sided t-test: +p<0.10, *p<0.05, **p<0.01, ***p<0.001. Models control for race/ethnicity, gender, age in 8th grade, school region, 7th grade math WASL scores, modified test status, out-of-grade level test status, disability status, bilingualism, housing status, migrant status, English Language Learning status, and highly capable/gifted program participation. Reference categories are White for race, male for gender, and non-disabled. Standard errors are clustered at the middle school level.