On Money and Motivation:
A Quasi-Experimental Analysis of Financial Incentives for College Achievement

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Abstract: *Programs linking substantial amounts of college aid to academic achievement could work either by lowering the cost of college or by inducing additional student effort. I examine the PROMISE program in West Virginia, which offers free tuition to students who maintain a minimum GPA and course load. Using administrative data, I exploit discontinuities in the eligibility formula and the timing of implementation to estimate causal effects. I find robust and significant impacts on key academic outcomes. Impacts are concentrated around the annual requirements for scholarship renewal, suggesting that the program works via incentives for academic achievement, not simply by relaxing financial constraints.*

I. Introduction

The United States has long ranked as the most educated nation in the world, but this status is beginning to slip. While bachelor's degree attainment rates have risen substantially in other countries over the past three decades, they have barely budged in the U.S. [1] Those who do earn degrees are taking longer to do so (Turner 2004; Bound, Lovenheim and Turner 2007). National figures also mask considerable variation in attainment at the state level. Statistics from the 2000 census show that only 16 percent of those born in the lowest-ranked state of West Virginia (age 25 and older) had attained a bachelor's degree—well below the national rate of 24 percent, and comparable to the U.S. average from the late 1970s.

It is not entirely obvious, however, which policy options are most likely to increase college attainment. Although much research and policy has focused on increasing college enrollments (see Dynarski 2002 for a review of this literature), entry alone is no guarantee of success. Only 36 percent of college entrants complete a bachelor's degree within six years and a mere 18 percent complete within four years.[2] There is no consensus on why so many entrants fail to complete a degree, or fail to complete on time, but policy debates have often focused on concerns either about students' financial constraints or their academic preparation/motivation.

These joint concerns may explain the increasing popularity of programs offering large financial incentives for college achievement.[3] These programs simultaneously reduce the cost of

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[1] According to the OECD's "Education at a Glance 2007," the United States was tied with Norway for the highest proportion of 25-64 year olds with a Bachelor's degree or higher (30 percent, see Table A.1.3a); however, among 25-34 year olds, the U.S. has fallen to sixth place, behind Norway, the Netherlands, Iceland, Korea, and Denmark.

[2] Author's calculations using data from NCES's Beginning Postsecondary Students (BPS) longitudinal study, 1996-2001, based on all degree-seeking students entering 2- and 4-year colleges. Among only those entering 4-year colleges, the corresponding rates are 67 percent and 36 percent, respectively.

[3] Although this paper focuses on incentives at the college level, several studies have examined educational incentive programs targeted at younger students (see, e.g., Roland Fryer's ongoing experiments with incentives for elementary school students in New York City and other U.S. cities [www.edlabs.harvard.edu]; Angrist and Lavy [forthcoming] examine incentives for high school achievement in Israel; Bettinger [2008] examines incentives for passing standardized tests as early as third grade in Ohio; Jackson [2008] examines incentives for A.P. testing in Texas;

college, and provide clear inducements for student effort. At least fourteen states have

introduced large-scale merit-based college scholarship programs since 1991, requiring students

to meet academic criteria both in order to initially qualify and to renew the awards each year.[4]

West Virginia joined this group in 2002 with the inauguration of the PROMISE scholarship,

which provided free tuition and fees at any state public institution for qualified students, but only

as long as they maintained a miminum GPA and course load during college.

Large merit-based scholarships such as West Virginia's PROMISE may affect outcomes

among eligible college enrollees via two mechanisms: cost-of-college effects and incentive

effects. The PROMISE scholarship is a high-value award, worth an average of approximately

$10,000 over four years for those who initially qualify.[5] Even without any academic incentives,

it might still enable some financially constrained students to enroll full-time rather than part-

time, or to attend for more semesters than they would have otherwise.[6] Lowering the cost of

college might also reduce student employment, thus enabling students to spend more time on

their coursework, raise their GPAs and accelerate their progress towards a degree.[7] Even among

the financially unconstrained, PROMISE generates direct incentives to increase academic effort

by establishing annual achievement requirements for renewal. Why might such external

motivation be needed? State-subsidized tuition and parent support mean that few students pay

Kremer, Miguel, and Thornton [forthcoming] examine incentives for high test scores for adolescent girls in Kenya; Behrman, Sengupta, and Todd [2005] and Schultz [2004] examine the PROGRESA program in Mexico). See Angrist and Lavy (forthcoming) for a review of the literature on incentives for younger students.

[4] This includes Arkansas, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Nevada, New Mexico, South Carolina, Tennessee, Florida, West Virginia, and Oklahoma, although Arkansas and Maryland have since phased out their programs. Of these, West Virginia has among the most stringent requirements for scholarship renewal.

[5] This figure is based on the first two PROMISE cohorts. The value has increased as college tuition has risen.

[6] Cost-of-college effects will be strongest when students are constrained in their ability (or willingness) to borrow, but may affect even unconstrained individuals via standard income and substitution effects. Note, however, that even this scholarship is relatively small in comparison to lifetime income or the cost of attendance including foregone wages.

[7] Simulations by Keane and Wolpin (2001) suggest that credit constraints primarily affect student employment rather than college enrollment decisions.

the full cost of an additional year of schooling. PROMISE may help resolve this principal-agent problem by aligning students' incentives with their funders' preferences.

Although previous research shows that merit-based aid, like traditional financial aid, can increase college enrollments (Kane 2003; Dynarski 2004; Cornwell, Mustard and Sridhar 2006), the scant evidence on outcomes *beyond* initial enrollment has been mixed. Dynarski (2008) estimates that large-scale merit-aid programs in Arkansas and Georgia reduced the college dropout rate by 3 to 5 percentage points.[8] But if Georgia's program increased persistence, it may also have lengthened time-to-degree: Cornwell, Lee, and Mustard (2005) find that the Georgia HOPE scholarship *reduced* the fraction of freshmen at the University of Georgia completing a full course load by 6 percentage points. One recent experiment with merit-based scholarships (up to $2,000) for low-income community college students finds positive and significant effects on full-time enrollment and credit accumulation over the first three semesters (Brock and Richburg-Hayes 2006); but another recent experiment offering even larger merit-based awards (up to $5,000) at a large public Canadian university found essentially no effects over the first two years (Angrist, Lang, and Oreopoulos 2009).[9] Importantly, no previous study has attempted to disentangle the mechanisms underlying observed effects.

This paper contributes to the literature by examining the effects of a large financial incentive program, West Virginia's PROMISE scholarship, on post-enrollment outcomes from semester-level GPAs and credits accumulated to degree completion five years later. I utilize two complementary quasi-experimental approaches to identify causal effects. The first is a

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[8] Dynarski's main finding is a three percentage point increase in the overall proportion of the population with a college degree, resulting from both higher college enrollments and a higher completion rate among enrollees. She bounds the effect on dropout rates by assuming that all or none of the individuals induced to enter college by the program complete a degree.
[9] Although Angrist, Lang, and Oreopoulos (2009) find no effects overall for students who were offered just the financial incentive, they do find positive effects for women who were offered extra student services in addition to the financial incentive.

regression-discontinuity (RD) analysis based on the college entrance test score cutoff for initial PROMISE eligibility. The second approach is a cohort analysis based on the discontinuous timing of program implementation. The primary threat to identification under either approach is differential selection into the West Virginia college system. In addition to controlling for an extremely rich set of covariates, I address this concern directly using a bounding exercise in the spirit of Lee bounds (2009).

To preview the results, I find significant positive effects on a range of outcomes. The RD and the cohort analysis generate broadly similar results, and the bounding exercise shows that the impacts are too large to be explained by differential selection. I find compelling evidence that cost-of-college effects alone cannot explain the results. Impacts are strongly concentrated around the specific annual achievement thresholds for PROMISE renewal, particularly the course load requirements. For example, at the end of the freshman year, PROMISE recipients were nearly 25 percentage points more likely to have earned 30 or more credits, the threshold for PROMISE renewal. Tellingly, the annual impacts are roughly constant in the freshman through junior years, but virtually disappear in the fourth year while students are still receiving PROMISE funds but no longer have the opportunity to renew. I conclude that a traditional grant with no strings attached would not produce the same pattern of effects.

Section II describes the PROMISE scholarship and the dataset in detail. Section III presents the empirical strategy and main results, including a bounding of the bias due to selection. Section IV investigates whether PROMISE works primarily by reducing the cost of college or by providing specific incentives for achievement. Section V discusses the results and implications for future research.

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II. West Virginia's PROMISE Scholarship

In 2002, West Virginia began offering PROMISE (Providing Real Opportunities to Maximize In-state Student Excellence) scholarships to promote academic achievement and encourage qualified students to stay in the state for college and, hopefully, beyond. The PROMISE scholarship covers full tuition and required fees for up to four years for eligible first-time freshmen who enroll full-time at a West Virginia public two- or four-year institution, or an "equivalent amount" at an eligible West Virginia private institution.[10] Full-time enrollment is defined as a minimum of 12 credit-hours per semester.[11]

Eligibility for the scholarship is based entirely on a student's academic record, not financial need. Incoming freshmen must have a 3.0 high school grade point average (GPA) both overall and within a set of "core courses." They must also have scored at least a 21 overall on the ACT or 1000 on the SAT.[12] West Virginia estimates that approximately 23 percent of their high school graduates (or about 40 percent of their in-state first-time freshmen) meet the initial eligibility requirements.[13] Thus, PROMISE recipients are not the academic elite, but neither are they average students. To renew the scholarship, undergraduates must successfully complete at least 30 credits per year and maintain a 3.0 cumulative GPA, although they are allowed a 2.75 GPA in their first year. Those who fail to meet renewal requirements once cannot later regain the scholarship. In the first two PROMISE cohorts, approximately 75 percent renewed the scholarship for a second year and approximately 50 percent retained the scholarship for four

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[10] This paper focuses on the first two cohorts of PROMISE recipients. Eligibility rules have changed several times since then, and in early 2009, awards were limited to a fixed dollar amount that may not cover full tuition.

[11] Credit hours are roughly intended to correspond to the number of hours of class time per week. Regular courses are typically worth 3-4 credits per semester, although some courses may be worth more or less than that.

[12] Composite ACT scores are calculated by averaging its four subject test sub-scores and rounding to the nearest whole number, so PROMISE's stated threshold of 21 translates into a true threshold of 20.50 along the underlying ACT scale.

[13] Phone conversation with Jack Toney, Director of State Financial Aid Programs, April 17, 2008 and author's calculations based on WV college entrants age 19 or younger.

years. The average value of the award in 2002-03 was $2,900 for the first year. Those who

initially qualified received an average of about $10,000 in PROMISE funds over four years.[14]

(See Appendix A for additional program details, including recent rule changes.)

The West Virginia Higher Education Policy Commission Data. The West Virginia

Higher Education Policy Commission (WVHEPC) is a state agency that maintains a

comprehensive database on the state's public college enrollees, and provided me de-identified

data on four cohorts of new entrants under a restricted-use data agreement (2000-01 through

2003-04). The data include limited background information such as age, race, gender, overall

high school GPA, and ACT and SAT scores if applicable.[15] No direct measure of family income

or wealth is available for the full sample. The data include complete college transcripts and

financial aid records for five years after initial enrollment. A unique feature of the data is that

they also include administrative records of quarterly employment and earnings for students who

worked in-state, acquired by WVHEPC from the state's Employment Security agency.[16]

III. Impacts on College Persistence, Performance and Completion

I utilize two complementary quasi-experimental strategies to identify causal effects: the

first is a regression-discontinuity (RD) that estimates the effect of being just above rather than

just below the test score threshold for initial eligibility; and the second approach is a cohort

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[14] This average includes students who failed to renew the scholarship for all four years.

[15] Over 90 percent of students took the ACT and approximately 15 percent took the SAT. For those that took both exams, the higher score is used to determine eligibility. SAT scores are converted to ACT scores using a national concordance table: http://www.collegeboard.com/prod_downloads/highered/ra/sat/satACT_concordance.pdf. Approximately ½ of 1 percent of the RD sample (34 students) scored 980 or 990 on the SAT, which is below the threshold for PROMISE but converts to an ACT of 21 (which would meet the PROMISE threshold). These students are assigned an ACT-equivalent of 20.

[16] In theory, the limitation to West Virginia employment is non-trivial given that West Virginia's two largest universities are located within a few miles of state borders. In practice, these earnings data appear quite comparable to students' self-reports on the FAFSA, which include earnings from any state (see Appendix A for additional details).

analysis based on the discontinuous timing of program implementation. The two approaches are much stronger together than either would be alone. The advantage of the RD is that it tightly links any observed impacts to an arbitrary program rule, eliminating several alternative explanations for the findings. Neither institutional policies, labor market conditions, nor students' background characteristics should vary discontinuously around the ACT threshold. The major limitations of the RD are that it estimates impacts only for those near the eligibility threshold, who represent only about 20 percent of all PROMISE recipients and who may differ from other students in their response to the program; also, because the threshold was known, the RD may be sensitive to differential selection around the cutoff.

The advantage of the cohort analysis, which compares similar students just before and after the implementation of PROMISE, is that it estimates the average treatment effects across all recipients, not just those near the threshold. These results may be less sensitive to selection concerns (discussed below), and if credible, are more useful than the RD findings. The drawback of this approach is that I have data for only two cohorts before and two cohorts after PROMISE. Without the RD, one might wonder whether any differences are truly attributable to the program, rather than to pre-existing trends, idiosyncratic variation in labor market conditions or institutional policies that just happened to coincide with PROMISE implementation.

Selection bias is the primary threat to validity in either approach. The analysis focuses on college enrollees, but the program may influence who becomes an eligible enrollee in the first place. Indeed, encouraging more students to meet eligibility thresholds and attend college in-state were explicit goals of the program. Selection bias could arise from three sources: 1) individuals who otherwise would have attended college out-of-state could choose to enroll in-state, 2) individuals who otherwise would not have enrolled in college could choose to do so, and

3) individuals who would have enrolled in college but failed to meet the eligibility criteria could work harder in order to reach them.[17] Only the first factor is likely to induce a positive bias in both the RD and cohort analysis; the second factor is likely to negatively bias both analyses, while the third is likely to negatively bias the cohort analysis but could cause a positive bias in the RD.[18]

I address these concerns explicitly with a bounding exercise, in the spirit of Lee (2009), after presenting the main results. For the moment, I simply note that all specifications control for two of the best predictors of college success—high school GPA and ACT score—as well as gender, race/ethnicity, and age at entry. Differential selection is only a concern to the extent it occurs on other unmeasured dimensions. Moreover, as explained above, the net effect of these compositional changes is *a priori* unclear.

Identification Based on Regression Discontinuity (RD) around the ACT Eligibility Threshold

For this analysis, I limit the sample to West Virginia residents entering in the first two years after PROMISE implementation who earned at least a 3.0 high school GPA.[19] For these students PROMISE receipt is strongly determined by ACT score (or SAT equivalent): the vast majority of those who score a 20.50 have access to the program while those who score only

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[17] A particular concern is that students may retake the ACT until they achieve the required score. On average, about 36 percent of ACT test-takers repeat the test at least once (Andrews and Ziomek 1998), unfortunately I was not able to obtain data on repeat test taking by state over time. Vigdor and Clotfelter (2003) find that the prevalence of retesting (at least with respect to the SAT at three selective college) places low-income and African-American students at a disadvantage because these groups are less likely to retest, all else equal.

[18] Note that if the best-achieving students just below the ACT threshold exert additional effort to become the lowest-achieving students just above the threshold, mean outcome levels on both side of the threshold would fall. The direction of bias resulting from such a shift could be positive in the RD, but is not necessarily so.

[19] In theory, one could also use the GPA cutoff for an RD analysis. In practice, however, the GPA requirement was rarely decisive. Among those meeting the GPA requirement, just scaling the ACT threshold increases PROMISE receipt by about 70 percentage points; but among those meeting the ACT requirement, just scaling the GPA threshold increases PROMISE receipt by only 13 to 25 percentage points, depending on bandwidth. This is likely because students near the GPA threshold are at much higher risk of being disqualified based on their "core course GPA," which is unobservable to me.

20.49 do not.[20] Except for PROMISE, students scoring just above 20.5 should not systematically

differ from those scoring just below. If this assumption holds, then one can examine outcomes

by ACT score and attribute any discontinuous jumps at the threshold to the effects of PROMISE.

Graphical analysis. Figure 1 confirms that PROMISE receipt increases sharply for those

just above the test score threshold. Nonetheless, about 7 percent of those just below the

eligibility threshold received PROMISE, and about 23 percent of those just above the threshold

did not. My preferred regressions will follow a "fuzzy" RD approach to adjust for the

discrepancy between apparent eligibility and actual PROMISE receipt, and the causes and

implications of this discrepancy will be discussed below.

Figures 2 and 3 plot the raw means of end-of-college outcomes by ACT score, along with

linear predictions. Figure 2 shows no discontinuities in the number of semesters of enrollment

over four years (a measure of persistence) or in typical weekly school-year earnings, but

indicates perceptible-if-modest discontinuities in total credits and cumulative GPA at the end of

four years.[21] The four panels of Figure 3 show clear and substantively important discontinuities

in the percent of students meeting key credit and GPA thresholds after four years as well as in

four- and five-year BA completion rates.

Estimation. Following Imbens and Lemieux (2008), I use a local linear regression

specification:

(1) $y_i = \alpha + \beta(above_i) + \varsigma(ACTdist_i * below_i) + \pi(ACTdist_i * above_i) + X_i\delta + \varepsilon_i$

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[20] Composite ACT scores are calculated by averaging its four subject test sub-scores and rounding to the nearest whole number.

[21] Typical weekly school-year earnings are based on the largest subset of data available for the full sample, including the sophomore spring semester (January-March), junior fall and spring semesters (October-March) and senior year fall semester (October-December). All earnings data are inflated to 2007 dollars. For the cumulative GPA measure, students who were not enrolled at the end of four years are assigned the cumulative GPA as of last enrollment.

where *above_i* is an indicator that the student is above the threshold, *below_i* is an indicator that the student is below the threshold, $ACTdist_i$ is the distance between the student's individual score and the underlying cutoff score (20.5), X_i is a vector of covariates including gender, race/ethnicity, age, high school GPA and high school GPA squared, and ε_i is an idiosyncratic error term.[22] The parameter β estimates the difference in outcomes at the threshold. Intuitively, the equation above approximates the prediction lines shown in Figures 2 and 3, except that the estimates are adjusted for small differences in covariates.

Equation (1) provides "sharp" RD estimates of the effect of crossing the ACT threshold, not the effect of receiving PROMISE. To estimate the effect of actually receiving PROMISE, a "fuzzy" RD is required. I implement this using an instrumental variables (IV) regression in which I first predict PROMISE receipt using the test score discontinuity, and then estimate the effect of predicted receipt on a given outcome. I again use a local linear specification:

(2a) $P_i = \lambda + \psi(above_i) + \gamma(ACTdist_i * below_i) + \varphi(ACTdist_i * above_i) + X_i\phi + \varepsilon_i$

(2b) $y_i = \alpha + \beta(\hat{P}_i) + \varsigma(ACTdist_i * below_i) + \pi(ACTdist_i * above_i) + X_i\delta + \varepsilon_i$

where P_i represents actual PROMISE receipt, $\hat{P}_i$ represents predicted PROMISE receipt, and all other variables are as defined in equation (1). Intuitively, this scales up the sharp RD estimates by a factor of 1.43 (or 1.00/0.70) to account for the fact that crossing the ACT threshold only increases PROMISE receipt by 70 percentage points.

In other contexts, sharp RD results can be interpreted as intent-to-treat (ITT) effects—bottom-line estimates of the effect of offering someone the treatment, whether or not they take it up—while the fuzzy RD gives the effects of actual treatment on the treated (TOT). But in this

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[22] Lee and Card (2008) suggest clustering standard errors by values of the forcing variable (ACT score, in this case) when the forcing variable is discrete rather than continuous. In this case, clustering by ACT score substantially *reduces* the standard errors (see Appendix B, Table. B.1), hence I rely on the more conservative unclustered standard errors.

case the sharp RD results may not be interpretable as ITT effects, depending upon what drives the discrepancy between apparent program eligibility and actual receipt. If take-up among the truly eligible is perfect (i.e. the discrepancy is driven entirely by misclassification of eligibility status), then the fuzzy RD can be interpreted as providing ITT estimates which simply have been corrected for misclassification bias, and the sharp RD has no useful interpretation.[23]

It is clear that my measure of PROMISE eligibility is imperfect. Above the threshold, the discrepancy between estimated eligibility and actual receipt is attributable largely to the requirement that students have earned a 3.0 high school GPA within a set of "core courses." Though I limit the sample to students with a 3.0 overall high school GPA, I do not observe the "core course" GPA, so not everyone above the threshold is truly eligible. The ACT score itself may be imperfectly measured, so not everyone below the threshold is truly ineligible.[24]

Moreover, it is implausible that much of the discrepancy between estimated eligibility and actual receipt could be driven by truly eligible students choosing to enroll in college but failing to take up the scholarship. The program was introduced with great fanfare, highly publicized, and simple to understand, so lack of awareness is an unlikely explanation.[25] Nor does claiming the scholarship require much paperwork, and even students who missed the deadline or only learned about PROMISE upon college enrollment could apply late and still

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[23] Note: in the case of perfect take-up among the truly eligible, the ITT effects equal the TOT effects.

[24] I have one set of scores per student, as reported by individual institutions from their college application data, but PROMISE eligibility is officially determined by scores obtained directly from the relevant testing agency. If a student took the test more than once, this could introduce conflicts, as could reporting errors in the application data. For example, college applications often allow students to report results from more than one testing session, but the WVHEPC data only allow for one set of results. In some cases the first or last score may be recorded rather than the highest score.

[25] Jack Toney, West Virginia's Director of State Financial Aid Programs, indicated that it would be highly unlikely for a high school student to be unaware of the program, particularly if they were college-bound (personal communication).

receive funding in the spring term.[26] For these reasons, I focus on the fuzzy RD results and interpret them as ITT estimates which have been corrected for misclassification of eligibility.

Results. The results are shown in Table 1. For context, column (1) shows mean outcomes for students just below the ACT threshold. Confirming the graphical analysis, PROMISE receipt has no significant impact on persistence (semesters enrolled, over four years) nor on typical weekly school-year earnings for students near the ACT threshold. Note that these are the two measures one might have expected to be most sensitive to reductions in college costs. Conditional on full-time enrollment, the direct marginal cost of taking additional courses is zero for most students with or without PROMISE.[27] Yet the program appears to have substantial impacts on cumulative GPA and total credits earned in the first year as well as moderate impacts on these outcomes after four years: total credits increase by 2.1 after the first year (equivalent to one-half to two-thirds of a course) and 4.6 after four years, while cumulative GPA increases by 0.16 after the first year (from a baseline of 2.52) and 0.10, or about one-tenth of a letter grade, after four years.[28]

The program also appears to have large effects on the percentage of students meeting key achievement thresholds. PROMISE recipients were 9.5 percentage points more likely to have completed 120 credits after four years (four times the 30-credit annual requirement of PROMISE and generally a minimum requirement for a BA).[29] They were also 9 percentage points more likely to have a 3.0 cumulative GPA. Finally, PROMISE generates large and statistically

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[26] Eligible students must also submit the federal financial aid form (the FAFSA) and enroll full-time to claim PROMISE; however, the discrepancy between apparent PROMISE eligibility and actual receipt persists even if I limit the sample to FAFSA filers who enrolled full-time. It is particularly implausible that a truly eligible student would enroll full-time and take up federal student aid, while simultaneously declining PROMISE.

[27] In WV as in many other states, full-time students are charged a flat tuition rate so even outside of PROMISE, additional courses are free.

[28] Note that the GPA increases cannot readily be explained by grade inflation, as instructors at large public institutions are unlikely to know who within a given course is just above or just below the PROMISE threshold.

[29] Requirements are often higher, depending on the degree program.

significant impacts on BA completion. Four-year BA completion rates rise by 9.4 percentage points from a baseline of just 16 percent (more than a 50 percent increase). Five-year BA completion rises by 4.5 percentage points from a baseline of 37 percent (a 12 percent increase). The difference between the four- and five-year impacts suggests that PROMISE not only increases graduation rates, but also reduces time-to-degree.[30]

The bottom of Table 1 also shows differences in covariates around the cutoff (using the same LLR specification, but with no controls), for which one hopes to see no significant discontinuities. There are no differences in percent female, age at entry, or average first-year Pell Grant. Although those above the cutoff have *statistically* significantly higher high school GPAs, the difference is *substantively* very small, measuring only three hundredths of a GPA point, from a mean just below the cutoff of 3.46.

Robustness checks. Table 2 provides evidence that these results are highly robust to alternative specifications. First, I test whether the results are robust to the inclusion of additional background controls: an indicator of whether the student graduated from a private high school (as well as an indicator for whether the high school type was unknown) and a set of 55 indicators for the student's county of residence at entry.[31] This has virtually no effect on the estimates. Next, I test whether the results are sensitive to the choice of bandwidth (i.e., the range of ACT scores included in the analysis). As one might have predicted from the graphical analysis, the estimates fluctuate very little. Third, I test whether the results are sensitive to the choice of functional form. In column (5) of Table 2, I estimate a two-stage model identical to (2a) and

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[30] Impacts beyond five years are not yet available. In earlier cohorts prior to PROMISE, the vast majority of BA graduates (75 percent) completed their degree within five years; nonetheless, this still leaves the possibility that the graduation impact may attenuate further with a longer follow-up.

[31] These controls are not in the baseline specification for two reasons: first, almost as many students are missing information on high school type (1.6 percent of this sample) as attended a private high school (2.6 percent); second, controlling for county of residence at entry may unintentionally control for some effects of the program, if students move near their intended college prior to entry.

(2b) except for the addition of two quadratic terms for $ACTdist_i$, one for each side of the threshold. These local quadratic results indicate no systematic differences with the local linear specification, other than a noticeable increase in the standard errors. Finally, I perform a falsification check in which I re-estimate the baseline (sharp) RD specification using students who entered prior to 2002.[32] Since none of these students received PROMISE, the RD should estimate no effects for this group. Column (6) of Table 2 shows the results; indeed, no impacts are found.

Limitations. Figure 4 shows the density of enrollments in the WV system by ACT score before and after PROMISE. After PROMISE, there is a spike in the number of students with scores at or above the cutoff score. A formal test of the continuity of the density function following McCrary (2008) indicates that the discontinuity is indeed significant, raising the possibility of differential selection.[33] The consequences of differential selection will be estimated directly via a bounding exercise, after presenting the results from the cohort analysis. For the moment, I simply note that it is reassuring that there are no substantively significant discontinuities in observable characteristics around the threshold.[34]

Identification Based on Timing of Program Implementation

For this analysis, I limit the sample to 12,911 enrollees meeting both the high school GPA and ACT score requirements for PROMISE who entered in the two cohorts just before (2000-01 and 2001-02) and just after (2002-03 and 2003-04) the program was implemented.

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[32] Since none of these students received PROMISE, it is impossible to estimate a fuzzy RD for this group.
[33] This test is implemented via local linear regression using the density (by ACT score bins) after PROMISE as the dependent variable and a bandwidth of 10. The estimated jump in the density at ACT=21 is 3.4 percent (p=.002). Given that the density at ACT=21 is 15.5, this suggests that up to 3.4/(15.5-3.4)=28% of those just above the cutoff may be "marginal" students.
[34] A previous reader asked how it could be that there would be no noticeable differences in covariates in the presence of such a noticeable discontinuity in the density function. As will be discussed in the bounding exercise, even when there is significant selection into the program, the marginal enrollees still represent a minority of program beneficiaries (see footnote above), so differences in their covariates must be rather extreme to affect the cell means.

Graphical analysis . Simple plots of cohort means reveal discontinuous increases in

college outcomes (a decrease in the case of school-year earnings) between 2001 and 2002, rather

than steady increases over time, lessening the concern that before-after differences simply reflect

broad underlying trends. Similar plots of covariates (not shown), including average high school

GPA and ACT scores, indicate no noticeable changes around the implementation of PROMISE.

Estimation. A regression framework enables me to adjust the raw differences for any

observable changes in sample composition. The basic OLS specification estimates:

$$(3)\ y_{it} = \alpha + \beta(after_t) + X_i \delta + v_t + \varepsilon_{it}$$

where i indexes individuals, t indexes entry cohorts, $after_t$ is an indicator variable equal to one

for the 2002 and 2003 entry cohorts and zero for earlier cohorts, X_i is a vector of individual

covariates including gender, race/ethnicity, high school GPA, high school GPA squared, and a

set of indicator variables for each ACT score.[35] Although treatment status generally varies at the

individual level, once the sample is restricted to academically qualified individuals, the treatment

varies only by year of entry. For this reason I cluster the standard errors by cohort: v_t is a cohort

error term and ε_{it} is an idiosyncratic error term. Doing so increases the standard errors for most,

though not every outcome (see Appendix B, Table B.1, for details).

Equation (3) estimates the effects of predicted PROMISE eligibility, not the effects of

actual PROMISE receipt. Because eligibility status is imperfectly measured (as discussed in the

RD section above), only 86 percent of apparently eligible enrollees in the PROMISE cohorts

actually receive PROMISE funds. An IV specification can estimate the causal effect of actual

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[35] For the 10 percent taking the SAT instead, scores are first converted to ACT scores. Given the small number
scoring 27 or higher, a single indicator variable is included for this group.

PROMISE receipt, using $after_t$ as the plausibly exogenous instrument.[36] I estimate the two-stage

model:

$$(4a)\ P_{it} = \lambda + \gamma(after_t) + X_i\phi + \eta_t + u_{it}$$

$$(4b)\ y_{it} = \alpha + \beta(\hat{P}_{it}) + X_i\delta + v_t + \varepsilon_{it}$$

where P_{it} represents actual PROMISE receipt, $\hat{P}_{it}$ represents predicted PROMISE receipt based

on the parameter estimates from (4a), and all other variables are as previously defined. As was

the case in the RD analysis, the IV results here can be interpreted as an intent-to-treat (ITT)

estimates that have been corrected for misclassification of program eligibility status. Because of

misclassification, the OLS estimates are of less interest, but are again provided for comparison.

Results. Table 3 presents the estimates from equations (3) and (4), along with baseline

means and raw differences for comparison. For most outcomes, adding controls slightly

increases the magnitude of the estimates (comparing the OLS results in column [3] to column

[2]). This suggests that at least along observable dimensions, eligible enrollees are a slightly *less*

high-achieving group after the implementation of PROMISE. The IV scales up the OLS

estimates by a factor of 1.17 (i.e., 1.00/0.86).

Receiving PROMISE appears to have only a small effect on total semesters of enrollment

(a 0.15 increase, from a baseline of 6.7 semesters over four years) and little effect on GPA. But

after the first year, recipients had earned 1.8 additional credits and 0.08 additional GPA points

(both significant at the 1 percent level). At the end of four years, the GPA effects dissipate but

recipients had earned nearly 6 more credits on average (about a 6 percent increase) and earned

about $10 per week less during the school year, slightly more than a 10% reduction. As in the

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[36] Given that no students receive PROMISE prior to 2002, one could also include interactions of covariates and "after" in the first stage. This has little effect in practice, but prevents the clean interpretation of the IV as a simple scaling up of the OLS results, so for simplicity I omit these interactions.

RD, effects at key thresholds are larger than average effects. The percentage of students who had earned at least 120 credits after four years rose by 11 percentage points (from a baseline of 43 percent) and four-year BA completion rates increase by nearly 7 percentage points (from a baseline of just 27 percent). Also as in the RD, some of the BA completion impact attenuates over time, leaving a marginally statistically significant impact of 3.7 percentage points after five years (from a baseline of 51 percent).

Robustness checks. I first test whether the cohort analysis is robust to controlling for students' high school type (public or private) and 55 indicators for county of residence at entry. The results are presented in column (2) of Table 4, and are virtually identical to the baseline estimates in column (1). Next, I explicitly control for a linear time trend in order to focus on breaks from trend at the year of implementation. This would be the preferred specification with a longer time series, but with only four cohorts it is more appropriate as a sensitivity test. These estimates, presented in column (3), increase relative to the basic specification.

I next test whether the findings from the cohort analysis are robust to the inclusion of comparison groups. First, I estimate a difference-in-difference (DD) model in which I compare the changes among PROMISE-eligible enrollees to changes among out-of-state students enrolled in West Virginia who met the academic eligibility requirements but could not receive PROMISE due to their residency status. Out-of-state enrollees comprise about one-quarter of the student body at West Virginia institutions. This is not an ideal test, because the state's largest university, WVU, substantially increased other scholarship opportunities for out-of-state students during the sample period in an explicit attempt to increase out-of-state enrollments.[37] If these other

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[37] E-mail correspondence with Brenda Thompson, Asst. Vice President for Enrollment Management at WVU, June 4, 2008. The introduction of PROMISE did not directly coincide with any of these major new initiatives, which began in 2000.

scholarships attracted higher-quality out-of-state students over time, or had impacts of their own, this biases against finding positive impacts of PROMISE.

I estimate the two-stage (IV) difference-in-difference equation:

$$(5a) \ P_{ist} = \lambda + \psi(after_t * WV_s) + \gamma(after_t) + \varphi(WV_s) + X_i\phi + \eta_t + \varepsilon_{ist}$$

$$(5b) \ y_{ist} = \alpha + \beta(\hat{P}_{ist}) + \varsigma(after_t) + \pi(WV_s) + X_i\delta + v_t + \varepsilon_{ist}$$

where WV_s is an indicator for whether the student was a West Virginia resident, P_{ist} represents actual PROMISE receipt and $\hat{P}_{ist}$ represents predicted PROMISE receipt based on the parameter estimates from (5a). The IV estimates scale up the OLS estimates here by about 18 percent.[38]

Column (4) of Table 4 presents these DD estimates. Virtually all of the point estimates shrink, and some lose significance as standard errors also increase. But the differences in point estimates between columns (4) and (1) are almost all too small to be of any substantive importance (with school-year earnings being the exception). However, if I add a linear time trend (not shown) the results are very similar to those in column (3), confirming that while non-WV residents slightly increase in quality over time, there is no jump in their performance at the time of PROMISE implementation.

Finally, I perform a separate DD analysis in which I compare the changes for all students above the threshold to changes among all those below the threshold. This test is also imperfect because students far below the threshold may not be a good comparison group for those far above it (indeed, this is the motivation for the RD analysis). In any case, these estimates (presented in column [5] of Table 4) are generally similar to (and in several cases larger than) the basic estimates, indicating that the changes for those above the threshold are *not* mirrored by similar changes for ineligible students below the threshold.

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[38] Fewer than one percent of WV non-residents receive PROMISE. This could occur if a West Virginia resident's family moved out-of-state between the student's high school graduation and college entry.

In contrast to other studies which have founded stronger effects of achievement incentives for women (for example, Kremer, Miguel, and Thornton 2009; Angrist, Lang, and Oreopoulos 2009), I find no systematic differences in the pattern of effects by gender (see appendix tables). Estimates for additional outcomes for the full sample (such as major choice, for which there is no effect) can also be found in the appendix.

Based on these robustness tests and the RD results, I conclude that the basic cohort analysis provides credible and perhaps even conservative estimates of the program's impact. From this point forward, I will focus primarily on these results.

Bounding the effects of selection bias

It is fair to ask whether the results above could be biased by differential selection, given that an explicit goal of the program was to increase in-state enrollment among qualified students.[39] Yet the potential for nonrandom selection need not make the evaluation problem intractable; Manski (1995), Lee (2009) and others suggest methods for bounding selection which influence the approach I take below. To understand how selection may bias the findings presented above, recall the before-after model as specified in equation (3). The concern is that those who enter the sample as "eligible enrollees" after the implementation of PROMISE may be different from eligible enrollees who entered the sample in earlier cohorts. Any differences captured by the covariates in X_i (including gender, race/ethnicity, age at entry, ACT score and high school GPA) can be controlled, but other differences may remain. To control for these remaining differences, one would ideally like to include in all regressions an indicator of whether the student was induced by PROMISE to become an eligible enrollee, instead estimating:

$$(6) \quad y_{it} = \tilde{\alpha} + \tilde{\beta}(after_t) + X_i\tilde{\delta} + \lambda Z_i + \tilde{\varepsilon}_{it}$$

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[39] The question of whether PROMISE increased enrollments is important in itself. It is not a main focus of my paper because of previous research on the topic, and my comparative data advantage for post-enrollment outcomes.

where Z_i is equal to 1 if the student was induced to become an eligible enrollee because of

PROMISE, and zero otherwise.[40] The coefficient λ estimates how different these marginal

enrollees are from intra-marginal enrollees, after controlling for other observable characteristics.

If at least some students are induced to become eligible enrollees because of the program,

and these students are different in unobservable ways ($\lambda>0$), then the estimated $\hat{\beta}$ from equation

(6) will not converge to the true $\tilde{\beta}$. If X_i were completely orthogonal to Z_i (i.e. if none of the

covariates were useful proxies for Z_i) then:

$$(7)\ \hat{\beta} - \tilde{\beta} \rightarrow \left[\Pr(Z_i = 1) \mid after_t = 1\right] \times \lambda$$

In words, equation (7) says that the size and magnitude of the bias will depend on two factors: 1)

what fraction of eligible enrollees who are "marginal," that is, induced to become eligible

enrollees by PROMISE, and 2) how different marginal enrollees are from intra-marginal

enrollees (as measured by the parameter λ). This is an upper bound on the potential bias; it will

be smaller to the extent that the covariates in X_i help proxy for the unobserved Z_i. In this section,

I first estimate (1) using publicly available enrollment trend data, and then test the sensitivity of

the main findings to varying assumptions about (2).

To estimate the impact of PROMISE on eligible enrollment, Figure 6 plots four different

college enrollment rates for WV high school graduates: the percent enrolling in a public WV

institution as a PROMISE-eligible student, the percent enrolling in a public WV institution as a

PROMISE-ineligible student, the percent enrolling in a WV private institution, and the percent

enrolling in an out-of-state institution.[41] The figure indicates that the percent of WV high school

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[40] This analysis follows a framework used by Jonathan Guryan in the commentary section of Bettinger (2004).
[41] Trends in public WV enrollments come from the restricted-use individual-level WVHEPC data. Trends in the number of high school graduates and aggregate data on private college enrollments were also obtained from WVHEPC. The Integrated Postsecondary Education Data System (IPEDS) provides data on the home states of first-time college freshmen by institution, but only in even-numbered years. WVHEPC collects migration data annually,

graduates enrolling in public WV institutions as PROMISE-eligible students jumped by 4 to 5 percentage points between 2001 and 2002, from a baseline of about 15 percent.[42]   This suggests that out of the 20 students eligible for the program in 2002 and later, 15 would have met the initial requirements and enrolled in a public WV institution with or without the scholarship, while 4 to 5 (20 to 25 percent) appear to be "marginal" enrollees.[43]

Figure 6 also provides some information about where these marginal enrollees came from, and where they did not. Between 2001 and 2002, the out-of-state enrollment rate declined by 1.2 percentage points. If one assumes that this entire decrease represents students switching to WV public institutions as eligible enrollees, then one-quarter to one-third of marginal enrollees were induced from out-of-state. The percentage may be much lower if some of those induced from out-of-state decided to use their PROMISE scholarship at a WV private institution (private WV enrollment does tick upward in 2002). These are the students most likely to create a positive bias, so it is reassuring that they cannot account for more than a third of the enrollment increase, or more than 6 percent (=1.2/20) of all PROMISE-eligible enrollees.

It is impossible to identify in the data precisely who these 6 percent are, but one approach to bounding, following Lee (2009), is to make the extreme assumption that these marginal students represent the top 6 percent of values for a given outcome and then re-estimate the effects with these top values excluded. But in the case where multiple, related outcomes are

but the data are based on surveys of high school administrators (who, according to WVHEPC, base their estimates largely on where students send ACT/SAT scores). In even-numbered years, the IPEDS out-of-state enrollment numbers are consistently about 75 percent the level of the WVHEPC estimates. I use the IPEDS statistics in even years and impute the out-of-state enrollments in odd years as 75 percent of the WVHEPC estimates for those years.
[42] The difference between just 2001 and 2002 is 3.7 percentage points, but including additional years increases the average before-after difference to about 5 percentage points. Given that enrollment appears to be trending upwards even before 2002, the smaller figure may be more realistic, but I will use the larger figure to calculate upper bounds of the effect of compositional change.
[43] While this clearly limits the potential for compositional change, it is still a sizable enrollment effect. This estimate is slightly higher than Cornwell, Mustard, and Sridhar (2006) find for Georgia HOPE, and comparable to Dynarski's (2002) estimate for seven state programs.

available, Lee (2009) bounds, which were designed for the case of a single outcome, can be too conservative. In the present case, it is empirically impossible for marginal students to simultaneously represent the top 6 percent of values for *every* outcome of interest. For example, the top 6 percent of PROMISE recipients by cumulative college GPA had a five-year graduation rate of only 83 percent (not 100 percent), a four-year graduation rate of 68 percent (not 100 percent), and accumulated an average of only 118 credits (which is just below the median, not the 94th percentile of credit accumulation).

Thus, instead of throwing out the top 6 percent of values for each outcome individually, I re-estimate the effects for all outcomes after "trimming" the sample based on the 94th percentile of a key outcome, here either cumulative GPA (3.90 or above) or cumulative credits earned (149 or above). The results are shown in Table 5. Column (1) restates the baseline estimates for comparison, and columns (2) and (3) provide the adjusted estimates after trimming the sample. Even under this rather extreme assumption, the coefficients shrink but generally remain above zero, and several key impacts retain significance, including the effects on first year outcomes, school-year earnings, meeting the 120 credit threshold, and earning a BA within 4 years. Interestingly, trimming based on outcomes over four years has virtually no effect on the estimated effect on first year credits, which is arguably the outcome most proximal to the policy (because most students were meeting the 2.75 GPA threshold even prior to PROMISE; and recall that 25% of recipients lost the scholarship after the first year). Note that this analysis only examines the effects of positive selection; if one made similarly extreme assumptions about negatively-selected marginal students (those who otherwise would not have enrolled at all or

would have enrolled with an ACT score below the cutoff), the net effect of selection may way be a downward rather than upward bias.[44]

IV. Inside the Black Box: Are Impacts Driven by Cost-of-College or Incentive Effects?

Are Impacts Concentrated Around Annual Renewal Thresholds?

If PROMISE were a traditional grant with no strings attached, there would be no reason to expect the impacts on course credits or GPAs to be concentrated around the annual GPA and credit thresholds for renewal. Yet this is precisely what is observed, at least in the case of course credits. Figure 7 shows the cumulative distribution function (CDF) of credits attempted in each year of college, by entry cohort. For the two pre-cohorts, the CDFs are basically smooth. For the two PROMISE cohorts in the first three years of college, the CDFs shift to the right and a clear kink is visible just below the renewal threshold of 30 credits.[45] The kink demonstrates the shift from below 30 to just above 30 credits.

Figure 8 presents CDFs for college GPAs. GPAs are clearly higher for the PROMISE cohorts in the first three years.[46] There are no clear kinks in the GPA distributions around the annual renewal thresholds, but the distributions appear slightly bowed with the largest before-after differences found near the thresholds. The absence of clear kinks is not surprising given that students cannot manipulate their GPAs as precisely as their course loads.

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[44] One can perform a similar analysis using the RD estimates, to arrive at a similar conclusion. For the sake of brevity, I focus on bounding the cohort analysis estimates since they provide the more interesting policy parameter (average effects versus estimates local to the ACT threshold).

[45] The CDFs express the probability that the value of the outcome is less than or equal to X. Thus, the kink in the CDFs of credits attempted at 29 credits indicates that the greatest impact is on the probability of completing 29 or fewer credits (or one minus the probability of completing 30 or more), which corresponds to the 30-credit renewal threshold.

[46] Indeed, if effects had been measured at the end of three years there would be a significant increase in cumulative GPAs of 0.06 points, using the cohort analysis specification.

Tellingly, these patterns disappear in the fourth (senior) year, when students still receive PROMISE funds but no longer face specific incentives regarding course load or GPA, because the scholarship cannot be renewed for a fifth year. The distribution of credits remains slightly shifted to the right, but there is no longer a kink at the threshold. The GPA distribution among PROMISE cohorts becomes virtually indistinguishable from that of the pre-cohorts, with the PROMISE cohorts perhaps even falling slightly behind.[47] The change in pattern is not due to a dropoff in the number of PROMISE recipients: nearly 85 percent of those who received PROMISE in their third year also received it in their fourth.

In Table 6, I quantify the differences shown in these figures. I estimate impacts on the percentage meeting the renewal thresholds in each year, using the cohort analysis OLS specification as well as an IV approach to account for declining PROMISE receipt in each year of college.[48] The results show that PROMISE recipients are *20 to 25 percentage points more likely* to complete 30 or more credits in each of the first three years, but the impact is only 8 percentage points in the fourth year. Similarly, PROMISE recipients are 6 to 8 percentage points more likely to exceed the cumulative GPA thresholds in each of the first three years, but in the senior year the impact on (annual) GPA disappears completely.

One alternative explanation is that students in the fourth year of college do not need to take 30 credits because they are closer than that to graduation. This could account for some of the dropoff between junior and senior year impacts. But even among students who received PROMISE for all four years, only 60 percent graduated in four years, and only one in five

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[47] I examine annual GPA rather than cumulative GPA in the fourth year because the cumulative GPA is mostly pre-determined by actions in years 1-3. The annual GPA thus represents a cleaner test of students' responses to the removal of the incentives. A CDF of the fourth-year cumulative GPAs looks like a more muted version of the CDF of third-year cumulative GPAs.

[48] For the IV approach, "after" is used as an instrument for PROMISE receipt in the freshman, sophomore, junior, and senior years, respectively.

graduated in four years without taking at least 30 credits. It thus seems unlikely that senior year

course loads are much limited by the prospect of imminent graduation. This explanation also

cannot explain the falloff in fourth year grades.

Are impacts limited to students with high financial need?

Another way to test whether the effects are driven by income or incentive effects is to

examine subgroups with differing levels of financial constraint. The behavioral incentives of

PROMISE should apply similarly to rich and poor students. Richer students may, however, be

less financially constrained and thus less sensitive to reductions in the cost of college.[49] Because

family income data are not available, I use a binary indicator of federal Pell Grant eligibility as a

rough proxy for financial need.[50] Both before and after the introduction of PROMISE, about 31

percent of eligible enrollees received Pell Grants, which generally go to students with family

incomes of $40,000 or less.

Estimating the preferred specification (the basic cohort analysis IV) by Pell grant status, I

find that the impacts are very similar between the two groups and if anything are somewhat

smaller for Pell recipients for some outcomes (see Appendix Table B.6). It should be noted that

this comparison is not definitive: even Pell non-recipients could be financially constrained

without PROMISE, and some Pell recipients may remain significantly constrained even with

PROMISE. Still, the finding that impacts are not concentrated among the neediest students is

suggestive that cost-of-college effects are not the sole mechanism driving the results.

Do programs of similar value but with different incentives generate different effects?

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[49] Note that the two primary sources of need-based aid for WV college students—Pell grants and WV Higher Education Grants—are generally unaffected by PROMISE receipt, so rich and poor students have equal amounts of funding staked on the achievement incentives (see Appendix A for relevant program rules).
[50] Pell Grant eligibility is not a perfect measure either. Although PROMISE does not directly affect Pell Grant eligibility, PROMISE requires students to apply for federal aid and thus may increase Pell Grant take-up. It is reassuring that the rate of Pell receipt remains stable before and after the introduction of PROMISE (see Table 2).

If large financial incentives for college achievement work primarily by lowering the cost

of college rather than by increasing the rewards for student effort, then programs of similar value

should have similar effects on enrollees even if the incentives are slightly different. The Georgia

HOPE program provides a particularly instructive comparison. Georgia's HOPE scholarship was

the early model for many subsequent state programs, including PROMISE. The two programs

are of similar monetary value (both cover tuition and fees), and both require students to maintain

a 3.0 GPA while in college (although PROMISE allows a 2.75 GPA in the first year). But in

Georgia there are no minimum course load requirements for renewal; students need not even

attend full-time.

While PROMISE accelerates students' course progression, HOPE apparently had the

opposite effect. Cornwell, Lee and Mustard (2005) find that HOPE recipients at Georgia's

flagship university were 9.3 percentage points less likely to complete a full-time course load in

their freshman year. Given the similar value of the scholarships, this is dramatically different

from PROMISE's 25 percentage point increase the in the proportion of students completing a 30-

credit course load in the first year. The difference suggests that students respond strategically to

each program's incentives: Georgia's rules encouraged students to reduce course loads in order

to raise their GPAs; West Virginia's 30-credit renewal requirement effectively eliminates this

strategy for "gaming" the system.[51]

While HOPE may have slowed time-to-degree, Dynarski (2008) estimates that it (along

with a similar program in Arkansas) increased the eventual BA completion rate among enrollees

by 3 to 5 percentage points, which is comparable to the 3.7 percentage point impact on five-year

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[51] The 30-credit requirement may also reduce another form of gaming that was a concern under Georgia HOPE: students might switch out of science and math courses in favor of more leniently-grading subjects. Since many science courses are worth four credits instead of three, PROMISE does not provide a clear incentive to substitute out of science. I find that PROMISE had no impact on the proportion of students choosing to major in science or math at entry.

graduation rates under PROMISE. It is thus possible that specific achievement incentives matter more for *how* students complete college rather than *whether* they complete college. These results are also consistent with several other findings from the literature: an evaluation of the Gates Millenium Scholars program, which provides large financial benefits but minimal academic requirements, finds limited effects on academic outcomes (DesJardins and McCall 2008; note, because the recipient population was academically elite, effects may simply have been limited by outcome ceilings). Similarly, Stinebrickner and Stinebrickner (2008) find that alleviating financial constraints alone does little to reduce dropout rates at a small private college in Kentucky. Finally, Garibaldi et al. (2007) find that on-time completion rates at one Italian institution rose after a policy was implemented to charge students more for enrolling beyond the expected completion time, although such a tuition increase obviously does nothing to alleviate financial constraints.

VI. Discussion

I find that PROMISE has a significant impact on many end-of-college outcomes, with particularly large impacts on time-to-degree. Despite the potential for selection bias, a bounding exercise shows that the estimated impacts on several important outcomes would remain significantly above zero even in the presence of very extreme assumptions about marginal enrollees. Overall, including the estimated effects on initial enrollment, PROMISE increased the overall BA attainment rate (BA completers as a proportion of all individuals in an age cohort) by 1.8 to 2.3 percentage points depending on whether the five-year BA impact persists, which may seem modest in absolute terms, but comes on top of a baseline BA attainment rate in West

Virginia of just 21.5 percent.[52, 53] A full cost-benefit analysis is premature as graduates have just

begun to enter the labor market. But even if the five-year BA completion impact fades out,

PROMISE easily passes a social cost-benefit analysis under reasonable assumptions about the

returns to completed schooling, with up to 25 percent of net social benefits due to the

improvements in time-to-degree (see appendix for details of this analysis). In simplistic terms,

PROMISE cost about $63 million and produced about 1000 additional graduates over its first

two cohorts, with approximately three-quarters of these graduates remaining in-state for at least 6

months after graduating (longer follow-up data not yet available).

An analysis of the mechanisms behind PROMISE's impact makes clear that incentives

matter, and the details of incentive design can have big consequences. PROMISE likely would

not have had the same impact, particularly on time-to-degree, had it been designed as a

traditional grant with no strings attached or different strings attached. This study also exposes an

important (if obvious) explanation for delayed graduation: many students simply are not taking

enough course credits each semester, beginning in the freshman year.

While the cost-of-college effects of PROMISE are insufficient to explain its impact, this

hardly implies that the value of the award is irrelevant. It is important to note that cost-of-

college effects of the scholarship may matter most for dimensions of behavior beyond those

covered in the present analysis. Although I focus primarily on effects among college enrollees, I

also find evidence that PROMISE increased the percent of high school graduates who enroll in

West Virginia in the first place. The initial enrollment decision may be more sensitive to the

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[52] This assumes a stable high school graduation rate of 82 percent, a 4 percentage point impact on eligible
enrollment among high school graduates, and a graduation impact of 3.7 percentage points among eligible enrollees.
[53] Baseline BA attainment rate is from ACS 2005, based on WV residents aged 25-34, corresponding to the age
cohorts just prior to PROMISE.

cost-of-college effects of PROMISE. The scholarship also reduced student loan debt (see appendix), which could affect post-graduation decisions (Field 2009; Rothstein and Rouse 2008).

Of course, financial aid policy is not the only means of affecting collegiate attainment, and even with large incentives, many students still fail to graduate or fail to graduate on time. Still, the findings here suggest that incentives tied to minimum course loads (not just GPAs) may be one of several promising tools for increasing educational attainment and speeding time-to-degree.

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Table 1
RD Estimates of the Effect of West Virginia's PROMISE Scholarship

Outcome	(1) Means Among ACT=20	(2) Sharp RD, LLR 16<=ACT<=25 B	(SE)	(3) Fuzzy RD, LLR 16<=ACT<=25 B	(SE)
Received PROMISE	0.067	0.700 ***	(0.012)	**1.000 ***	(0.000)**
Value of PROMISE in Year 1	$172	$2,108 ***	($37)	**$3,012 ***	($21)**
Total PROMISE received (over 4 years)	$507	$5,835 ***	($158)	**$8,338 ***	($180)**
GPA, end of year 1	2.523	0.109 ***	(0.036)	**0.156 ***	(0.051)**
Credits earned, end of year 1	24.529	1.466 ***	(0.325)	**2.095 ***	(0.461)**
Number of semesters enrolled (over 4 years)	6.354	0.026	(0.091)	**0.037	(0.130)**
Total credits earned (over 4 years)	86.345	3.250 *	(1.769)	**4.644 *	(2.519)**
Cumulative GPA (over 4 years) [a]	2.675	0.069 **	(0.031)	**0.099 **	(0.045)**
Typical weekly school-year earnings [b]	$101.77	-$1.48	($4.93)	**-$2.12	($7.04)**
Earned 120 credits by end of Year 4	0.310	0.067 ***	(0.018)	**0.095 ***	(0.026)**
Had 3.0+ cumulative GPA at end of Year 4	0.401	0.063 ***	(0.019)	**0.090 ***	(0.027)**
Earned BA within 4 Years	0.155	0.066 ***	(0.015)	**0.094 ***	(0.022)**
Earned BA within 5 Years	0.367	0.032	(0.019)	**0.045 *	(0.028)**
Covariates					
High school GPA	3.457	0.025 **	(0.011)	**0.034 **	(0.015)**
Percent female	0.626	-0.006	(0.020)	**-0.008	(0.028)**
Age at entry (years)	18.6	-0.01	(0.02)	**-0.02	(0.02)**
Average Pell Grant (year of entry)	$1,097	-$44	($62)	**-$61	($86)**
Sample size	972	8,567		**8,567**	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger who were West Virginia residents, entered in 2002-03 or 2003-04, and met the high school GPA requirement for PROMISE (3.0+).

NOTES: Robust standard errors in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, as well as a quadratic function of high school GPA. "LLR" indicates a local linear specification is used. Stars indicate the significance of individual findings at the p<0.10, p<0.05, or p<0.01 level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

Table 2
RD Robustness Checks

Outcome	(1) Baseline: Fuzzy RD, LLR 16<=ACT<=25 B	(SE)	(2) Add controls for HSType/CountyFE B	(SE)	(3) 18<=ACT<=23 B	(SE)	(4) 11<=ACT<=30 B	(SE)	(5) Local Quadratic Regression B	(SE)	(6) Falsification Exercise: Sharp RD Before 2002 B	(SE)
Received PROMISE	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	n/a	
Value of PROMISE in Year 1	$3,012 ***	($21)	$3,004 ***	($20)	$3,029 ***	($27)	$2,979 ***	($16)	$3,038 ***	($34)	n/a	
Total PROMISE received (over 4 years)	$8,338 ***	($180)	$8,293 ***	($180)	$8,539 ***	($235)	$8,211 ***	($139)	$8,630 ***	($293)	n/a	
GPA, end of year 1	0.156 ***	(0.051)	0.156 ***	(0.051)	0.210 ***	(0.064)	0.073 *	(0.043)	0.221 ***	(0.081)	-0.033	(0.036)
Credits earned, end of year 1	2.095 ***	(0.461)	2.121 ***	(0.460)	2.609 ***	(0.582)	1.455 ***	(0.383)	2.751 ***	(0.736)	-0.359	(0.330)
Number of semesters enrolled (over 4 years)	0.037	(0.130)	0.003	(0.130)	0.066	(0.165)	-0.051	(0.108)	0.059	(0.208)	-0.130	(0.094)
Total credits earned (over 4 years)	4.644 *	(2.519)	4.331 *	(2.518)	3.842	(3.251)	2.193	(2.069)	2.829	(4.085)	-1.027	(1.799)
Cumulative GPA (over 4 years) [a]	0.099 **	(0.045)	0.091 **	(0.045)	0.105 *	(0.057)	0.024	(0.038)	0.122 *	(0.072)	-0.008	(0.032)
Typical weekly school-year earnings [b]	-$2.12	($7.04)	-$1.58	($7.08)	$4.16	($9.01)	-$0.58	($5.91)	$1.40	($11.44)	-$6.06	($4.97)
Earned 120 credits by end of Year 4	0.095 ***	(0.026)	0.093 ***	(0.026)	0.087 **	(0.034)	0.109 ***	(0.021)	0.072 *	(0.043)	-0.003	(0.019)
Had 3.0+ cumulative GPA at end of Year 4	0.090 ***	(0.027)	0.084 ***	(0.027)	0.080 **	(0.035)	0.082 ***	(0.022)	0.090 **	(0.044)	0.023	(0.020)
Earned BA within 4 Years	0.094 ***	(0.022)	0.094 ***	(0.022)	0.100 ***	(0.029)	0.078 ***	(0.017)	0.096 ***	(0.036)	0.011	(0.015)
Earned BA within 5 Years	0.045 *	(0.028)	0.043	(0.028)	0.038	(0.036)	0.040 *	(0.022)	0.048	(0.045)	0.000	(0.020)
Sample size	8,567		8,567		6,086		10,719		8,567		7,826	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger who were West Virginia residents, entered in 2002-03 or 2003-04, and met the high school GPA requirement for PROMISE (3.0+).

NOTES: Robust standard errors are in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, as well as a quadratic function of high school GPA. Column (2) includes an indicator for private high school graduates and an indicator for those whose high school public/private status was missing, as well as a set of indicators for each WV county of residence. Except where otherwise noted, regressions use a fuzzy RD, local linear regression for students with ACT scores of 16 to 25. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

Table 3
Cohort Analysis Estimates of the Effect of PROMISE

Outcome	(1) Pre- Mean	(2) After- Before	Before-After with Controls			
			(3) OLS		**(4) IV**	
			B	(SE)	**B**	**(SE)**
Received PROMISE	0.000	0.852	0.859 ***	(0.009)	**1.000 ***	(0.000)**
Value of PROMISE in Year 1	$0	$2,621	$2,643 ***	($131)	**$3,077 ***	($120)**
Total PROMISE received (over 4 years)	$0	$8,598	$8,677 ***	($310)	**$10,101 ***	($255)**
GPA, end of year 1	2.849	0.062	0.066 ***	(0.005)	**0.077 ***	(0.006)**
Credits earned, end of year 1	26.239	1.529	1.572 ***	(0.085)	**1.830 ***	(0.118)**
Number of semesters enrolled (over 4 years)	6.731	0.118	0.126 **	(0.037)	**0.146 **	(0.045)**
Total credits earned (over 4 years)	97.225	4.668	4.967 **	(0.940)	**5.782 **	(1.136)**
Cumulative GPA (over 4 years) [a]	2.982	0.025	0.033	(0.015)	**0.039	(0.018)**
Typical weekly school-year earnings [b]	$85.51	-$7.40	-$8.20 **	($1.76)	**-$9.55 **	($2.10)**
Earned 120 credits by end of Year 4	0.431	0.091	0.095 ***	(0.015)	**0.111 ***	(0.018)**
Had 3.0+ cumulative GPA at end of Year 4	0.590	0.025	0.030 *	(0.010)	**0.035 *	(0.012)**
Earned BA within 4 Years	0.267	0.054	0.058 ***	(0.004)	**0.067 ***	(0.005)**
Earned BA within 5 Years	0.509	0.029	0.031 *	(0.010)	**0.037 *	(0.012)**
Sample size			12,911		12,911	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. The sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Robust standard errors, clustered by cohort, are in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

Table 4
Cohort Analysis Robustness Checks

Outcome	(1) IV Before/After, All WV Eligibles		(2) Add controls for HSType/CountyFE		(3) Linear Time Trend		IV Diff-in-diffs (4) Out of state		(5) Below threshold	
	B	(SE)	B	(SE)	B	(SE)	B	(SE)	B	(SE)
Received PROMISE	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)
Value of PROMISE in Year 1	$3,077 ***	($120)	$3,076 ***	($119)	$2,742 ***	($200)	$3,076 ***	($120)	$3,092 ***	($119)
Total PROMISE received (over 4 years)	$10,101 ***	($255)	$10,089 ***	($259)	$9,381 ***	($403)	$10,084 ***	($269)	$10,156 ***	($268)
GPA, end of year 1	0.077 ***	(0.006)	0.079 ***	(0.005)	0.057 ***	(0.002)	0.053	(0.026)	0.093	(0.049)
Credits earned, end of year 1	1.830 ***	(0.118)	1.796 ***	(0.107)	2.136 ***	(0.186)	1.419 *	(0.481)	1.786 ***	(0.296)
Number of semesters enrolled (over 4 years)	0.146 **	(0.045)	0.138 **	(0.040)	0.296 ***	(0.001)	0.102	(0.118)	0.301 ***	(0.047)
Total credits earned (over 4 years)	5.782 **	(1.136)	5.544 **	(1.062)	9.511 ***	(0.457)	4.045	(2.817)	9.460 ***	(1.269)
Cumulative GPA (over 4 years) [a]	0.039	(0.018)	0.037	(0.017)	0.101 ***	(0.014)	0.019	(0.037)	0.040	(0.043)
Typical weekly school-year earnings [b]	-$9.55 **	($2.10)	-$9.51 **	($2.08)	-$15.48 ***	($2.13)	-$4.47 *	($1.47)	-$15.78 **	($3.64)
Earned 120 credits by end of Year 4	0.111 ***	(0.018)	0.108 ***	(0.017)	0.158 ***	(0.023)	0.088 **	(0.019)	0.119 ***	(0.016)
Had 3.0+ cumulative GPA at end of Year 4	0.035 *	(0.012)	0.034 *	(0.011)	0.072 ***	(0.009)	0.018	(0.013)	0.019	(0.022)
Earned BA within 4 Years	0.067 ***	(0.005)	0.066 ***	(0.005)	0.076 ***	(0.008)	0.044 **	(0.014)	0.066 ***	(0.002)
Earned BA within 5 Years	0.037 *	(0.012)	0.034 *	(0.012)	0.069 **	(0.017)	0.019	(0.028)	0.056 **	(0.013)
Sample size	12,911		12,911		12,911		16,645		20,849	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. Unless otherwise noted, the sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Robust standard errors, clustered by cohort, are in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Column (2) includes an indicator for private high school graduates and an indicator for those whose high school public/private status was missing, as well as a set of indicators for each WV county of residence. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

Table 5
Bounding Exercise

Outcome	(1) IV Before/After, All WV Eligibles		(2) Trim Top 6% (After) Based on Cumulative GPA		(3) Trim Top 6% (After) Based on Cum. Credits	
	B	(SE)	B	(SE)	B	(SE)
Received PROMISE	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)
Value of PROMISE in Year 1	$3,077 ***	($120)	$3,076 ***	($120)	$3,068 ***	($120)
Total PROMISE received (over 4 years)	$10,101 ***	($255)	$9,939 ***	($261)	$9,848 ***	($257)
GPA, end of first year	0.077 ***	(0.006)	0.038 **	(0.007)	0.054 ***	(0.004)
Credits earned, end of first year	1.830 ***	(0.118)	1.740 ***	(0.111)	1.578 ***	(0.114)
Number of semesters enrolled (over 4 years)	0.146 **	(0.045)	0.142 *	(0.045)	0.087	(0.046)
Total credits earned (over 4 years)	5.782 **	(1.136)	5.540 **	(1.174)	2.108	(1.135)
Cumulative GPA (over 4 years) [a]	0.039	(0.018)	-0.011	(0.016)	0.012	(0.020)
Typical weekly school-year earnings [b]	-$9.55 **	($2.10)	-$8.66 **	($2.23)	-$7.52 **	($2.16)
Earned 120 credits by end of Year 4	0.111 ***	(0.018)	0.104 ***	(0.018)	0.082 **	(0.018)
Had 3.0+ cumulative GPA at end of Year 4	0.035 *	(0.012)	0.020	(0.012)	0.019	(0.013)
Earned BA within 4 Years	0.067 ***	(0.005)	0.049 ***	(0.005)	0.045 ***	(0.005)
Earned BA within 5 Years	0.037 *	(0.012)	0.024	(0.012)	0.012	(0.012)
Sample size	12,911		12,464		12,462	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. Unless otherwise noted, the sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Robust standard errors, clustered by cohort, are in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Column (2) includes an indicator for private high school graduates and an indicator for those whose high school public/private status was missing, as well as a set of indicators for each WV county of residence. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

The proportion of the sample that is trimmed, 6 percent, is calculated based on an analysis of the enrollment shifts displayed in Figure 6. I then identify the set of students in the "after" cohorts with the top 6 percent of values on either cumulative GPA (equivalent to a 3.90 or above) after four years or cumulative credits earned after four years (149 credits or above), respectively, and reestimate the effects with these students excluded.

Table 6

**OLS and IV Cohort Analysis Estimates of the Effect of West Virginia's PROMISE Scholarship
on Selected Outcomes (Using "After" as Instrument for PROMISE Receipt in Each Year)**

Outcome	Pre-Mean	Basic OLS B	(SE)	IV: Freshman Year B	(SE)	IV: Soph. Year B	(SE)	IV: Junior Year B	(SE)	IV: Senior Year B	(SE)
Received PROMISE: Year 1	0.000	0.859 ***	(0.009)	1.000 ***	(0.000)						
Year 2	0.000	0.652 ***	(0.005)			1.000 ***	(0.000)				
Year 3	0.000	0.520 ***	(0.001)					1.000 ***	(0.000)		
Year 4	0.000	0.440 ***	(0.009)							1.000 ***	(0.000)
Completed at least 30 credits in: Year 1	0.409	0.210 ***	(0.003)	0.245 ***	(0.004)						
Year 2	0.383	0.139 ***	(0.006)			0.213 ***	(0.010)				
Year 3	0.360	0.109 ***	(0.007)					0.209 ***	(0.013)		
Year 4	0.296	0.035 *	(0.012)							0.080 *	(0.027)
Cumulative 2.75+ GPA, end of Year 1 (a)	0.678	0.051 ***	(0.007)	0.059 ***	(0.008)						
Cumulative 3.0+ GPA, end of Year 2	0.572	0.050 ***	(0.006)			0.077 ***	(0.010)				
Cumulative 3.0+ GPA, end of Year 3	0.578	0.044 **	(0.008)					0.084 **	(0.015)		
Cumulative 3.0+ GPA, end of Year 4	0.590	0.030 *	(0.010)							0.067 *	(0.023)
Annual 2.75+ GPA, Year 1 (b)	0.633	0.044 ***	(0.004)	0.052 ***	(0.005)						
Annual 3.0+ GPA, Year 2	0.537	0.049 *	(0.018)			0.075 *	(0.028)				
Annual 3.0+ GPA, Year 3	0.562	0.032 **	(0.008)					0.061 **	(0.016)		
Annual 3.0+ GPA, Year 4 (a)	0.599	-0.008	(0.009)							-0.018	(0.021)
Sample size		12,911		12,911		12,911		12,911		12,911	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. Unless otherwise noted, the sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Robust standard errors, clustered by cohort, are in parentheses. All regressions use the basic before-after specification and include controls for gender, race/ethnicity, age, high school GPA and ACT score (or equivalent). (a) I present both cumulative and annual GPAs. PROMISE renewal is contingent upon cumulative GPAs in the first three years; but because the cumulative GPA in Year 4 is mostly determined by behavior prior to Year 4, it does not reveal behavioral changes as clearly as the annual GPA measure. (b) In Year 1, the cumulative and annual GPA measures are not identical because of slight differences in how certain courses (such as transfer and/or remedial courses) are counted. For students not enrolled in a given year, annual GPA is first imputed as the semester GPA if the student enrolled for at least one semester, otherwise it is imputed as the cumulative GPA as of last enrollment (71 percent of the sample enrolled for at least part of year 4; 68 percent enrolled full-time for the full year; 62 percent enrolled full-time for all four years).

Figure 1. Actual PROMISE receipt by ACT score.

Actual PROMISE Receipt
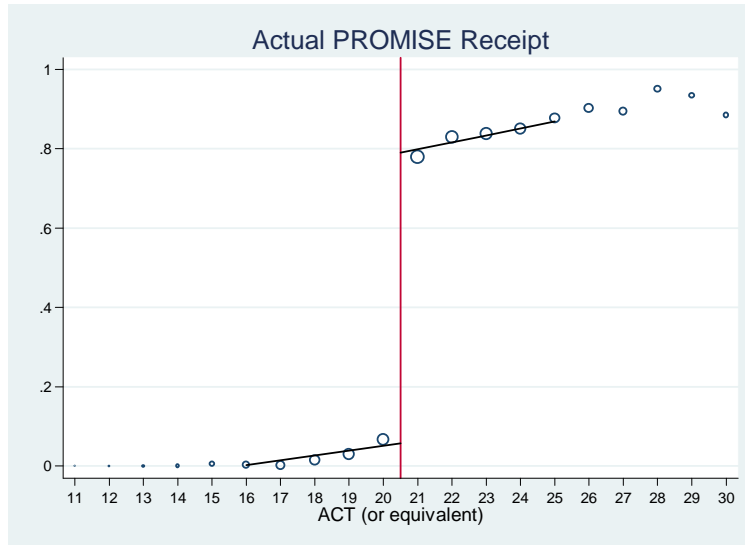
SOURCE: Author's calculations using WVHEPC data on West Virginia resident first-time freshmen entering two- or four-year public WV institutions in fall 2002 or fall 2003, who had at least a 3.0 high school GPA.

NOTES: Each dot indicates the rate of PROMISE receipt for students with a given ACT score, with the size of the dots reflecting the distribution of students across ACT scores. The line represents a linear prediction, allowed to vary on either side of the threshold, based on the cell-size weighted group means for students with scores between 16 and 25. The vertical red line indicates the threshold for PROMISE eligibility.

Figure 2

Selected Outcomes by ACT Score (1)

Num. of sems. enrolled over 4 yrs

Typical school-year wkly earnings

Total credits earned after 4 years

Cumulative GPA after 4 years

Figure 3

Selected Outcomes by ACT Score (2)

Earned 120+ credits within 4 yrs

Had 3.0+ cumulative GPA after 4 yrs

Earned a BA Within 4 Years

Earned a BA Within 5 Years

Figure 4

Density of ACT Scores, Before and After PROMISE
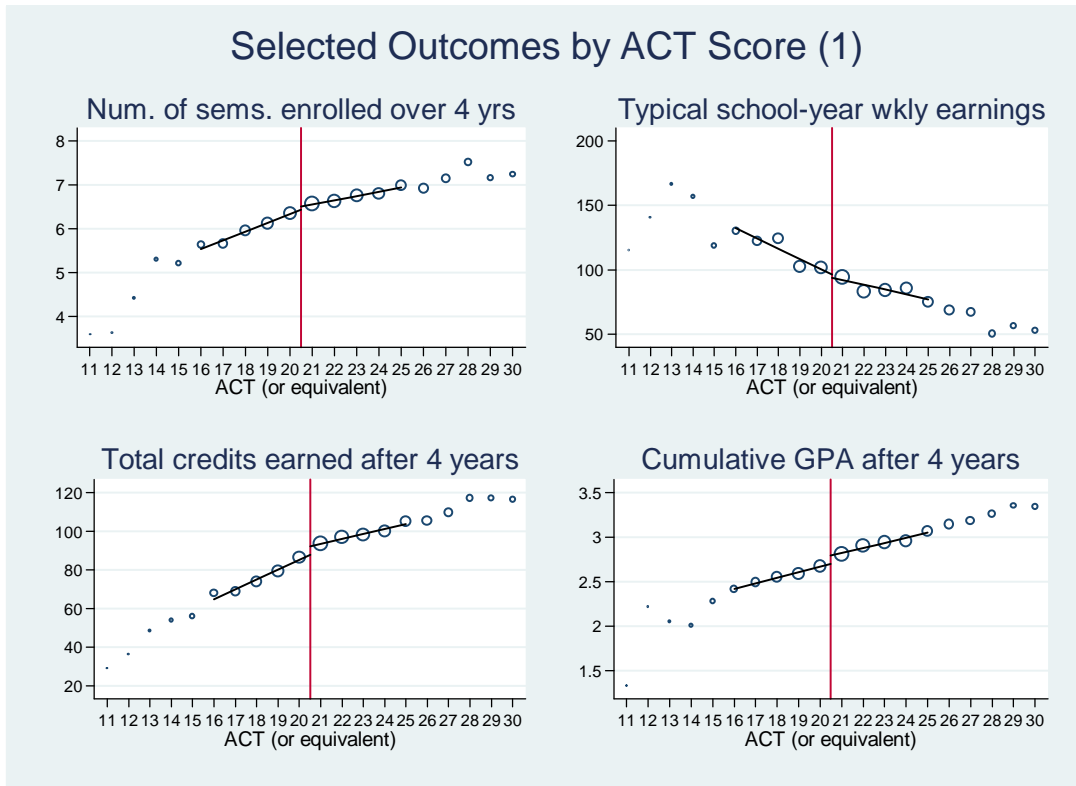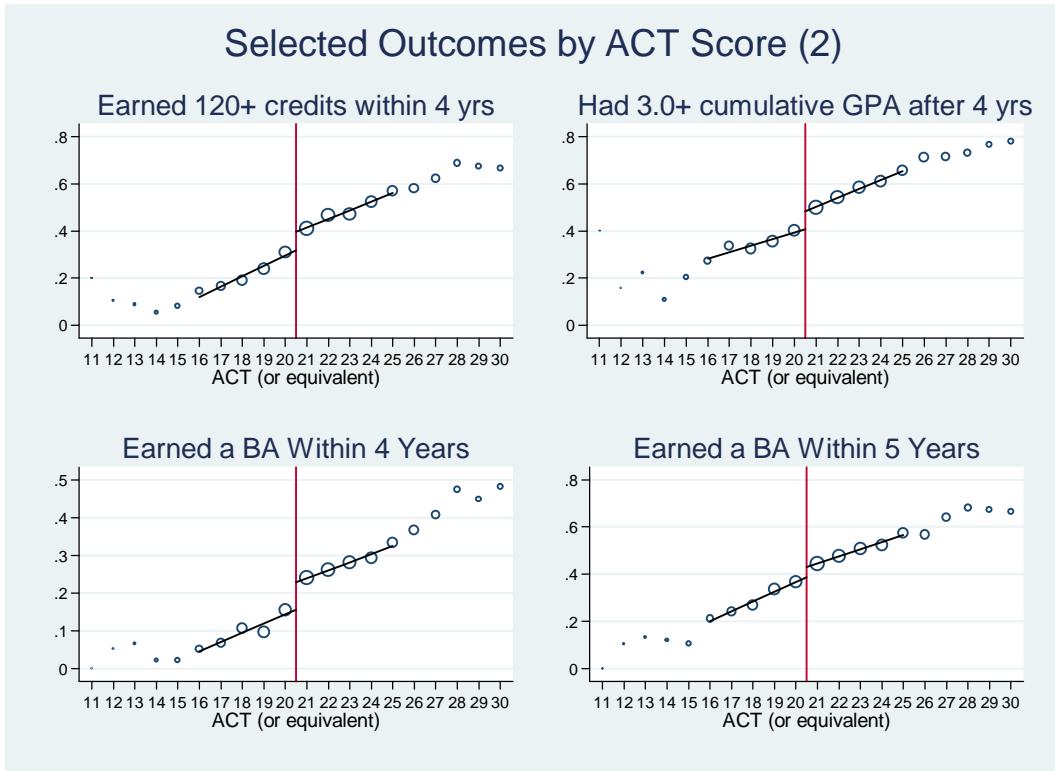
Figure 5

Selected Outcomes By Cohort

Num. of Sems. Enr. Over 4 Yrs.

Typical School-Yr Wkly Earnings

Total Credits Earned After 4 Yrs.

Cumulative GPA After 4 Yrs.

Completed 120+ Credits by Yr. 4

Had 3.0+ GPA at End of Yr. 4

Earned a BA within 4 Years

Earned a BA within 5 Years

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking West Virginia residents aged 19 and younger, who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.
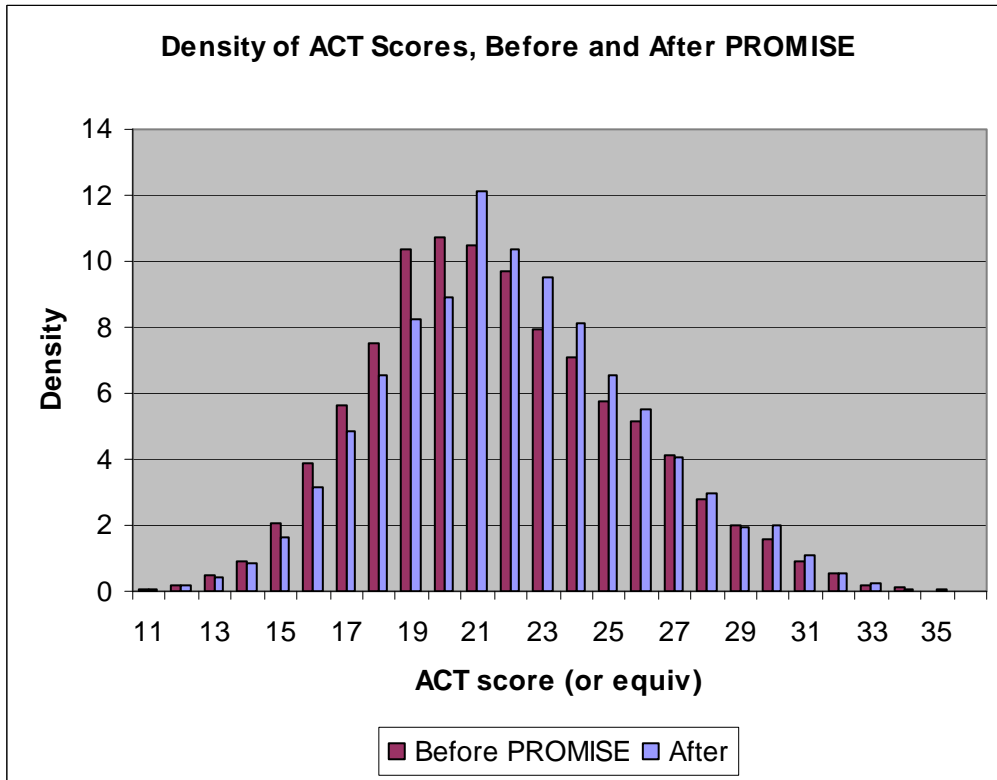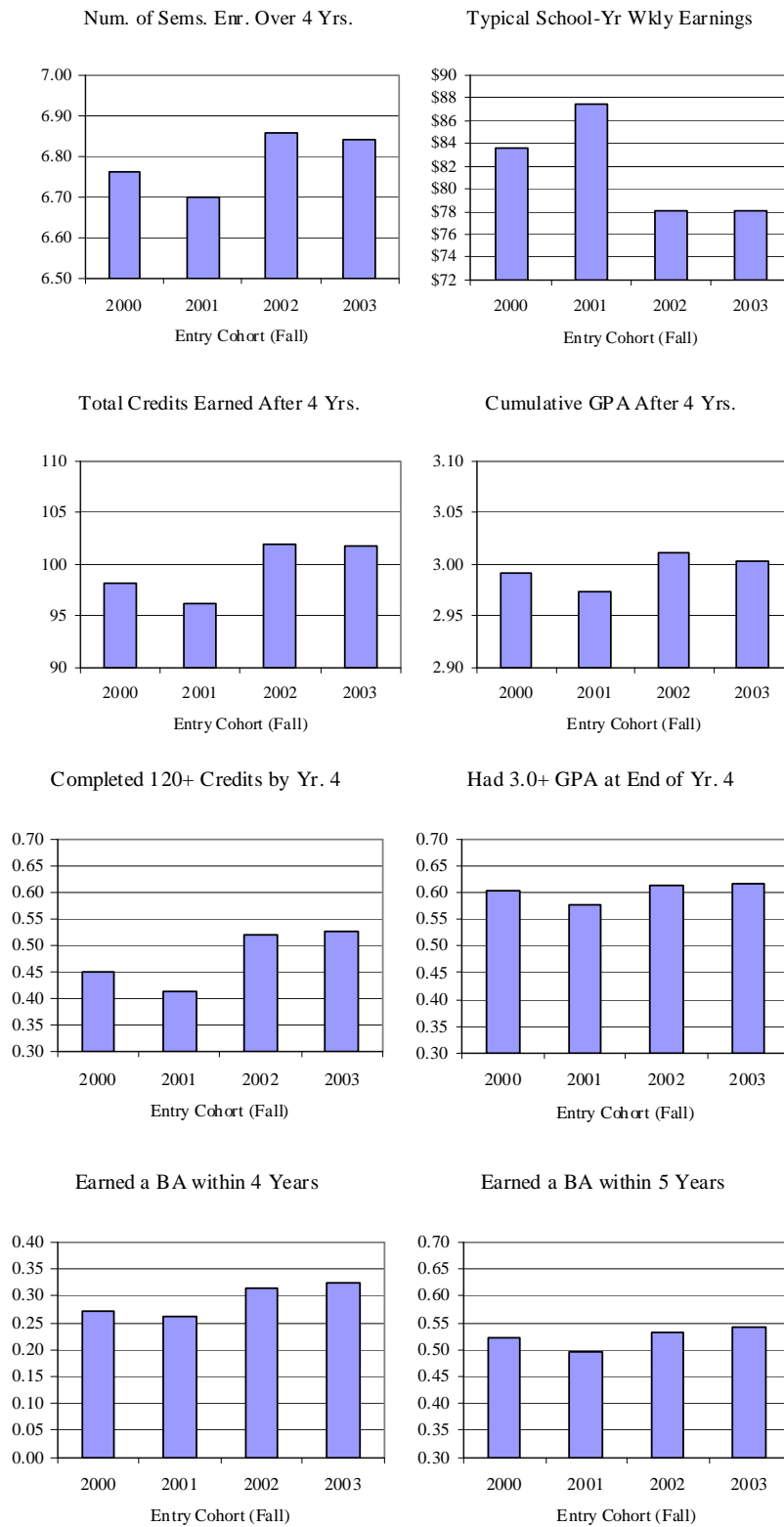
NOTES: Bars indicate unadjusted means by cohort. Typical weekly earnings are based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment. For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled.

Figure 6

Fall Enrollment Rates of Recent WV HS Grads
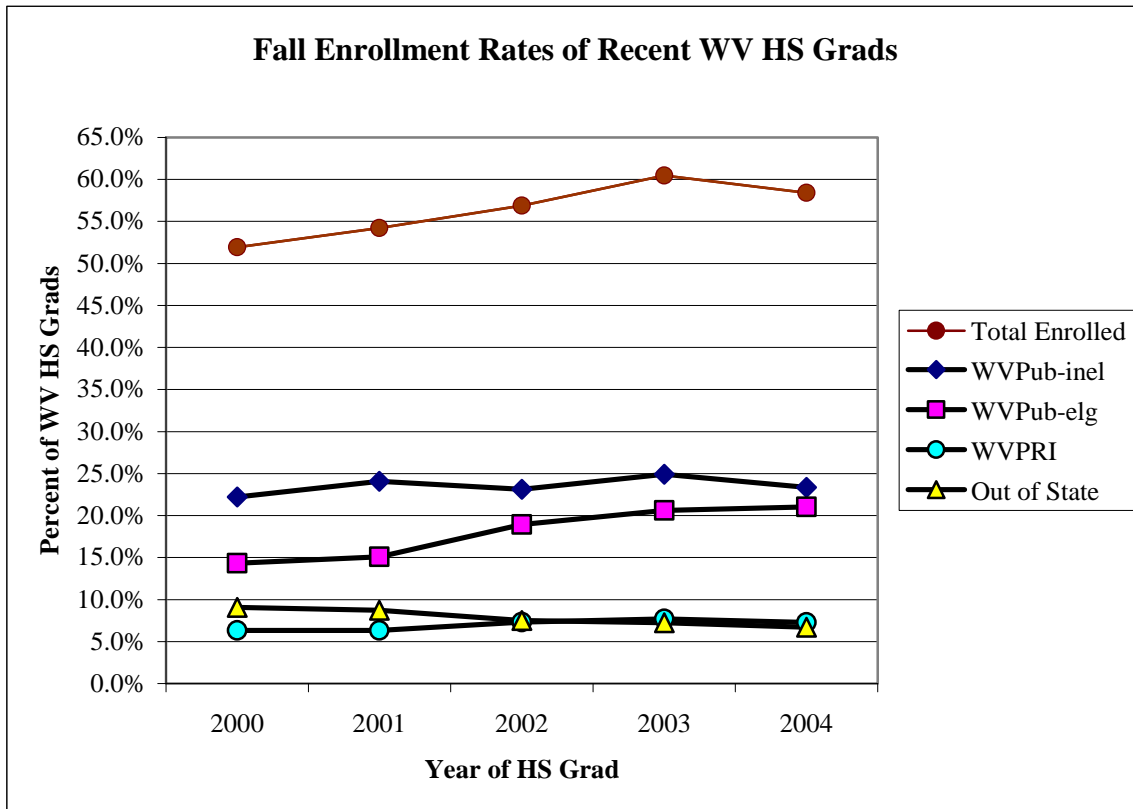
SOURCE: Author's calculations using the following data sources: HSGRADS - WV Department of Education, as reported in annual WVHEPC College Going Rate reports. WVPUB - Author's calculations using WVHEPC administrative data on enrollments. Includes first-time freshmen seeking a 2/4-yr degree who graduated from a WV high school in the past 12 months. WVPRI - Annual institutional survey conducted by WVHEPC, as reported in annual WVHEPC College Going Rate reports. Out of state - IPEDS Residence and Migration reports, and WVHEPC surveys of high school administrators as reported in annual College Going Rate reports.

Figure 7

CDFs of Credits Completed Each Year, By Cohort

SOURCE: Author's calculations using WVHEPC administrative data on 12,911 first-time freshmen age 19 and younger, who met PROMISE eligibility requirements.

Figure 8

CDFs of GPA Each Year, By Cohort

***APPENDIX ***

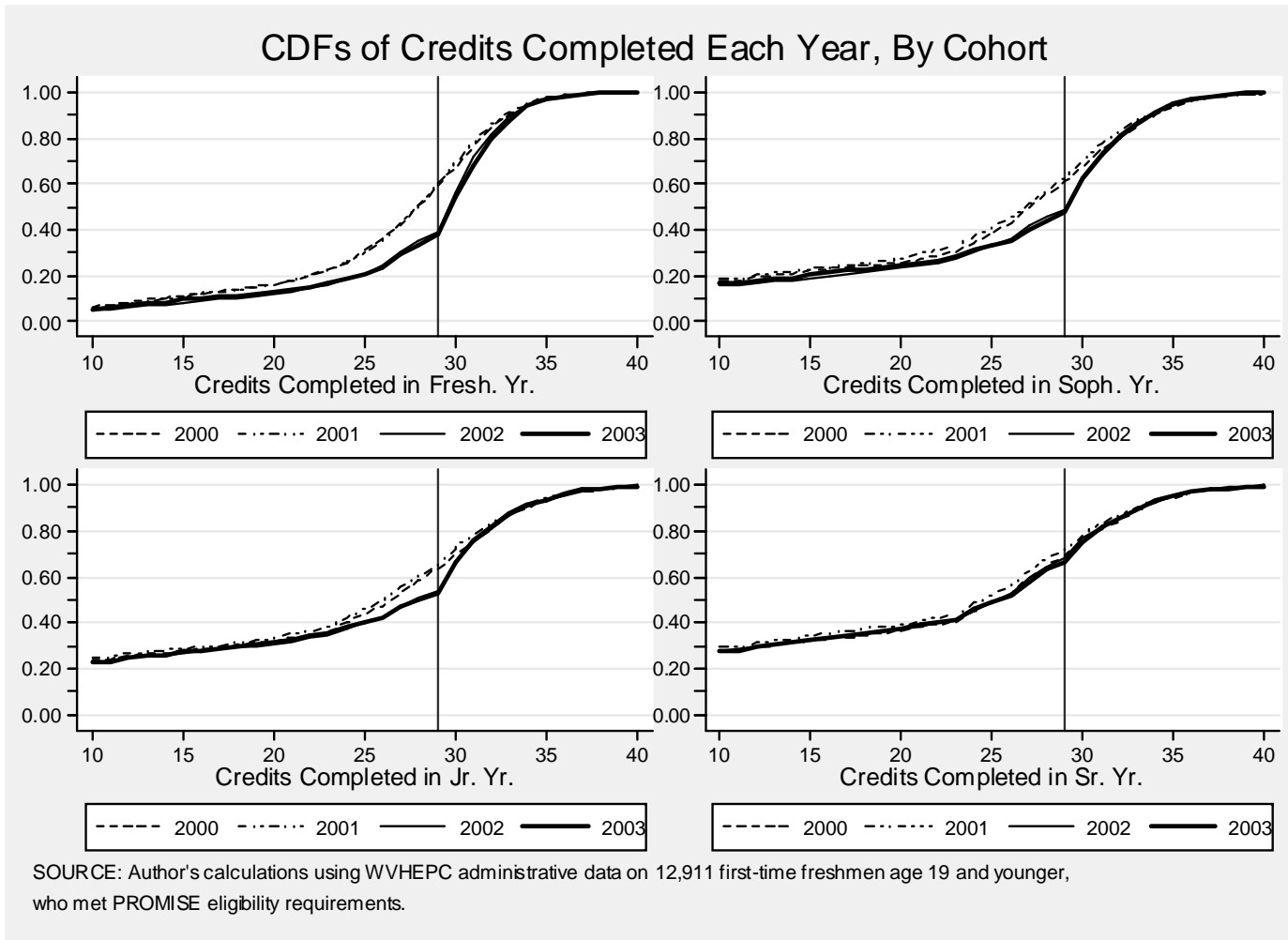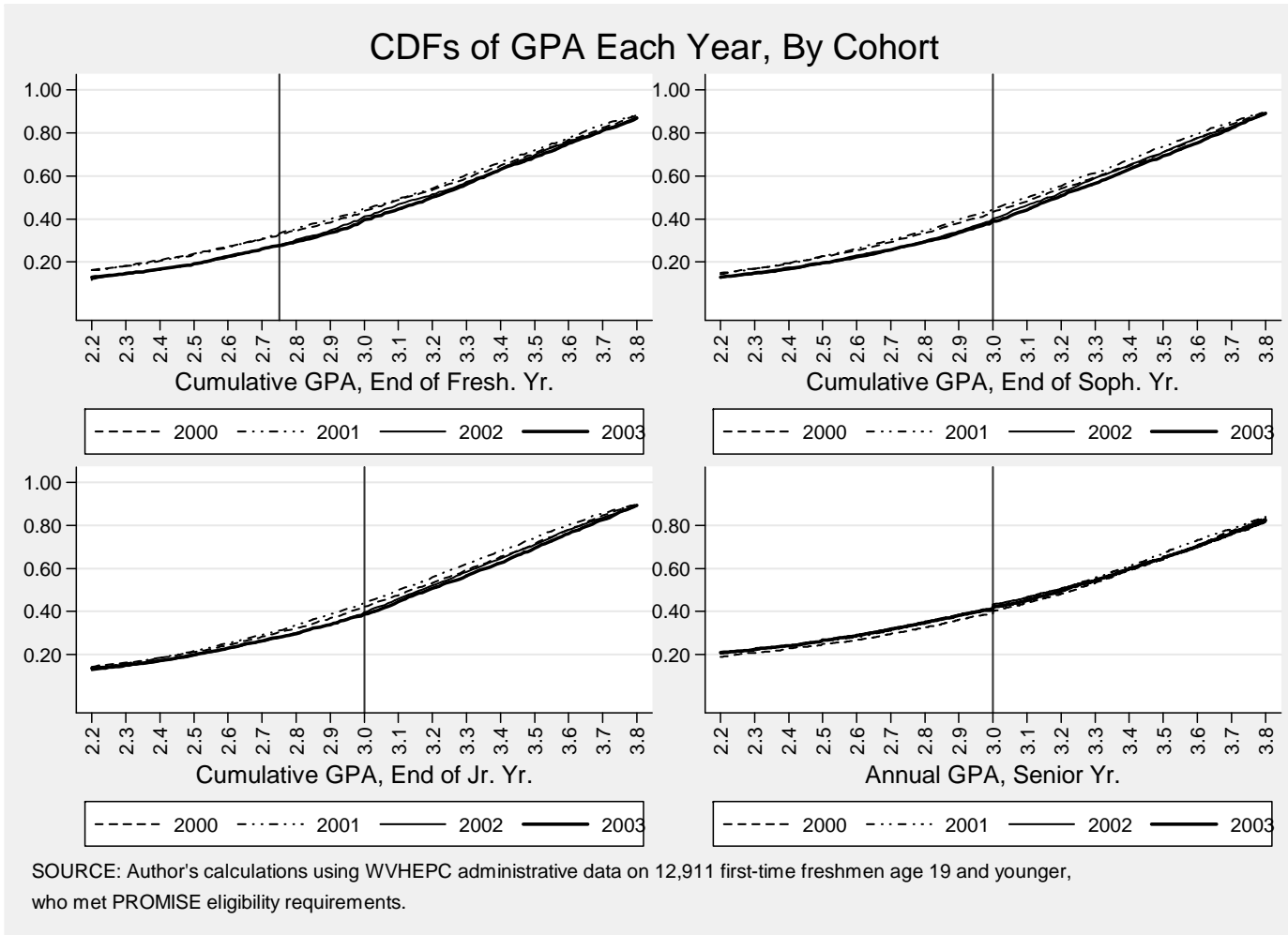## List of Appendices

A. Additional Information on WV PROMISE and the WVHEPC Database

B. Supplementary Analyses (covariates, subgroups, additional outcomes)

C. Cost-Benefit Analysis

***APPENDIX ***

APPENDIX A:

Additional Information on WV PROMISE and the WVHEPC Database

Additional details on PROMISE. Students must have graduated from high school in 2002 or later to qualify. No students were "grandfathered" into the program. College credits earned in high school do not count towards the annual renewal requirements. Students may take courses over the summer in order to meet the GPA or credit requirements, but must do so at their own expense. Enrollment must be continuous, and those who fail to meet renewal requirements once cannot later regain the scholarship. Beginning in 2004-05, initial eligibility requirements became increasingly stringent. The state also created a "leave of absence" policy enabling students to take time off from college (or delay entry) without forfeiting their scholarship eligibility. This analysis focuses on the first two PROMISE cohorts (2002-03 and 2003-04), for whom the original requirements applied. Table A.1 provides a list of West Virginia two- and four-year institutions at which the scholarship could be used (note: only public institutions are represented in the WVHPEC dataset).

To claim the scholarship, students must complete a one-page "Application for State Level Financial Aid Programs" which collects little more than students' identifying information. No financial information is collected and no academic information is collected beyond the student's high school and intended college. Students also must file the Free Application for Federal Student Aid (FAFSA) in order to claim PROMISE.

Students' PROMISE awards are not reduced by other aid received. PROMISE awards will generally not affect federal Pell eligibility, but may reduce eligibility for the state's need-based Higher Education Grant Program (HEGP) for students whose total "need" is already met. HEGP provides funds equaling up to 75 percent of stated tuition and fees for those who have unmet need remaining after accounting for PROMISE, Pell, and other scholarships. Because HEGP awards cannot exceed the total cost of attendance after subtracting the federally-determined EFC, PROMISE, Pell and other grants, some students may receive smaller HEGP awards as a result of PROMISE. In practice, PROMISE reduces cumulative HEGP awards over four years of schooling by an average of just $313—a small fraction of the average $10,000 value of PROMISE. The reductions tend to be higher for students with less need; students receiving Pell Grants and PROMISE saw virtually no reductions in HEGP awards.

PROMISE is relatively selective compared to some other state merit aid programs, such as those introduced in Arkansas and Georgia in the early 1990s. For example, Georgia's HOPE scholarship is based only on achieving a 3.0 high school GPA, and Arkansas' program requires only a 19 on the ACT in addition to a 2.5 high school GPA requirement (Dynarski 2002). Among large-scale merit-based aid programs, West Virginia's 30-credit renewal requirement is also unusual. Several programs require only a minimum college GPA (typically 3.0) and "full-time" enrollment (equivalent to 24 credits per year) to renew; some have no minimum credit requirements. I could identify only one other state program with a 30-credit-per-year renewal requirement (South Carolina's LIFE scholarship).

Table A.2 provides descriptive statistics on students who met the high school GPA and test score requirements before and after the introduction of PROMISE (hereafter, "eligible enrollees"). For comparison, I also provide statistics for all young public college enrollees in West Virginia, including enrollees from out-of-state. Eligible enrollees have significantly higher high school GPAs and test scores, and are more likely to enroll in a four-year college than the typical WV student. Among eligible enrollees, those who enrolled after 2002 were more likely to take the SAT, had slightly lower test scores, and were slightly more likely to enroll in a four-year college (particularly the state's flagship, West Virginia University).[54]

Additional details on WVHEPC data. Permission to utilize these data, stripped of all personal identifiers, was provided by WVHEPC under a restricted-use data agreement. The original data are limited to 64,280 first-time freshmen in the entering cohorts of 2000-01 through 2004-05. Because of changes in eligibility requirements as well as insufficient follow-up, I exclude the 2004-05 cohort. I further limit all analyses to young entrants (age 19 and under), who were the targets of PROMISE. I exclude approximately 5 percent of young entrants who were missing a high school GPA or ACT/SAT score. From the remaining 40,792 students, I select samples appropriate to each analysis. Much of the analysis focuses on the 12,911 enrollees who were West Virginia residents and who met the high school GPA and ACT/SAT requirements for PROMISE eligibility between 2000-01 and 2003-04.

Before stripping the file of identifiers, WVHEPC matched the student records to quarterly employment data as reported to the state's Employment Security agency. The

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[54] Note that PROMISE generates incentives to take the SAT in addition to the ACT (some students may do better on the SAT) and eliminates the price difference between WVU and other WV public institutions.

employment data covers calendar years 2002 through 2006, and cover only West Virginia employment. In theory, this is a non-trivial limitation given that West Virginia's two largest universities are located within a few miles of state borders (West Virginia University in Morgantown borders Pennsylvania while Marshall University in Huntington borders both Kentucky and Ohio). In practice, these earnings data appear quite comparable to students' self-reports on the FAFSA, which include earnings from any state: for the 2002 and 2003 entry cohorts during the first two full calendar years after college entry, total administratively-reported West Virginia earnings represented 88 to 94 percent of total self-reported earnings on the FAFSA (among those who filed a FAFSA). The discrepancy may be even smaller during the school year, but this is impossible to test since earnings are only reported annually on the FAFSA. [Since FAFSA data are only available beginning in 2002, and then are only available for those who apply for financial aid, I do not utilize FAFSA earnings in my main analysis.] Even if absolute earnings levels are somewhat underestimated, this will not compromise the analysis as long as the West Virginia share of total earnings is relatively stable over time.

***APPENDIX ***

Table A.1

Public and Private 2/4 Year Colleges in West Virginia

Institution Name	City	Type	Undergraduate Enrollment
West Virginia University	Morgantown	4-year, Public	20590
Marshall University	Huntington	4-year, Public	9723
Fairmont State University	Fairmont	4-year, Public	4264
Shepherd University	Shepherdstown	4-year, Public	3970
WVU at Parkersburg	Parkersburg	4-year, Public	3884
West Virginia State University	Institute	4-year, Public	3465
Concord University	Athens	4-year, Public	2693
West Liberty State College	West Liberty	4-year, Public	2260
Bluefield State College	Bluefield	4-year, Public	1923
West Virginia University Institute of Tech.	Montgomery	4-year, Public	1462
Glenville State College	Glenville	4-year, Public	1381
West Virginia Northern Community Coll	Wheeling	2-year, Public	2844
Pierpont State Community & Technical Coll	Fairmont	2-year, Public	2809
Marshall Community & Technical Coll	Huntington	2-year, Public	2579
Southern West Virginia Comm & Tech Coll	Mount Gay	2-year, Public	2317
Blue Ridge Community & Technical Coll	Martinsburg	2-year, Public	1953
New River Community & Technical Coll	Beckley	2-year, Public	1861
WV State Community & Technical Coll	Institute	2-year, Public	1717
Potomac State Coll of WVU	Keyser	2-year, Public	1485
Eastern WV Community & Technical Coll	Moorefield	2-year, Public	786
Community & Technical Coll at WVU Tech	Montgomery	2-year, Public	678
Mountain State University	Beckley	4-year, Private not-for-profit	3921
Wheeling Jesuit University	Wheeling	4-year, Private not-for-profit	1203
West Virginia Wesleyan College	Buckhannon	4-year, Private not-for-profit	1176
University of Charleston	Charleston	4-year, Private not-for-profit	1074
Bethany College	Bethany	4-year, Private not-for-profit	833
Davis & Elkins College	Elkins	4-year, Private not-for-profit	636
Alderson Broaddus College	Philippi	4-year, Private not-for-profit	623
Ohio Valley University	Vienna	4-year, Private not-for-profit	527
Appalachian Bible College	Mount Hope	4-year, Private not-for-profit	274
Salem International University	Salem	4-year, Private for-profit	420
Huntington Junior College	Huntington	2-year, Private for-profit	785
National Institute of Technology	Cross Lanes	2-year, Private for-profit	315
West Virginia Junior College-Bridgeport	Bridgeport	2-year, Private for-profit	214
West Virginia Junior College	Morgantown	2-year, Private for-profit	152
West Virginia Junior College	Charleston	2-year, Private for-profit	150
Mountain State College	Parkersburg	2-year, Private for-profit	106
Valley College	Beckley	2-year, Private for-profit	90
Valley College	Martinsburg	2-year, Private for-profit	68
West Virginia Business College-Wheeling	Wheeling	2-year, Private for-profit	62
Valley College	Princeton	2-year, Private for-profit	19

Source: Integrated Postsecondary Education Data System (IPEDS).

Table A.2

Descriptive Statistics for West Virginia Public College Enrollees

Outcome	All degree-seeking enrollees, age 19 and under, enrolling before PROMISE	PROMISE-Eligible Enrollees		
		Before	After	diff.
Percent female	0.524	0.568	0.556	-0.012
Percent white, non-hispanic	0.927	0.972	0.967	-0.005
Age at entry	18.577	18.531	18.534	0.002
High school GPA	3.133	3.640	3.639	0.000
Took the ACT	0.747	0.908	0.905	-0.003
Took the SAT	0.253	0.092	0.142	0.050 ***
ACT score (or equivalent)	20.634	24.234	24.193	-0.041 **
Entered a 4-Year institution [a]	0.789	0.880	0.892	0.012 **
Entered West Virginia Univ. [a]	0.340	0.379	0.409	0.030 ***
Entered as a full-time student [a]	0.981	0.989	0.996	0.007 ***
West Virginia resident	0.721	1.000	1.000	0.000
Received any Pell Grant, First Yr.	0.325	0.310	0.315	0.005
Sample size	20,217	5,777	7,134	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, entering public WV two- and four-year colleges. "Before" cohorts are those entering in Fall 2000 and Fall 2001; "after" cohorts are those entering in Fall 2002 and Fall 2003. "PROMISE-eligible enrollees" are West Virginia resident students who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Stars indicate the significance of before-after differences at the $p<0.10$, $p<0.05$, or $p<0.01$ level.

[a] These measures are likely endogenous to PROMISE receipt.

APPENDIX B: Additional Analyses

Table B.1
Unclustered and Clustered Standard Errors

Outcome	Regression Discontinuity				Cohort Analysis			
	(1) Robust SEs		(2) Clustered-ACT		(1) Robust SEs		(2) Clustered-Year	
	B	(SE)	B	(SE)	B	(SE)	B	(SE)
Received PROMISE	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)
Value of PROMISE in Year 1	$3,012 ***	($21)	$3,012 ***	($10)	$3,077 ***	($7)	$3,077 ***	($120)
Total PROMISE received (over 4 years)	$8,338 ***	($180)	$8,338 ***	($87)	$10,101 ***	($65)	$10,101 ***	($255)
Number of semesters enrolled (over 4 years)	0.037	(0.130)	0.037	(0.044)	0.146 ***	(0.041)	0.146 **	(0.045)
Total credits earned (over 4 years)	4.644 *	(2.519)	4.644 ***	(1.163)	5.782 ***	(0.864)	5.782 **	(1.136)
Cumulative GPA (over 4 years) [a]	0.099 **	(0.045)	0.099 ***	(0.017)	0.039 ***	(0.015)	0.039	(0.018)
Typical weekly school-year earnings [b]	-$2.12	($7.04)	-$2.12	($6.30)	-$9.55 ***	($2.17)	-$9.55 **	($2.10)
Earned 120 credits by end of Year 4	0.095 ***	(0.026)	0.095 ***	(0.014)	0.111 ***	(0.010)	0.111 ***	(0.018)
Had 3.0+ cumulative GPA at end of Year 4	0.090 ***	(0.027)	0.090 ***	(0.008)	0.035 ***	(0.009)	0.035 *	(0.012)
Earned BA within 4 Years	0.094 ***	(0.022)	0.094 ***	(0.016)	0.067 ***	(0.009)	0.067 ***	(0.005)
Earned BA within 5 Years	0.045 *	(0.028)	0.045 ***	(0.013)	0.037 ***	(0.010)	0.037 *	(0.012)
Observations	8,567		8,567		12,911		12,911	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. The RD sample is further restricted to those with an ACT score between 16 and 25. The cohort analysis sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Robust standard errors in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared. RD regressions also include local linear controls for ACT score; cohort regressions include indictors for each ACT score. Cell mean regressions are performed by residualizing both the outcome and the treatment (actual PROMISE receipt), then collapsing these residuals and running the regressions on cell means, weighted by cell size. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

Table B.2
Comparing Cohort Analysis and RD Estimates

Outcome	(1) IV Before/After All WV Eligibles		(2) IV Before/After ACT=21 only		(3) IV Diff-in-Diff ACT 20/21		(4) Fuzzy RD 16<=ACT<=25	
	B	(SE)	B	(SE)	B	(SE)	B	(SE)
Received PROMISE	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)	1.000 ***	(0.000)
Value of PROMISE in Year 1	$3,077 ***	($120)	$2,987 ***	($134)	$3,028 ***	($129)	$3,012 ***	($21)
Total PROMISE received (over 4 years)	$10,101 ***	($255)	$8,666 ***	($307)	$8,774 ***	($342)	$8,338 ***	($180)
GPA, end of year 1	0.077 ***	(0.006)	0.119 **	(0.025)	0.205 *	(0.087)	0.156 ***	(0.051)
Credits earned, end of year 1	1.830 ***	(0.118)	2.512 ***	(0.367)	2.497 *	(0.938)	2.095 ***	(0.461)
Number of semesters enrolled (over 4 years)	0.146 **	(0.045)	0.150 *	(0.053)	0.097 *	(0.033)	0.037	(0.130)
Total credits earned (over 4 years)	5.782 **	(1.136)	4.192 ***	(0.596)	3.734	(2.189)	4.644 *	(2.519)
Cumulative GPA (over 4 years) [a]	0.039	(0.018)	0.059	(0.027)	0.089	(0.076)	0.099 **	(0.045)
Typical weekly school-year earnings [b]	-$9.55 **	($2.10)	$1.79	($1.77)	$5.01	($5.28)	-$2.12	($7.04)
Earned 120 credits by end of Year 4	0.111 ***	(0.018)	0.096 **	(0.027)	0.093 ***	(0.013)	0.095 ***	(0.026)
Had 3.0+ cumulative GPA at end of Year 4	0.035 *	(0.012)	0.038 ***	(0.002)	0.044	(0.022)	0.090 ***	(0.027)
Earned BA within 4 Years	0.067 ***	(0.005)	0.081 **	(0.016)	0.091 ***	(0.002)	0.094 ***	(0.022)
Earned BA within 5 Years	0.037 *	(0.012)	0.033 *	(0.013)	0.063 *	(0.022)	0.045 *	(0.028)
Sample size	12,911		2,364		4,398		8,567	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. Unless otherwise noted, the sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility. NOTES: Robust standard errors (clustered by year, except for RD) are in parentheses. Before/after regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Column (2) estimates the IV before/after only for students with an ACT score of 21. Column (3) limits the sample to those with an ACT score of 20 or 21 and uses after*(ACT=21) as the instrument for PROMISE receipt. The RD regressions include the same covariates but control for a local linear function of ACT scores. Stars indicate the significance of individual findings at the p<0.10, p<0.05, or p<0.01 level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

Table B.3
Cohort Analysis for Key Outcomes by Gender

Outcome	Women (1) Pre-Mean	Women (2) IV Before/After B	(SE)	Men (3) Pre-Mean	Men (4) IV Before/After B	(SE)
Received PROMISE	0.000	1.000 ***	(0.000)	0.000	1.000 ***	(0.000)
Value of PROMISE in Year 1	$0	$3,044 ***	($114)	$0	$3,121 ***	($124)
Total PROMISE received (over 4 years)	$0	$10,353 ***	($247)	$0	$9,766 ***	($271)
Number of semesters enrolled (over 4 years)	6.704	0.175 **	(0.042)	6.766	0.105	(0.061)
Total credits earned (over 4 years)	98.853	5.355 **	(1.386)	95.084	6.297 ***	(0.815)
Cumulative GPA (over 4 years) [a]	3.085	0.021	(0.028)	2.847	0.063 ***	(0.005)
Typical weekly school-year earnings [b]	$88.58	-$10.02 ***	($1.30)	$81.48	-$9.04 *	($3.03)
Earned 120 credits by end of Year 4	0.464	0.106 **	(0.025)	0.388	0.117 ***	(0.008)
Had 3.0+ cumulative GPA at end of Year 4	0.653	0.033	(0.016)	0.506	0.037 **	(0.007)
Earned BA within 4 Years	0.304	0.067 ***	(0.007)	0.218	0.068 ***	(0.002)
Earned BA within 5 Years	0.535	0.031	(0.014)	0.475	0.044 **	(0.014)
Sample size		7,248			5,663	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. The sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: Robust standard errors, clustered by cohort, in parentheses. All regressions include indicator controls for race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

***APPENDIX ***

Table B.4
RD and Cohort Analysis Estimates of PROMISE Impact on Additional Outcomes

Outcome	(1) Means, ACT=20	(2) Fuzzy RD B	(SE)	(3) Pre-means, all eligibles	(4) Cohort Analysis B	(SE)
Entered a four-year college	0.771	0.016	(0.024)	0.880	0.017 **	(0.004)
Entered West Virginia University	0.238	0.057 **	(0.025)	0.379	0.041 *	(0.013)
Entered Marshall University	0.215	-0.028	(0.024)	0.238	-0.005	(0.003)
Entered another two/four year college	0.547	-0.029	(0.029)	0.383	-0.036 **	(0.011)
Enrolled full-time, full year in first year	0.899	0.036 **	(0.016)	0.932	0.024 ***	(0.001)
Continuously enrolled FTFY, two years	0.735	0.021	(0.025)	0.793	0.040 ***	(0.006)
Continuously enrolled FTFY, three years	0.611	0.015	(0.028)	0.693	0.042 **	(0.012)
Continuously enrolled FTFY, four years	0.502	0.032	(0.029)	0.603	0.039 **	(0.011)
Annual GPA in first year [a]	2.523	0.156 ***	(0.051)	2.849	0.077 ***	(0.006)
Annual GPA in second year	2.498	0.079	(0.056)	2.796	0.088 *	(0.033)
Annual GPA in third year	2.537	0.083	(0.056)	2.846	0.045	(0.020)
Annual GPA in fourth year	2.624	0.056	(0.057)	2.923	-0.018	(0.022)
Credits earned in first year	24.529	2.095 ***	(0.461)	26.239	1.830 ***	(0.118)
Credits earned in second year	21.111	0.688	(0.668)	23.384	1.409 **	(0.333)
Credits earned in third year	18.631	0.790	(0.746)	21.654	1.070 *	(0.351)
Credits earned in fourth year	17.190	0.298	(0.781)	19.890	0.465	(0.384)
Summer credits (over three summers)	3.035	0.927 ***	(0.284)	3.457	0.805 ***	(0.058)
Earned BA within 4 Years	0.155	0.094 ***	(0.022)	0.267	0.067 ***	(0.005)
Earned BA by summer after 4th year	0.177	0.092 ***	(0.023)	0.288	0.077 ***	(0.005)
Earned BA by fall of 5th year	0.253	0.076 ***	(0.025)	0.378	0.063 ***	(0.007)
Earned BA within 5 years	0.367	0.045 *	(0.028)	0.509	0.037 *	(0.012)
Earned AA within 2 years	0.019	0.016 *	(0.009)	0.024	-0.002	(0.002)
Earned AA within 5 years	0.119	-0.023	(0.018)	0.086	-0.013 *	(0.005)
Entered as a math/science major	0.082	-0.003	(0.017)	0.236	-0.013	(0.008)
Earned a BA in math/science w/in 4 years	0.011	0.012	(0.007)	0.057	0.013 ***	(0.001)
Earned a BA in math/science w/in 5 years	0.035	0.001	(0.011)	0.108	0.008 *	(0.003)
Earned BA w/in 4 years, and was employed or enrolled in WV in next fall [b]	0.121	0.076 ***	(0.020)	0.208	0.041 ***	(0.005)
Federal student loans, first year (2007$)	$1,132	-$425 ***	($89)	$1,072	-$298 **	($70)
Cumulative federal student loans (2007$)	$5,968	-$972 **	($407)	$6,007	-$780 **	($175)
Sample size	972	8,567		5,777	12,911	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking West Virginia resident freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04, who had a high school GPA of at least 3.0. The RD sample is further restricted to 2002 and 2003 entrants who had an ACT between 16 and 25. The cohort analysis is restricted to entrants who had at least a 21 on the ACT.
NOTES: Robust standard errors, clustered by cohort, in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Robust standard errors are in parentheses. Stars indicate the significance of individual findings at the p<0.10, p<0.05, or p<0.01 level. [a] For students who drop out, annual GPA is imputed as the cumulative GPA when last enrolled. [b] Data are not yet available for employment/enrollment in the fall following the fifth year.

Table   B.5
Time Series Results for Key Outcomes by Pell Grant Status

Outcome	Pell Grant Recipients			Non-Recipients		
	(1) Pre-Mean	(2) Time Series IV		(3) Pre-Mean	(4) Time Series IV	
		B	(SE)		B	(SE)
Received PROMISE	0.000	1.000 ***	(0.000)	0.000	1.000 ***	(0.000)
Value of PROMISE in Year 1	$0	$2,992 ***	($124)	$0	$3,116 ***	($119)
Total PROMISE received (over 4 years)	$0	$9,022 ***	($279)	$0	$10,593 ***	($252)
Number of semesters enrolled (over 4 years)	6.449	0.068	(0.035)	6.857	0.187 **	(0.048)
Total credits earned (over 4 years)	90.3	4.435 ***	(0.584)	100.3	6.491 **	(1.228)
Cumulative GPA (over 4 years) [a]	2.893	0.005	(0.016)	3.023	0.056 *	(0.023)
Typical weekly school-year earnings [b]	$90.97	-$7.68 ***	($1.02)	$83.06	-$10.52 **	($2.70)
Earned 120 credits by end of Year 4	0.363	0.094 **	(0.017)	0.461	0.119 ***	(0.016)
Had 3.0+ cumulative GPA at end of Year 4	0.551	0.016	(0.012)	0.607	0.044 **	(0.012)
Earned BA within 4 Years	0.217	0.061 **	(0.012)	0.290	0.070 ***	(0.004)
Earned BA within 5 Years	0.443	0.008	(0.018)	0.539	0.051 **	(0.015)
Sample size		4,038			8,873	

SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking freshmen aged 19 and younger, enrolling in the fall semester of school years 2000-01 through 2003-04. The sample is restricted to West Virginia residents who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.

NOTES: About 31% of PROMISE-eligible students receive Pell Grants; the rate is stable before and after PROMISE implementation (see Table 2). Robust standard errors, clustered by cohort, in parentheses. All regressions include indicator controls for gender, race/ethnicity, age, high school GPA and GPA squared, and indictors for each ACT score. Stars indicate the significance of individual findings at the $p<0.10$, $p<0.05$, or $p<0.01$ level. [a] For students who drop out, cumulative GPA is imputed as the cumulative GPA when last enrolled. [b] I calculate average weekly earnings based on the four quarters of school year employment data that are available for all cohorts, corresponding to the spring of the second (sophomore) year, the spring and fall of the third year, and the fall of the fourth year following enrollment.

APPENDIX C: Cost-Benefit Analysis

This cost-benefit analysis follows the general approach of Dynarski (2008), who evaluates PROMISE-like programs in Georgia and Arkansas. I assume an enrollment impact of 4 percentage points among high school graduates (this is based on the analysis of enrollment trends at the end of Section IV, and attributes one percentage point of the 5 percentage point increase in eligible enrollment to students who would otherwise have enrolled out of state). I also assume that PROMISE did not affect the high school graduation rate. This is plausible for the first PROMISE cohorts, who were already at least halfway through high school when PROMISE funding was announced.

Direct costs. Using WVHEPC administrative data on PROMISE recipients, and inflating amounts to 2007 dollars, I calculate that the direct scholarship costs were approximately $67 million for the cohorts entering college in 2002 and 2003. Using data on the number of high school graduates from the WV Dept. of Education along with an estimated 82 percent WV high school graduation rate from the American Community Survey (ACS) 2005, I estimate that there are 43,663 total individuals in the two corresponding age cohorts (i.e. all potential recipients of the program). This generates an average direct cost of $1,548 per potential recipient.

Additional state subsidy costs. The cost to the state of providing a year of college is generally much more than tuition and fees. I assume that tuition and fees cover 30 percent of total resource costs (this follows Dynarski, and is based on estimates from Winston [1999]). In 2007 dollars, average annual tuition/fees for students in the first two PROMISE cohorts was approximately $3,900, implying subsidy costs of $9,100 (=3900/0.30 - 3900) per student per year. Given that the typical eligible student enrolled for 3.75 years (after five years of follow up), this implies 0.12 years of schooling induced by the program per potential recipient (=0.82*0.04*3.75). This generates additional costs of $1,092 per potential recipient.

Foregone wages. Individuals induced to enroll by PROMISE spend less time in the labor market. I assume foregone wages of $18,000 per year for non-college graduates (this is a generous estimate, based on first-year estimated earnings for WV residents who attain some college but less than an Associate's Degree [WV Higher Education Report Card 2007]). This opportunity cost is reduced, however, by student employment. Using the WVHEPC data, I

estimate that eligible enrollees earn about $4,000 per calendar year while enrolled. Therefore, opportunity costs add an additional $1,680 per potential recipient (=14000*.12).

Deadweight loss of taxation. Following Dynarski, I assume a deadweight loss of taxation equal to 0.245 times the direct scholarship costs and indirect subsidy costs of PROMISE. This adds $645 per person to the cost of the program, for a total cost of $4,960 per person.

Time-to-degree benefits. If the five-year BA completion attenuates completely, this implies that 10.4 percent of PROMISE recipients graduate one year early (6.7 in four instead of five, plus 3.7 in five instead of six). Assuming first-year graduate earnings of $27,000 (estimate based on WV administrative data, reported in WVHEPC 2007 Education Report Card), a real return to experience of 5 percent over the first 10 years (this is consistent with Census figures of earnings for 18 to 24 year olds versus 25 to 34 year olds with the same education level), a real discount rate of 4 percent, and student earnings of $4,000, the net present value of graduating one year early is $36,500. [It is not clear whether finishing early reduces the resource costs of obtaining the degree, so I make the conservative assumption that it does not.] This generates benefits of $607 per potential recipient (=36500*0.82*0.195*0.104). If the five-year BA effect persists, time-to-degree benefits would be $391.

Benefits from increased BA attainment. If the five-year graduation rate impact among enrollees persists, PROMISE increased the overall BA attainment rate (BA completers as a proportion of all individuals in an age cohort) by 2.3 percentage points. This calculation uses an estimated 4 percentage point impact on eligible enrollment among high school graduates (from a baseline rate of 15.5 percent) and a graduation impact of 3.7 percentage points among eligible enrollees (from a baseline rate of 50.9 percent): 2.3=.82*.195*.546 - .82*.155*.509. Even if the graduation rate impact does not persist, the enrollment impact would still raise the BA attainment rate by 1.8 percentage points.

Barrow and Rouse (2005) estimate that college graduates earn $440,000 more in present value than high school graduates (I have inflated their figure to 2007 dollars). However, some of this difference may be due to underlying ability rather than the causal effect of education. Causal estimates of the return to a year of schooling fall between 9 and 15 percent for recent cohorts (see Dynarski 2008 for a review). Using a middling estimate of 12 percent, and assuming that a BA is worth four years of schooling, and expected lifetime earnings for high school graduates are about $477,000 (Dynarski 2008; I inflate her estimate from 2002 to 2007

dollars), then the causal earnings gain from a BA is about $274,000. Depending on whether or not the graduation rate impact persists, this generates benefits worth $4,925 to $6,293 per person.

Benefits from increased "some college" attainment. In addition to benefits from increased BA attainment, PROMISE increases the proportion of the population with some college but less than a BA. If the graduation rate impact persists, the increase in this category is 1.0 percentage point for the entire age cohort (=0.82*0.195*0.454-0.82*0.155*0.491; note: among eligible enrollees, PROMISE did not affect the proportion earning an AA). If the graduation rate impact does not persist, this implies a larger increase in the percentage of students with some college of about 1.5 percentage points. If some college but no degree is worth two years of schooling, this adds benefits of $1,200 to $1,800 per person (=0.01*$121,000).

Summary. Total social costs of PROMISE are estimated to be $4,960 per person while total social benefits are at least $7,352 per person even if the five-year graduation impact eventually fades out completely. Under these assumptions, the program easily passes a cost-benefit test, generating net benefits of $2,392 per potential recipient (a 48 percent net return on investment). Even if the five-year graduation impact fades out and returns to schooling are lower than estimated, the program would break even in social welfare terms as long as the return to a year of schooling is about 8 percent.

The above analysis treats scholarships paid to students whose enrollment was not affected as a cost. Based on my analysis in Section IV, I estimate that approximately 75 percent of the direct scholarship costs are paid to intramarginal enrollees. If these payments are considered as a transfer rather than a cost, the estimated social cost of the program falls to $3,800, net social benefits rise to at least $3,548, and the break-even rate of return to schooling is about 6 percent.

Additional factors which are difficult to estimate, but likely add to the social benefits, include any lifetime earnings gain to higher cumulative GPAs at graduation, any health improvements due to the increase in educational attainment, and any economic externalities to increasing the skills of the labor force.