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

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ABSTRACT

Programs that link substantial amounts of college financial aid to student achievement have proved increasingly popular in recent years. These programs could work either by relaxing financial constraints or by inducing additional student effort (or both). I examine the PROMISE scholarship in West Virginia, which provides free tuition and fees to college students who maintain a minimum GPA and course load. Using an unusually comprehensive administrative database, I exploit discontinuities in both the eligibility formula and the timing of implementation to identify program effects. I find robust and statistically significant impacts on key academic outcomes, including a 6.7 percentage point increase in four-year BA completion rates among PROMISE recipients. Impacts are concentrated at the precise thresholds for annual scholarship renewal—particularly the minimum course load requirement—and disappear in the fourth year of college when students are still receiving the scholarship but no longer have the opportunity to renew. The findings suggest that the program works by establishing clear academic goals and incentives to meet them, rather than simply reducing the cost of college.

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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. Furthermore, only about one-third of students who earn a "four-year" degree in WV do so within four years of college entry (compared to about 50 percent nationally).[2] Whether America regains its most-educated-nation status in the 21st century depends, in part, on the success or failure of recent efforts to improve these educational outcomes in struggling states like West Virginia.

The solutions, however, are not entirely obvious. A large body of existing research shows that financial aid can increase college enrollments (see Dynarski 2002 for a review of this literature), but much less is known about whether financial aid affects college achievement and completion.[3] Entry alone is clearly no guarantee of college success: only 36 percent of college entrants complete a bachelor's degree within six years and a mere 18 percent complete within four years.[4] Moreover, under Manski's (1989) college-as-experimentation framework, aid policies that encourage enrollment could attract some students with below-average preparation or motivation and thus an even lower likelihood of completion.

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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] The West Virginia figure is based on author's calculations for students who entered college in 2000, and were followed for eight years. The national figure based on NCES data from the Beginning Postsecondary Students (BPS) 1996 survey, which followed students for only six years.

[3] Dynarski (2003) finds a large but noisy increase in years of schooling among students who benefitted from the Social Security Student Benefit program (eliminated in 1983); Bettinger (2004) finds suggestive evidence that need-based federal Pell Grants improve college persistence but the estimates are highly sensitive to specification.

[4] Author's calculations using data from BPS: 1996-2001, based on all degree-seeking students at 2- and 4-year colleges. The corresponding rates among students entering four-year colleges are 67 and 36 percent, respectively.

Joint concerns about both students' financial constraints and their preparation or motivation may explain the increasing popularity of programs offering large financial incentives for college achievement.[5] These programs simultaneously reduce the cost of college, and provide 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.[6] In the five years between 1998-99 and 2003-04, the value of merit-based state grants more than doubled in real terms, from $0.7 billion to nearly $1.5 billion (National Association of State Student Grant and Aid Programs [NASSGAP] 2005). The federal government also jumped on the bandwagon in 2007, introducing Academic Competitiveness Grants (ACG) that offer $750 to college freshmen and $1,300 to sophomores who complete a "rigorous secondary school program" and maintain a 3.0 GPA in college.[7]

Previous research shows that merit-based aid, like traditional financial aid, can increase college enrollments (Kane 2003; Dynarski 2004; Cornwell, Mustard and Sridhar 2006). But the scant evidence on subsequent outcomes is somewhat mixed. Cornwell, Lee, and Mustard (2005) find that a merit-aid program in Georgia significantly *reduced* students' courseloads in the freshman year. Brock and Richburg-Hayes (2006) find that financial incentives for community college students can increase GPAs and number of courses completed in the first two years, while Angrist, Lang, and Oreopoulos (forthcoming 2009) find essentially no effects of large financial incentives in the first two years.[8] Only a single study (Dynarski 2008) examines college completions. Dynarski finds a positive effect, but cannot

[5] 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; 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.
[6] 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.
[7] ACG awards are also limited by financial need; only students who qualify for a federal Pell Grant are eligible.
[8] Although they 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.

distinguish the mechanisms: do the incentives for achievement matter, or do these programs work mainly by relaxing financial constraints for some needy students caught in a very widely-cast net of assistance?

This paper contributes to the literature by examining the effects of a large financial incentive program on college completions as well as more proximal outcomes such as persistence, cumulative GPA, credits earned, and school-year employment. West Virginia's PROMISE scholarship, implemented in 2002, offers full tuition and fees for new college entrants who earn at least a 3.0 GPA in high school and an ACT score of 21 or higher (or SAT equivalent) and enroll full-time at any West Virginia public institution.[9] Once enrolled, students must maintain a 3.0 GPA (2.75 in the first year) and complete at least 30 credits per year in order to renew the scholarship for the following year. I exploit discontinuities both in the program's initial eligibility requirements and in the timing of implementation to identify effects. I also leverage the unusual depth and breadth of a restricted-use administrative dataset to get inside the black box of program mechanisms and partially disentangle the role of income effects (i.e. relaxation of financial constraints) from incentive effects (i.e. inducements for increased student effort).

I utilize two complementary quasi-experimental approaches to identify causal effects. The first is a regression-discontinuity (RD) analysis based on the ACT score threshold for PROMISE eligibility. Limiting the sample to college enrollees who met the high school GPA requirement, I compare outcomes for students on either side of the threshold. I test the robustness of these results to variations in bandwidth and functional form. The second approach is a cohort analysis based on the discontinuous timing of program implementation. I limit the sample to enrollees meeting both the high school GPA and ACT requirements, and compare outcomes for those entering just before and just after 2002. Results are robust to the inclusion of parametric and non-parametric controls.

The primary threat to identification under either approach is selection bias. My analysis is limited to college enrollees, but the offer of PROMISE may affect who enrolls in the first place. Beyond controlling for an extremely rich set of covariates, I also address this concern explicitly through a bounding exercise. I estimate the proportion of recipients who were induced to enroll because of

[9] The program is described in full detail later in the paper. Some rules have changed for recent cohorts.

PROMISE, and then estimate the resulting bias under varying assumptions about how "marginal" enrollees differ from the average recipient.

To preview the results, I find that PROMISE increased five-year graduation rates by 3.7 percentage points among recipients, from a baseline of 51 percent. Four-year graduation rates increased by 6.7 percentage points from a baseline rate of just 26 percent, indicating a significant improvement in time-to-degree. These effects are generated by relatively small increases in credit accumulation that move students over the threshold required for graduation: by the end of the senior year, PROMISE recipients had earned an additional 6 credits on average (a 6 percent increase) but were 11 percentage points more likely to have completed the minimum 120 credits required for graduation (a 25 percent increase). The RD and the cohort analysis generate broadly similar results, with the RD suggesting possibly larger impacts for students near the initial eligibility cutoff. The bounding exercise shows that the impacts are too large to be explained by compositional change. If anything, the impacts occur *despite* compositional change, rather than because of it.

I find evidence that income effects alone cannot explain the results; indeed, the program's incentives for college achievement appear central to its impacts. First, impacts are strongly concentrated around the specific annual thresholds for PROMISE renewal, particularly the course load requirements. For example, at the end of the freshman year, PROMISE recipients had earned an extra 1.8 course credits (equivalent to about ½ a college course) but were nearly 25 percentage points more likely to have earned at least 30 credits. Tellingly, the annual impacts virtually disappear in the senior year while students are still receiving PROMISE funds but no longer have the opportunity to renew. Second, impacts are not limited to students with the highest financial need; if anything, some impacts appear smaller for this group. Finally, a comparison of these findings with those from other college incentive programs suggests that the details of incentive design are important and may help explain emerging conflicts in the literature.

In Section II, I draw upon theory and previous empirical work to explain how large financial incentive programs might influence student behavior. Section III describes the PROMISE scholarship and the dataset in detail. Section IV presents the empirical strategy and main results, including a bounding of the bias due to selection. Section V analyzes whether PROMISE works primarily by reducing the cost of college or by providing specific incentives for achievement. Section VI discusses the results and implications for future research.

II. Potential Student Responses to Large Financial Incentives

Large financial incentive programs such as PROMISE may affect outcomes among eligible college enrollees via two mechanisms: income effects and incentive effects.[10] The PROMISE scholarship is a high-value award, worth an average of approximately $10,000 over four years for those who initially qualify.[11] A large body of research has demonstrated that lowering the cost of college can increase initial college enrollment rates (see Dynarski 2002 for a review of this literature), and it requires no great leap of logic to imagine that lowering costs might increase persistence and completion rates among enrollees too. Even if the renewal requirements were eliminated, PROMISE might induce some students to enroll full-time rather than part-time, or to attend for more semesters than they would have otherwise.[12]

Even among students already planning to attend full-time, lowering the cost of college may affect outcomes by reducing student employment and enabling students to spend more time on their coursework. Simulations by Keane and Wolpin (2001) suggest that credit constraints primarily affect student employment rather than college enrollment decisions. Scott-Clayton (2007) shows that today's undergraduates spend twice as much time working for pay as similar students a generation ago, and provides suggestive evidence that the increase is related to the rise in college costs. If PROMISE induces

[10] I use the term income effects to refer primarily to the relaxation of binding financial constraints via reduction of college costs. Even unconstrained individuals may choose to purchase more education when they feel richer (another income effect) or when college is cheaper (a price effect), but even this scholarship is relatively small in comparison to lifetime income or even of the cost of attendance including foregone wages.

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

[12] Note that programs that increase enrollment may *appear* to harm persistence and completion among enrollees due to compositional changes in the student population. Under the college-as-experimentation framework (Manski 1989), students induced by financial aid to enroll in the first place may be more likely to realize they are not "college material" and drop out regardless of the potential for additional aid. Other than these compositional effects, it is unlikely that lowering the cost of college would harm outcomes among college enrollees (though not impossible: under Ben-Porath's 1967 model of human capital investment, time-to-degree could rise when tuition falls, as some students purchase more semesters of schooling but spend less time on school within each semester).

students to work less and study more, it may enable them to raise their GPAs and accelerate their progress towards a degree.

But PROMISE doesn't just lower the cost of college. It also gives college students incentives to meet key near-term goals along the path to graduation. Why should this external source of motivation matter? The prospect of graduating, earning a college graduate's full-time salary and escaping additional years of tuition, homework and exams might seem to offer incentive enough. But some students may simply prefer to experience college at a more leisurely pace—particularly if someone else is footing the bill. State-subsidized tuition and parent support mean that few students pay the full cost of an additional year of schooling. PROMISE may bring students' preferences more in line with those of their parents (and the state), especially for students near the renewal thresholds for whom the costs of meeting the requirements are lowest.

The effects of incentives on student achievement may extend beyond simple cost-benefit calculations. Clearly defined incentives may also help students overcome behavioral quirks that have been well-documented in other contexts (see Sunstein and Thaler 2008 for a recent review). These quirks are evident even when the stakes are high and the decision is purely financial. For example, Madrian and Shea (2001) find that employees' contributions to 401(k) savings plans are disproportionately influenced by the default contribution rate; few individuals actively opt to contribute more or less. Similarly, students may be swayed by default options when deciding on their course load. Although students must average at least 15 credit-hours per semester in order to graduate in four years, 12 credits per semester is the default required to maintain "full-time" status.[13] By requiring students to take a true four-year course load (30 credits per year), PROMISE alters the default course of action.

Like those of us who make a daily commitment to exercise tomorrow, students may exhibit self-control and procrastination problems when faced with the prospect of current pain for theoretical future

[13] This definition of full-time is not particular to West Virginia; it is the standard basis for most institutions' tuition charges as well as for federal financial aid and tax purposes, and possibly even for health insurance eligibility. But since most bachelor's degree programs require at least 120 credits to graduate (many require more), 12 credits per semester will not lead to graduation in less than five years.

gain (DellaVigna and Malmendier 2004; Loewenstein and Thaler 1989; Laibson 1997). Students may promise themselves (and their parents) to work harder "next semester," but their resolve weakens when "next semester" actually arrives. O'Donaghue and Rabin (2001) show that these problems can be worse for high-stakes decisions with many possible courses of action—a fitting description of the college experience. Incentives based on clear, short-term goals and linked to a salient near-term reward may help students overcome procrastination and choice paralysis.

Despite the multiple theoretical mechanisms by which large financial incentives might improve outcomes for college enrollees, the empirical evidence has been mixed, and no previous study has attempted to disentangle the roles of income effects and incentive effects. Dynarski (2008) estimates that large-scale merit-aid programs in Arkansas and Georgia reduced the college dropout rate by 3 to 5 percentage points.[14] But while Georgia's HOPE program increased persistence, it may have lengthened time-to-degree. Georgia HOPE required students to maintain a 3.0 GPA but placed no restrictions on their course load, thus creating an incentive to take fewer courses. Indeed, Cornwell, Lee, and Mustard (2005) find that Georgia HOPE *reduced* the fraction of freshmen at the University of Georgia completing a full course load by 6 percentage points, even though the quality of incoming freshmen increased.

Brock and Richburg-Hayes (2006) find significant effects of financial incentives on low-income parents attending two community colleges in New Orleans. Students randomly assigned to the treatment group could receive up to $2000 over two semesters for maintaining at least a half-time course load and a 2.0 GPA. These students were significantly more likely to enroll full-time, passed more courses, and earned more credits than those in the control group over the first three semesters of follow-up (a planned longer follow-up was cancelled by Hurricane Katrina).

In contrast, Angrist, Lang, and Oreopoulos (forthcoming 2009) find weak effects of a large financial incentive offered to incoming freshmen at a large public Canadian university. Students who

[14] 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.

were randomly assigned to participate in the incentive program, which offered a one-time award up to $5,000 (comparable to a year's tuition) for achieving a target GPA and maintaining a "full-time" course load, did not have higher GPAs or more credits than students in the control group at the end of the year.[15] Students who were offered additional student support services along with the financial incentive improved their GPAs, but only for one year.[16]

The present study contributes to this relatively small literature by analyzing a program similar but not identical to the ones described above. As I will discuss later in the paper, differences in design that may seem negligible to policymakers can have big consequences for student behavior. This study also tries to explicitly disentangle the roles of money and motivation in a college aid program that includes both elements. Understanding not only whether large financial incentives work, but also *how* they work, has implications not only for financial aid policy but also for our understanding of economic behavior more broadly.

III. West Virginia's PROMISE Scholarship

West Virginia (WV) has consistently ranked last among the fifty states in the percentage of adults 25 years and older with a Bachelor's degree, with a rate of 14.8 percent in 2000. This is driven by a low rate of BA attainment among individuals born in WV (15.9 percent among adults 25 and older), as well as a net loss of college graduates through migration. 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. Table 1 provides a list of public and

[15] As in the U.S., the definition of "full-time" at this Canadian university would not lead to graduation in four years.
[16] The effects the authors do find are also concentrated among women.

private colleges in West Virginia. Full-time enrollment is defined as a minimum of 12 credit-hours per semester.[17]

Eligibility for the scholarship is based entirely on a student's academic record. Family income plays no role. 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.[18] 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.[19] Those who fail to meet renewal requirements once cannot later regain the scholarship (see Appendix A for additional program details, including recent rule changes).

PROMISE recipients are not the academic elite, but neither are they average students. West Virginia estimates that approximately 23 percent of their high school graduates meet the initial eligibility requirements.[20] About 40 percent of young, in-state first-time freshmen at West Virginia public institutions qualify.[21] Table 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 the implementation of PROMISE 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).[22]

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[17] 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.

[18] 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.

[19] Students who enroll full-time, but do not earn 30 credits, will lose the scholarship for the following year. Students who fall below full-time status will lose funding immediately.

[20] Phone conversation with Jack Toney, Director of State Financial Aid Programs, April 17, 2008.

[21] Author's calculations based on college entrants age 19 or younger.

[22] 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.

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).

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 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.[23]

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. The data include limited background information such as age, race, gender, overall high school GPA, and ACT and SAT scores if applicable.[24] No direct measure of family income or wealth is available for the full sample. The data include complete college transcripts and financial aid records. 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.[25]

[23] This average includes students who failed to renew the scholarship for all four years.
[24] 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.
[25] 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).

Permission to utilize these data, stripped of all personal identifiers, was provided by WVHEPC under a restricted-use data agreement. The present analysis utilizes data on 44,133 first-time freshmen, age 19 or younger, entering any public WV two- or four-year college in the two years before PROMISE (2000-01 and 2001-02) and two years after (2002-03 and 2003-04). Academic records are available through the 2007-08 school year. This enables me to look at up to five years of academic outcomes for all cohorts. The employment data are more limited, covering calendar years 2002 through 2006, with some data for 2007. I will focus on employment outcomes that are available for all cohorts (see Appendix A for additional information about the data).

IV. Impacts on College Persistence, Performance and Completion

In this section I estimate the effect of West Virginia's PROMISE scholarship on college outcomes among PROMISE-eligible college enrollees. 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 analysis based on the discontinuous timing of program implementation. Figures 1a and 1b illustrate the identifying variation in the two approaches. Before describing each in detail, I first explain how they fit together and how I address their shared weakness.

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 limitation of the RD is 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.

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, if credible, are more useful than the RD findings.[26] 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.[27] 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.[28]

I address these concerns explicitly with a bounding exercise after presenting the main results. In this exercise, I estimate the fraction of eligible enrollees who are marginal (i.e. induced to become an eligible enrollee because of PROMISE), and then test the sensitivity of the main results to different assumptions about the characteristics of these marginal students. For the moment, 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. Compositional change is only a concern to the

[26] For this reason, once the main results are presented, all secondary and subgroup analyses will rely on the cohort approach.

[27] 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). 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.

[28] 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.

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 (2002 and 2003 entrants), who earned at least a 3.0 high school GPA. 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 20.49 do not.[29] 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. Before describing the regression estimation, I display the visual evidence for discontinuities. Figure 2 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. Above the threshold, the discrepancy 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.[30] My preferred regressions (described below) will follow a "fuzzy" RD approach to adjust estimates for the discrepancy between apparent eligibility and actual PROMISE receipt.

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[29] 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 scale.

[30] 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.

Figure 3 shows the means of selected background characteristics by ACT score, along with linear predictions estimated separately on either side of the threshold. One hopes *not* to see discontinuities here, since identification relies on the comparability of students just above and below the threshold. It is reassuring that none are found for the few covariates I can examine: high school GPA, age at college entry, percent female, and average Pell grant size.[31] The very smooth plot of high school GPA in particular should help ameliorate concerns about selection.

Finally, Figures 4 and 5 shows the raw means of end-of-college outcomes by ACT score, along with linear predictions. Figure 4 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.[32] The four panels of Figure 5, however, show clear and large 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. I now proceed to a formal analysis of the variation shown in these graphs. Following Imbens and Lemieux (2008), I use a local linear regression specification:

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

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.[33] The parameter β estimates the

[31] The average Pell Grant is a contemporaneous measure rather than a background characteristic, but it is shown as rough indicator of family income. Pell Grants are not explicitly endogenous to PROMISE receipt (neither program reduces awards because of the other), but neither are they fully exogenous since PROMISE requires students to apply for federal aid.

[32] 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.

[33] Card and Lee (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, neither clustering nor running the regressions on cell means has much effect on the standard errors; for most outcomes, the unclustered standard errors are larger.

difference in outcomes at the threshold. Intuitively, the equation above approximates the prediction lines shown in Figures 3 and 4, 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:

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

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

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 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.[34]

As described above, it is clear that I do not perfectly measure true eligibility for PROMISE. It is also implausible that much of the discrepancy 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

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

publicized, and simple to understand, so lack of awareness is an unlikely explanation.[35] 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 receive funding in the spring term.[36] For these reasons, I focus on the fuzzy RD results.

Results. The results are shown in Table 3. To provide context, column (1) shows the mean outcome levels for students just below the ACT threshold. Confirming the graphical analysis, PROMISE receipt has no significant impact on the number of semesters of enrollment (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 enrollment, the direct marginal cost of taking additional courses is zero for most students with or without PROMISE.[37] Yet the program appears to have a moderate impact on total course credits earned and cumulative GPA after four years: total credits increase by 4.6 (from a baseline of 86) while cumulative GPA increases by 0.10, or about one-tenth of a letter grade (from a baseline of 2.68).

These effects on average measures of college achievement pale in comparison to the 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, which is four times the 30-credit annual requirement of PROMISE (and generally a minimum requirement for a BA).[38] They were also 9 percentage points more likely to have a 3.0 cumulative GPA. These represent increases of 33 and 25 percent, respectively.

Finally, PROMISE generates large and statistically 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

[35] 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).

[36] 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.

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

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

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.[39] It is notable that these large impacts are achieved despite the fact that students near the ACT cutoff had below-average PROMISE renewal rates. Only about 35 percent retained the scholarship for all four years, meaning that the total value of the scholarship averaged only $8,338 for this group, compared to about $10,000 among all recipients.

Robustness checks. Table 4 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.[40] 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). The theoretically optimal bandwidth depends on the density, variance, and curvature of the data near the threshold; however, there is currently no consensus on how to determine the optimal bandwidth (Imbens and Lemieux 2008).[41] I opt for transparency and simply show the impact estimates for alternative bandwidths. Column (3) shows the results with a narrow bandwidth, including only students scoring between 18 and 23 on the ACT, while column (4) shows a wide bandwidth, including students scoring between 11 and 30 on the ACT. As one might have predicted from the graphical analysis, the estimates fluctuate very little.

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[39] 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.

[40] 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.

[41] Optimal bandwidth will be smaller when there is higher density around the cutoff, lower variance in outcome variable around the cutoff, and higher curvature around cutoff (Imbens and Lemieux 2007). Cross-validation procedures for calculating the optimal bandwidth may themselves be sensitive to the initial bandwidth and kernel.

Second, I test whether the results are sensitive to the choice of functional form. The baseline estimates assume a linear relationship between ACT score and a given outcome (estimated separately on either side of the threshold), which is supported by the graphical analysis but worthwhile to test. In column (5) of Table 4, I estimate a two-stage model identical to (2a) and (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.[42] Since none of these students received PROMISE, the RD should estimate no effects for this group. Column (6) of Table 4 shows the results; indeed, no impacts are found.

Limitations. Figure 6, which shows the distribution of ACT scores before and after PROMISE, provides some cause for concern that those just above the threshold may not be truly comparable to those just below it. After PROMISE, there is a clear spike in the number of students scoring at or above the cutoff score, and a slight dip in the number of students scoring just below. While it is reassuring that there are no discontinuities in observable characteristics around the threshold, it is impossible to rule out the possibility of differences along unobservable dimensions. As discussed above, compositional bias is a concern with both the RD and the time series analysis. But is potentially more problematic and difficult to bound in the RD. If some students who would have just missed the ACT cutoff now work to just make it, this has the greatest consequences for comparisons of means just above and below the threshold. This provides an additional reason to prefer the broader-based estimates from the cohort analysis.

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. I then compare their college

[42] Since none of these students received PROMISE, it is impossible to estimate a fuzzy RD for this group.

outcomes over the four years following initial enrollment. The identifying assumption is that the pre-PROMISE cohorts provide a good prediction of what would have happened to PROMISE cohorts in the absence of the program.

Graphical analysis. Figures 7 and 8 show the raw means of key outcomes by entry cohort, and generally reinforce the pattern of results from the RD. Discontinuous increases in college outcomes (a decrease in the case of school-year earnings) are clearly visible beginning with the 2002 cohort. The fact that outcomes shift sharply between 2001 and 2002, rather than steadily changing over time, lessens the concern that before-after differences simply reflect broad underlying trends. Figure 9 presents similar graphs of covariates, including average high school GPA and ACT scores. There is no indication of significant changes along these dimensions after the implementation of PROMISE.

Estimation. A regression framework enables me to adjust these raw differences for any observable changes in sample composition. The basic OLS specification estimates:

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

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, and ε_{it} is an idiosyncratic error term.[43, 44]

Equation (3) estimates the effects of predicted PROMISE eligibility, not the effects of actual PROMISE receipt. Because eligibility status is imperfectly measured (as discussed above), only 86 percent of apparently eligible enrollees in the PROMISE cohorts actually receive PROMISE funds. An

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[43] 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.

[44] It could be argued that because treatment varies within this sample only by cohort rather than individual, I should cluster the standard errors by cohort. But with so few cohorts, it is not really possible to identify common shocks for a cluster separate from the common shock of PROMISE implementation. In any case, my estimates of standard errors change little whether I allow for clustered errors in individual-level regressions, or collapse the data to cohort means and run regressions where N=4. For most outcomes, this tends to marginally decrease rather than increase the estimated standard errors.

IV specification can estimate the causal effect of actual PROMISE receipt, using $after_t$ as the plausibly exogenous instrument.[45] I estimate the two-stage model:

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

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

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 may 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 5 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) or cumulative GPA (a 0.04 increase, from a baseline of 2.98). Effects on total credits and school-year earnings are slightly larger in percentage terms. But as in the RD, the time series analysis suggests disproportionately large effects on the percentage of students who had earned at least 120 credits after four years (11 percentage points, from a baseline of 43 percent) and on four-year BA completion rates (a 6.7 percentage point impact, from a baseline of just 27 percent). As in the RD, some of the BA completion impact attenuates over time, leaving a still highly significant impact of 3.7 percentage points after five years (from a baseline of 51 percent).

[45] 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.

Comparison with RD results. The cohort analysis results are broadly similar to those of the RD, but with some differences. The cohort analysis suggests slightly larger impacts on total credits and school-year earnings, but slightly smaller impacts on cumulative GPA and BA completion. These differences could result from legitimate variation in treatment effects between the average recipient and those near the ACT threshold, or they could result from unaddressed biases in one or both approaches. To test this, I limit the sample to students near the threshold and test whether the cohort analysis can replicate the RD results. Table 6 shows that it can. Column (2) presents before-after results for students with an ACT score of 21, and column (3) presents difference-in-difference estimates that compare changes among the ACT=21 group with changes among the ACT=20 group, who were ineligible for the program. Both columns generally confirm the RD results, and suggest that key impacts for the threshold group may be slightly larger than the average impacts for all recipients (again, despite the fact that the threshold group received significantly less PROMISE funding over four years).

Robustness checks. I first test whether the cohort analysis is robust to controlling for students' high school type (public/private status) and 55 indicators for county of residence at entry. The results are presented in column (2) of Table 7, and are virtually identical to the baseline estimates in column (1).

I next test whether the findings from the cohort analysis are robust to the inclusion of a comparison group. 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, increased other scholarship opportunities for out-of-state students during the sample period in an explicit attempt to increase out-of-state enrollments.[46] If these other scholarships attracted higher-quality out-of-state

[46] 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.

students over time, or had impacts of their own, this is likely to work against finding positive impacts of PROMISE.

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

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

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

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 by about 18 percent.[47]

Column (3) of Table 7 presents the 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 (3) and (1) are almost all too small to be of any substantive importance (with school-year earnings being the exception).

As a further robustness check, I explicitly control for linear time trends in both the baseline and the difference-in-difference specification.[48] This would be the preferred specification with a longer time series, but with only four cohorts it is more appropriate as a sensitivity test. Results are presented in columns (4) and (5) of Table 7. In general, the estimated effects increase in magnitude. What is particularly reassuring is that when linear trends are included, adding the non-WV resident comparison group makes little difference. This is consistent with evidence that non-WV residents are increasing in quality over time, but there is no jump in their performance at the time of PROMISE implementation.

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 on these results.

[47] 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.
[48] In the DD, the time trend is allowed to vary by state of residence.

Bounding the effects of compositional change

PROMISE appears to have had a substantial impact. But given that an explicit goal of the program was to increase in-state enrollment among qualified students, it is fair to ask whether the results above could be biased by compositional changes.[49] To understand how compositional changes 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:

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

where Z_i is equal to 1 if the student was induced to become an eligible enrollee because of PROMISE, and zero otherwise.[50] 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 (λ>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:

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

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

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[49] 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.
[50] This analysis follows a similar framework presented by Jonathan Guryan (2004) in a comment to Bettinger (2004).

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 10 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.[51] The figure indicates that the percent of WV high school graduates enrolling in public WV institutions as PROMISE-eligible students jumped by 4 to 5 percentage points between 2001 and 2002.[52] This suggests that about 20 to 25 percent of eligible enrollees after PROMISE are marginal. The other 75 to 80 percent likely would have met the initial requirements and enrolled in a public WV institution with or without the scholarship.[53]

Figure 10 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.

[51] 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, 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.

[52] 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.

[53] 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.

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.

Table 8 presents results from the bounding exercise. Column (1) restates the baseline estimates from Table 5. Column (5) calculates the mean outcome levels among marginal enrollees that would be required to produce the baseline impacts restated in column (1), assuming marginals represent 25% of all eligible enrollees.[54]

These outcome levels are placed in context by actual mean outcomes for three groups prior to PROMISE: all eligible enrollees (column 2), enrollees who just missed the ACT score cutoff (column 3) and the top 10% of eligible enrollees as measured by ACT scores (column 4). The results indicate that to completely account for the impacts, marginal students would have to have college achievement levels at—or in many cases implausibly higher than—the top 10% of eligible enrollees prior to PROMISE.

Columns (6) through (8) provide adjusted impact estimates under different assumptions about marginals.[55] Column (6) assumes that marginal enrollees perform like those who would have scored a 19 or 20 on the ACT prior to PROMISE, which is plausible if some students simply retake the ACT until they score a 21, or if PROMISE attracts individuals who would not have attended college otherwise. If this were the case, the true impacts of PROMISE would be about 50 percent larger than estimated in Table 5. Column (7) assumes that marginal students are truly the "cream of the crop," with outcomes comparable to those scoring 29 or higher on the ACT (at the 94th percentile, nationally). Under this extreme assumption, many impacts shrink substantially, but the four-year BA completion impact survives. Finally, column (8) assumes a reasonable 60/40 mix of these relatively low- and high-achieving

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[54] The required mean difference is obtained by dividing an impact estimate by 0.25, and then the required mean level among marginals is obtained by adding this difference to the mean level among all enrollees (i.e., column5=column1/0.25+column2).

[55] The adjusted impact estimates in columns (6) through (8) of Table 6 are based on different mixtures of low-achieving and high-achieving students as described in columns (3) and (4). For example, column (8) is calculated as: [column1]+[(0.25*0.6)*(column2-column3)]+[(0.25*0.4)*(column2-column4)].

marginal students. The results indicate that if anything, compositional change may bias estimated impacts downward rather than upward.

<u>*Subgroup Analyses*</u>

Estimates of the effect on key outcomes by gender and entry institution, as well as additional outcomes for the full sample, can be found in Appendix B (Tables B1, B2, and B3). To summarize, the pattern of findings is very similar for both men and women. Effects are largest for students who entered West Virginia University or Marshall University, but generally go in the same direction among those who entered other schools (these results should be interpreted cautiously, since PROMISE affects the choice of institution, but they at least suggest that effects occur within institutions rather than resulting from changes in institutional context).

V. Inside the Black Box: Are Impacts Driven by Income or Incentive Effects?

Would PROMISE have had similar consequences if it had been designed as a traditional grant with no strings attached, rather than as an incentive for college achievement? In this section, I present evidence that income effects (i.e. relaxation of financial constraints) are *not* sufficient to explain the pattern of impacts observed. First, I show that impacts are concentrated around the specific annual requirements for PROMISE renewal. Credits (and to a lesser extent GPAs) bunch just above the renewal thresholds in the first three years of college, but not in the fourth year when students are still receiving PROMISE funds but no longer face renewal incentives. Second, I show that the impacts are not limited to students with high financial need; if anything, some impacts are smaller for this group. Finally, I compare these findings to those of other college grant programs and argue that small differences in the particular design of incentives may help resolving emerging conflicts in the literature.

<u>*Are Impacts Concentrated Around Annual Renewal Thresholds?*</u>

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 11 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.[56] The kink demonstrates the shift from just below to just above 30 credits.

Figure 12 presents CDFs for college GPAs. GPAs are clearly higher for the PROMISE cohorts in the first three years. 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.

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.[57] 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 9, I quantify the differences shown in these figures. I estimate impacts on the percentage meeting the renewal thresholds in each year, using the time series OLS specification as well as an IV approach to account for declining PROMISE receipt in each year of college.[58] The results show that PROMISE recipients are 20 to 25 percentage points more likely to attempt 30 or more credits in each of

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[56] 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.

[57] 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.

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

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.

Perhaps 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 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 would not 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.[59] Because family income data are not available, I use a binary indicator of federal Pell Grant eligibility as a rough proxy for financial need.[60] 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.

Table 10 presents the cohort analysis (IV) results for key college outcomes by Pell status, along with baseline subgroup means. In most cases the impacts are very similar between the two groups. The exceptions are on the cumulative GPA measures and five-year BA completion, for which Pell recipients show smaller or even zero impacts. 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

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[59] 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).

[60] 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).

significantly constrained even with PROMISE. Still, the finding that impacts are not concentrated among the neediest students is suggestive that income effects are not the sole mechanism driving the results.

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

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.[61]

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 graduation rates

[61] 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.

under PROMISE. It is thus possible that specific achievement incentives matter more for *how* students complete college rather than *whether* they complete college.

VI. Discussion

I find that PROMISE has a significant impact on many end-of-college outcomes, with particularly large impacts on time-to-degree. Five-year BA completion rates for recipients rose by nearly 4 percentage points while four-year BA completion rates rose by nearly 7 percentage points (a 25 percent increase). Including its 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 from a baseline rate of just 21.5 percent.[62, 63] Under reasonable assumptions about the returns to schooling, PROMISE easily passes a social cost-benefit analysis, with up to 25 percent of net social benefits due to the improvements in time-to-degree (see Appendix C for details of this analysis).

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 would not likely 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.

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. Other research highlights the importance of other factors. For example, Bettinger and Long (forthcoming) find large effects of math and English remediation on college persistence and completion, and Tinto's (1994) model of student retention focuses on college students' integration with social and academic communities. Still,

[62] 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.
[63] Baseline BA attainment rate is from ACS 2005, based on WV residents aged 25-34, corresponding to the age cohorts just prior to PROMISE.

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.

While the income effects of PROMISE are insufficient to explain its impact, this hardly implies that the value of the award is irrelevant. How students would respond to a lower-value award remains an open question. To the extent that students' responses to incentives are based on purely financial calculations, smaller financial incentives are likely to generate smaller results. To the extent that other psychological factors are at work, the relationship between the size of the incentive and the effect may be less clear. These questions are beyond the scope of the present study, but the following issues invite further investigation.

Note that first-year PROMISE recipients deciding whether to exert additional effort to meet the annual renewal requirements (particularly the 30-credit minimum) already face substantial financial incentives to graduate in four years: the present value of entering the labor force one year earlier plus saving a fifth year of tuition is about $40,000. PROMISE increases this financial incentive by about $9,000.[64] Under a purely financial calculation, PROMISE would thus induce students to graduate in four years only if $40,000 is insufficient to elicit the additional effort required, but $49,000 is enough. At least 7 percent of PROMISE recipients would need to have valuations in this rather narrow range to explain the four-year completion effect.

A number of psychological factors might reinforce students' responses to financial incentives. First, in some cases a dollar may be worth more than a dollar. Marketing research suggests that individuals attach extra value to "free" products (Shampanier, Mazar, and Ariely 2007). If this is true, the offer of free tuition and fees may have a unique salience beyond the value of the award; an implication is that reducing the award to 90 percent of tuition could have disproportionately large consequences. Avery and Hoxby (2004) also find that students' college choices are influenced more strongly by "scholarships" than "grants" of the same amount, suggesting that the prestige of the award may also matter.

[64] This is the average remaining value of PROMISE for students who renew after the first year.

Second, even relatively small incentives may help students overcome procrastination problems by providing immediate rewards for meeting concrete goals. Students may fully intend to graduate in four years, but each semester find it more appealing to take the minimum "full-time" course load. This hypothesis of a disconnect between intention and execution is supported by expectations data from a national survey of college freshmen, which found that nearly all respondents pursuing a BA degree (95 percent) expected to earn a BA within four years, even though only 35 percent actually did.[65]

Third, establishing clear incentives for a large group, rather than for only a few individuals, may increase the salience of the award and alter social norms on campus. With about 40 percent of young WV freshmen entering as PROMISE scholars, this gives the program a substantial advantage in terms of breaking through the din and "changing the kitchen table conversation," as stated by Brian Noland, Chancellor of Higher Education in West Virginia.[66]

Finally, it is important to note that income 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 income effects of PROMISE. The scholarship also reduced student loan debt, which could affect post-graduation decisions (Field forthcoming 2009; Rothstein and Rouse 2008).

To conclude, while the monetary value of PROMISE may be important, this study suggests that income effects alone cannot explain the pattern of findings. Incentives matter—particularly the incentive to complete a true "four-year" course load (that is, 30 credits) each year. A better understanding of *why* students responded to the incentives in PROMISE is an intriguing and important topic for future research.

[65] Authors calculations from the Beginning Postsecondary Students: 1996-2001 dataset. This hypothesis is also supported by anecdotal reports from officials in West Virginia (conversation with Larry Ponder of WVHEPC, July 2008).
[66] Phone conversation, March 13, 2008.

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List of Appendices

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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.

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.

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 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 B: Subgroup Analysis and Additional Outcomes

Table B.1
Cohort Analysis for Key Outcomes by Gender

	Women				Men			
	(1) Pre-	(2) IV Before/After			(3) Pre-	(4) IV Before/After		
Outcome	Mean	B		(SE)	Mean	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	***	($9)	$0	$3,121	***	($10)
Total PROMISE received (over 4 years)	$0	$10,353	***	($85)	$0	$9,766	***	($100)
Number of semesters enrolled (over 4 years)	6.704	0.175	***	(0.053)	6.766	0.105	*	(0.063)
Total credits earned (over 4 years)	98.853	5.355	***	(1.138)	95.084	6.297	***	(1.324)
Cumulative GPA (over 4 years) [a]	3.085	0.021		(0.018)	2.847	0.063	***	(0.024)
Typical weekly school-year earnings [b]	$88.58	-$10.02	***	($2.76)	$81.48	-$9.04	***	($3.47)
Earned 120 credits by end of Year 4	0.464	0.106	***	(0.013)	0.388	0.117	***	(0.015)
Had 3.0+ cumulative GPA at end of Year 4	0.653	0.033	***	(0.012)	0.506	0.037	**	(0.015)
Earned BA within 4 Years	0.304	0.067	***	(0.012)	0.218	0.068	***	(0.013)
Earned BA within 5 Years	0.535	0.031	**	(0.013)	0.475	0.044	***	(0.016)
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: 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.

Table B2
Cohort Analysis for Key Outcomes by Entry Institution

	WVU Entrants				Marshall Entrants				All Other Entrants			
	(1) Pre-	(2) IV Before/After			(3) Pre-	(4) IV Before/After			(5) Pre-	(6) IV Before/After		
Outcome	Mean	B		(SE)	Mean	B		(SE)	Mean	B		(SE)
Received PROMISE	0.000	1.000	***	(0.000)	0.000	1.000	***	(0.000)	0.000	1.000	***	(0.000)
Value of PROMISE in Year 1	$0	$3,343	***	($7)	$0	$3,053	***	($10)	$0	$2,739	***	($14)
Total PROMISE received (over 4 years)	$0	$11,387	***	($99)	$0	$9,981	***	($129)	$0	$8,468	***	($108)
Number of semesters enrolled (over 4 years)	7.153	0.109	**	(0.051)	6.717	0.212	**	(0.083)	6.322	0.073		(0.082)
Total credits earned (over 4 years)	106.0	5.048	***	(1.240)	94.9	10.178	***	(1.689)	89.9	2.342		(1.610)
Cumulative GPA (over 4 years) [a]	3.033	0.069	***	(0.021)	2.990	0.023		(0.028)	2.928	0.006		(0.029)
Typical weekly school-year earnings [b]	$63.07	-$10.26	***	($2.45)	$87.49	-$6.43		($4.16)	$106.48	-$6.20		($4.69)
Earned 120 credits by end of Year 4	0.537	0.085	***	(0.015)	0.378	0.192	***	(0.019)	0.359	0.070	***	(0.017)
Had 3.0+ cumulative GPA at end of Year 4	0.614	0.049	***	(0.013)	0.589	0.025		(0.018)	0.566	0.020		(0.017)
Earned BA within 4 Years	0.333	0.080	***	(0.014)	0.221	0.084	***	(0.017)	0.231	0.029	*	(0.015)
Earned BA within 5 Years	0.619	0.023		(0.015)	0.488	0.050	**	(0.020)	0.413	0.027		(0.017)
Sample size		5,106				3,027				4,778		

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: 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 B3
RD and Cohort Analysis Estimates of PROMISE Impact on Additional Outcomes

Outcome	(1) Means, ACT=20	(2) Fuzzy RD B		(2) Fuzzy RD (SE)	(3) Pre-means, all eligibles	(4) Cohort Analysis B		(4) Cohort Analysis (SE)
Entered a four-year college	0.771	0.016		(0.024)	0.880	0.017	***	(0.006)
Entered West Virginia University	0.238	0.057	**	(0.025)	0.379	0.041	***	(0.010)
Entered Marshall University	0.215	-0.028		(0.024)	0.238	-0.005		(0.009)
Entered another two/four year college	0.547	-0.029		(0.029)	0.383	-0.036	***	(0.010)
Enrolled full-time, full year in first year	0.899	0.036	**	(0.016)	0.932	0.024	***	(0.005)
Continuously enrolled FTFY, two years	0.735	0.021		(0.025)	0.793	0.040	***	(0.008)
Continuously enrolled FTFY, three years	0.611	0.015		(0.028)	0.693	0.042	***	(0.009)
Continuously enrolled FTFY, four years	0.502	0.032		(0.029)	0.603	0.039	***	(0.010)
Annual GPA in first year [a]	2.523	0.156	***	(0.051)	2.849	0.077	***	(0.017)
Annual GPA in second year	2.498	0.079		(0.056)	2.796	0.088	***	(0.019)
Annual GPA in third year	2.537	0.083		(0.056)	2.846	0.045	**	(0.019)
Annual GPA in fourth year	2.624	0.056		(0.057)	2.923	-0.018		(0.019)
Credits earned in first year	24.529	2.095	***	(0.461)	26.239	1.830	***	(0.150)
Credits earned in second year	21.111	0.688		(0.668)	23.384	1.409	***	(0.223)
Credits earned in third year	18.631	0.790		(0.746)	21.654	1.070	***	(0.252)
Credits earned in fourth year	17.190	0.298		(0.781)	19.890	0.465	*	(0.264)
Summer credits (over three summers)	3.035	0.927	***	(0.284)	3.457	0.805	***	(0.107)
Earned BA within 4 Years	0.155	0.094	***	(0.022)	0.267	0.067	***	(0.009)
Earned BA by summer after 4th year	0.177	0.092	***	(0.023)	0.288	0.077	***	(0.009)
Earned BA by fall of 5th year	0.253	0.076	***	(0.025)	0.378	0.063	***	(0.010)
Earned BA within 5 years	0.367	0.045	*	(0.028)	0.509	0.037	***	(0.010)
Earned AA within 2 years	0.019	0.016	*	(0.009)	0.024	-0.002		(0.003)
Earned AA within 5 years	0.119	-0.023		(0.018)	0.086	-0.013	**	(0.006)
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	***	(0.005)
Earned a BA in math/science w/in 5 years	0.035	0.001		(0.011)	0.108	0.008		(0.006)
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	***	(0.008)
Federal student loans, first year (2007$)	$1,132	-$425	***	($89)	$1,072	-$298	***	($30)
Cumulative federal student loans (2007$)	$5,968	-$972	**	($407)	$6,007	-$780	***	($148)
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: 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.

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.

Table 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 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.

Table 3
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)**
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)**
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: 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. 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, 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
RD Robustness Checks

Outcome	(1) Baseline: Fuzzy RD, LLR 16<=ACT<=25 B		(SE)	Robustness Checks: (2) Add controls for HSType/CountyFE B		(SE)	Alternate Bandwidths: (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	
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: 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. 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, 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
Cohort Analysis Estimates of the Effect of PROMISE

Outcome	(1) Pre-Mean	(2) After-Before	Before-After with Controls: (3) OLS B		(SE)	Before-After with Controls: (4) IV B		(SE)
Received PROMISE	0.000	0.852	0.859	***	(0.004)	**1.000**	***	**(0.000)**
Value of PROMISE in Year 1	$0	$2,621	$2,643	***	($13)	**$3,077**	***	**($7)**
Total PROMISE received (over 4 years)	$0	$8,598	$8,677	***	($67)	**$10,101**	***	**($65)**
Number of semesters enrolled (over 4 years)	6.731	0.118	0.126	***	(0.035)	**0.146**	***	**(0.041)**
Total credits earned (over 4 years)	97.225	4.668	4.967	***	(0.744)	**5.782**	***	**(0.864)**
Cumulative GPA (over 4 years) [a]	2.982	0.025	0.033	***	(0.013)	**0.039**	***	**(0.015)**
Typical weekly school-year earnings [b]	$85.51	-$7.40	-$8.20	***	($1.86)	**-$9.55**	***	**($2.17)**
Earned 120 credits by end of Year 4	0.431	0.091	0.095	***	(0.008)	**0.111**	***	**(0.010)**
Had 3.0+ cumulative GPA at end of Year 4	0.590	0.025	0.030	***	(0.008)	**0.035**	***	**(0.009)**
Earned BA within 4 Years	0.267	0.054	0.058	***	(0.008)	**0.067**	***	**(0.009)**
Earned BA within 5 Years	0.509	0.029	0.031	***	(0.009)	**0.037**	***	**(0.010)**
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: 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, 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 6
Comparing Cohort Analysis and RD Estimates

	(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	
Outcome	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 ***	($7)	$2,987 ***	($18)	$3,028 ***	($22)	$3,012 ***	($21)
Total PROMISE received (over 4 years)	$10,101 ***	($65)	$8,666 ***	($159)	$8,774 ***	($186)	$8,338 ***	($180)
Number of semesters enrolled (over 4 years)	0.146 ***	(0.041)	0.150	(0.115)	0.097	(0.188)	0.037	(0.130)
Total credits earned (over 4 years)	5.782 ***	(0.864)	4.192 *	(2.295)	3.734	(3.672)	4.644 *	(2.519)
Cumulative GPA (over 4 years) [a]	0.039 ***	(0.015)	0.059	(0.040)	0.089	(0.064)	0.099 **	(0.045)
Typical weekly school-year earnings [b]	-$9.55 ***	($2.17)	$1.79	($6.18)	$5.01	($10.06)	-$2.12	($7.04)
Earned 120 credits by end of Year 4	0.111 ***	(0.010)	0.096 ***	(0.025)	0.093 **	(0.039)	0.095 ***	(0.026)
Had 3.0+ cumulative GPA at end of Year 4	0.035 ***	(0.009)	0.038	(0.025)	0.044	(0.040)	0.090 ***	(0.027)
Earned BA within 4 Years	0.067 ***	(0.009)	0.081 ***	(0.021)	0.091 ***	(0.032)	0.094 ***	(0.022)
Earned BA within 5 Years	0.037 ***	(0.010)	0.033	(0.026)	0.063	(0.041)	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: 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. 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, 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 7
Cohort Analysis Robustness Checks

	(1) IV Before/After, All WV Eligibles			(2) Add controls for HSType/CountyFE			(3) IV Diff-in-diff Inc. Non-WV Elg.			Add Linear Time Trends (4) WV Eligibles			Add Linear Time Trends (5) Inc. Non-WV Elg.		
Outcome	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	***	($7)	$3,076	***	($7)	$3,076	***	($7)	$2,742	***	($15)	$2,739	***	($17)
Total PROMISE received (over 4 years)	$10,101	***	($65)	$10,089	***	($65)	$10,084	***	($71)	$9,381	***	($156)	$9,345	***	($173)
Number of semesters enrolled (over 4 years)	0.146	***	(0.041)	0.138	***	(0.041)	0.102		(0.108)	0.296	***	(0.094)	0.525	**	(0.254)
Total credits earned (over 4 years)	5.782	***	(0.864)	5.544	***	(0.862)	4.045	**	(2.014)	9.511	***	(1.992)	13.859	***	(4.763)
Cumulative GPA (over 4 years) [a]	0.039	***	(0.015)	0.037	**	(0.015)	0.019		(0.033)	0.101	***	(0.034)	0.127	*	(0.077)
Typical weekly school-year earnings [b]	-$9.55	***	($2.17)	-$9.51	***	($2.15)	-$4.47		($2.97)	-$15.48	***	($5.03)	-$9.85		($7.01)
Earned 120 credits by end of Year 4	0.111	***	(0.010)	0.108	***	(0.010)	0.088	***	(0.021)	0.158	***	(0.022)	0.157	***	(0.050)
Had 3.0+ cumulative GPA at end of Year 4	0.035	***	(0.009)	0.034	***	(0.009)	0.018		(0.020)	0.072	***	(0.021)	0.048		(0.047)
Earned BA within 4 Years	0.067	***	(0.009)	0.066	***	(0.009)	0.044	**	(0.020)	0.076	***	(0.021)	0.091	*	(0.047)
Earned BA within 5 Years	0.037	***	(0.010)	0.034	***	(0.010)	0.019		(0.022)	0.069	***	(0.023)	0.109	**	(0.051)
Sample size	12,911			12,911			16,645			12,911			16,645		

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: 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. 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, 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 8
Bounding Exercise Assuming 25% of Recips are Marginals

Outcome	(1) Baseline β estimate	Baseline Means of Different Groups (Prior to PROMISE implementation) (2) All Elg.	(3) ACT19/20	(4) ACT29+	(5) Mean Req. to Make β=0	Adjusted β Estimate, Different Assumptions Abt. 25% Marginals (6) All Low	(7) All High	(8) 60/40 Mix
Number of semesters enrolled (over 4 years)	0.146	6.731	6.231	7.193	7.315	0.271	0.031	0.175
Total credits earned (over 4 years)	5.782	97.225	81.315	111.217	120.351	9.759	2.283	6.769
Cumulative GPA (over 4 years)	0.039	2.982	2.587	3.320	3.137	0.138	-0.046	0.064
Typical weekly school-year earnings	-$9.55	$85.51	$106.08	$59.86	$47.33	-$14.69	-$3.13	-$10.07
Earned 120 credits by end of Year 4	0.111	0.431	0.232	0.591	0.873	0.160	0.071	0.124
Had 3.0+ cumulative GPA at end of Year 4	0.035	0.590	0.347	0.780	0.728	0.095	-0.013	0.052
Earned BA within 4 Years	0.067	0.267	0.105	0.385	0.535	0.108	0.038	0.080
Earned BA within 5 Years	0.037	0.509	0.324	0.629	0.655	0.083	0.007	0.052
Sample size	12,911	5,891	1,055	533				

SOURCE: Author's calculations using WVHEPC and external data sources. See Table 5 and Figure 11 for additional information.
NOTES: Baseline impact estimates are from Table 5, column (4). Baseline means in columns (2) through (4) are based on West Virginia residents, age 19 and younger, who had at least a 3.0 high school GPA and had ACT scores as indicated in the column headers. Column (5) calculates the mean outcomes levels among marginal enrollees that would be required to produce the baseline impacts from column (1), assuming marginals represent 25% of all eligible enrollees. The adjusted impact estimates in columns (6) through (8) are based on different mixtures of relatively lower-achieving and higher-achieving students as described in columns (3) and (4). For example, column (8) is calculated as: [column1]+[(0.25*0.6)*(column2-column3)]+[(0.25*0.4)*(column2-column4)].

Table 9

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 ***	(0.004)	1.000 ***	(0.000)						
Year 2	0.000	0.652 ***	(0.005)			1.000 ***	(0.000)				
Year 3	0.000	0.520 ***	(0.006)					1.000 ***	(0.000)		
Year 4	0.000	0.440 ***	(0.006)							1.000 ***	(0.000)
Completed at least 30 credits in: Year 1	0.409	0.210 ***	(0.008)	0.245 ***	(0.009)						
Year 2	0.383	0.139 ***	(0.008)			0.213 ***	(0.012)				
Year 3	0.360	0.109 ***	(0.008)					0.209 ***	(0.015)		
Year 4	0.296	0.035 ***	(0.008)							0.080 ***	(0.018)
Cumulative 2.75+ GPA, end of Year 1 (a)	0.678	0.051 ***	(0.007)	0.059 ***	(0.009)						
Cumulative 3.0+ GPA, end of Year 2	0.572	0.050 ***	(0.008)			0.077 ***	(0.012)				
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 ***	(0.008)							0.067 ***	(0.018)
Annual 2.75+ GPA, Year 1 (b)	0.633	0.044 ***	(0.008)	0.052 ***	(0.009)						
Annual 3.0+ GPA, Year 2	0.537	0.049 ***	(0.008)			0.075 ***	(0.012)				
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	(0.008)							-0.018	(0.019)
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: 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).

Table 10
Time Series Results for Key Outcomes by Pell Grant Status

	Pell Grant Recipients				Non-Recipients			
	(1) Pre-	(2) Time Series IV			(3) Pre-	(4) Time Series IV		
Outcome	Mean	B		(SE)	Mean	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	***	($13)	$0	$3,116	***	($8)
Total PROMISE received (over 4 years)	$0	$9,022	***	($119)	$0	$10,593	***	($77)
Number of semesters enrolled (over 4 years)	6.449	0.068		(0.081)	6.857	0.187	***	(0.047)
Total credits earned (over 4 years)	90.3	4.435	***	(1.612)	100.3	6.491	***	(1.015)
Cumulative GPA (over 4 years) [a]	2.893	0.005		(0.029)	3.023	0.056	***	(0.017)
Typical weekly school-year earnings [b]	$90.97	-$7.68	*	($4.00)	$83.06	-$10.52	***	($2.58)
Earned 120 credits by end of Year 4	0.363	0.094	***	(0.017)	0.461	0.119	***	(0.012)
Had 3.0+ cumulative GPA at end of Year 4	0.551	0.016		(0.017)	0.607	0.044	***	(0.011)
Earned BA within 4 Years	0.217	0.061	***	(0.015)	0.290	0.070	***	(0.011)
Earned BA within 5 Years	0.443	0.008		(0.018)	0.539	0.051	***	(0.012)
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). 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.

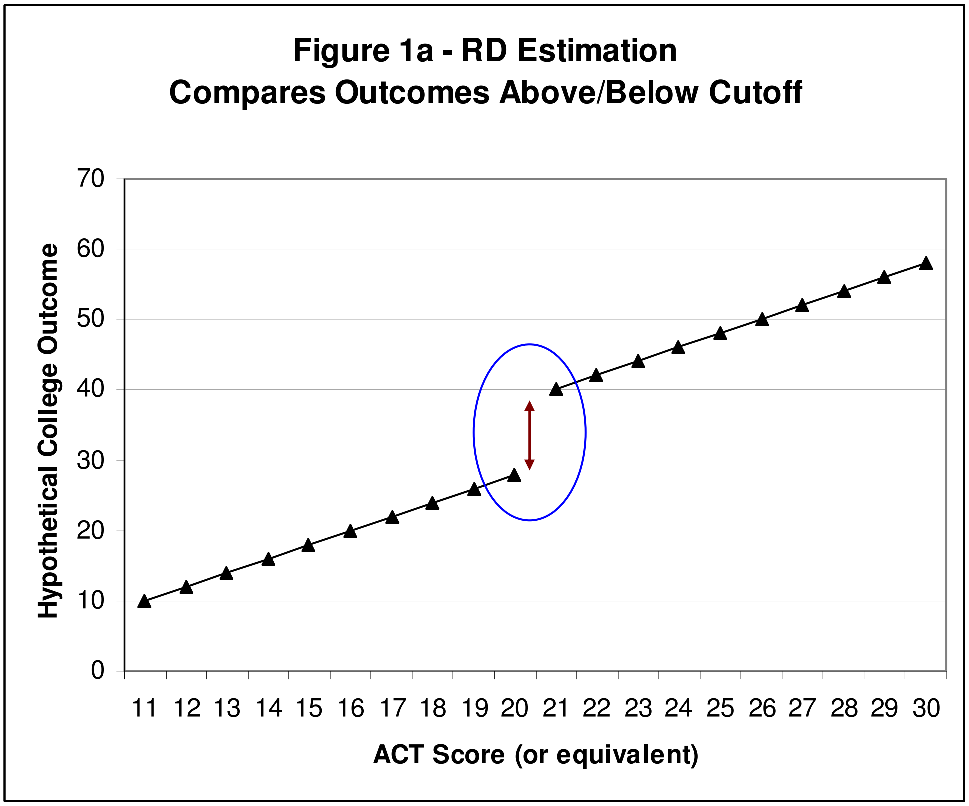


Figure 1a - RD Estimation
Compares Outcomes Above/Below Cutoff
Hypothetical College Outcome
0
10
20
30
40
50
60
70
11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30
ACT Score (or equivalent)

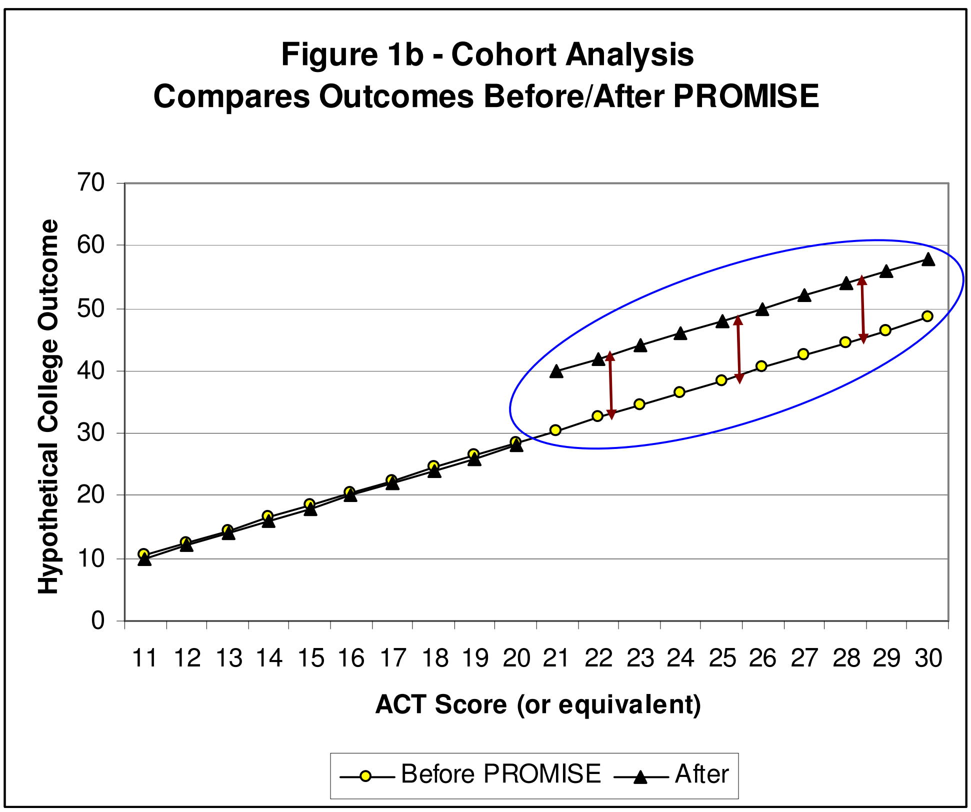
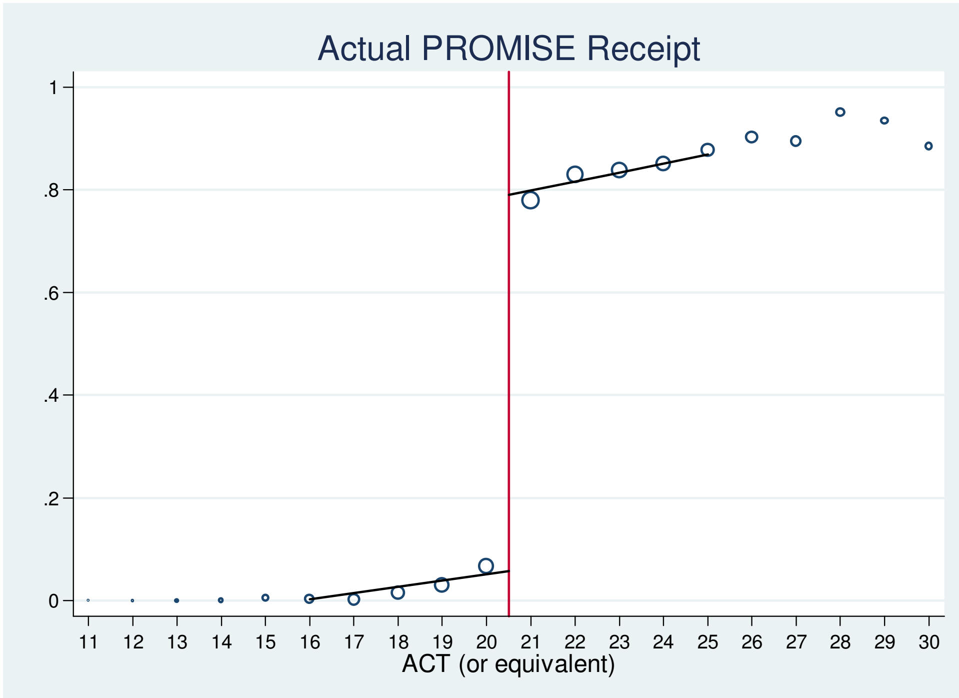


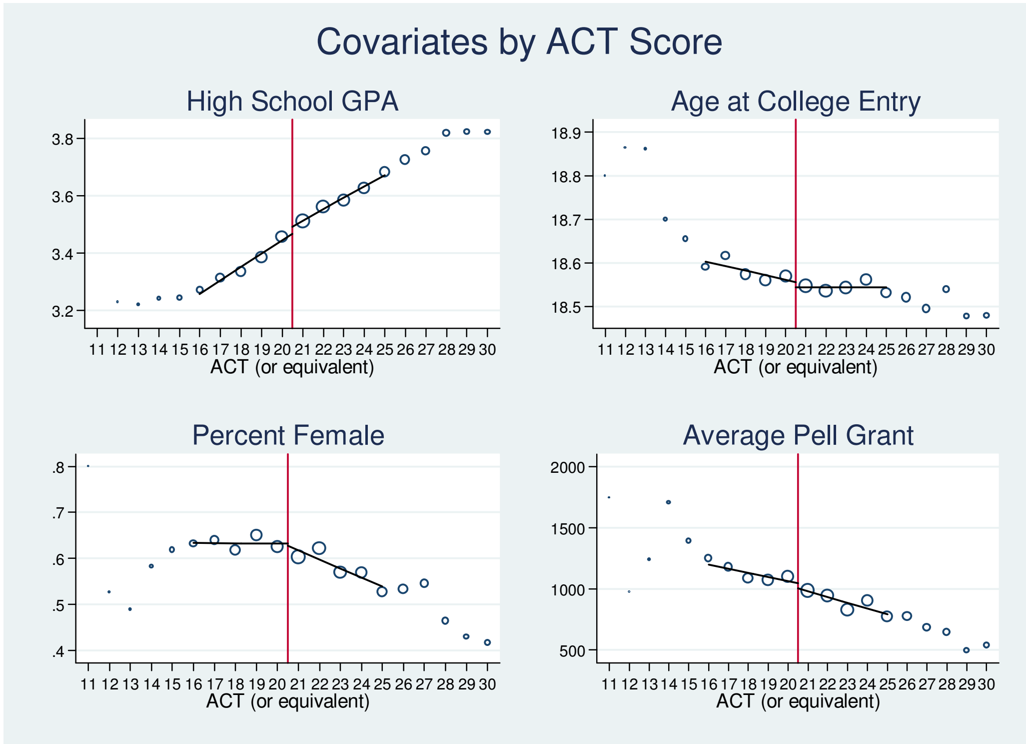
Figure 1b - Cohort Analysis
Compares Outcomes Before/After PROMISE
Hypothetical College Outcome
0
10
20
30
40
50
60
70
11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30
ACT Score (or equivalent)
Before PROMISE
After

Figure 2. Actual PROMISE receipt by ACT score.

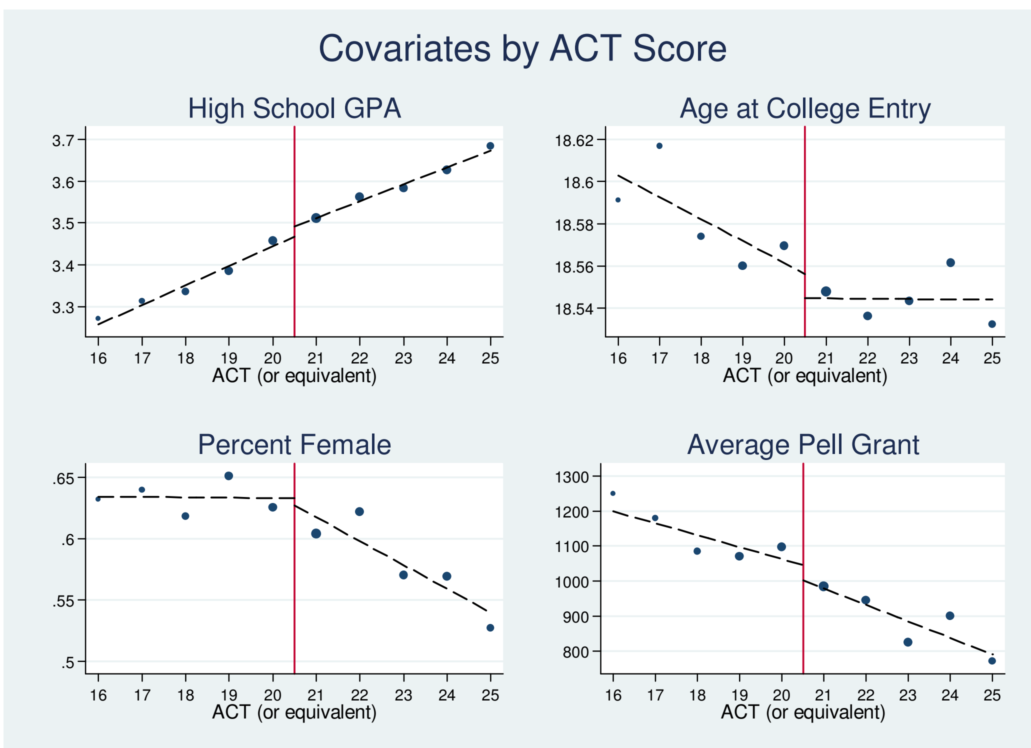


SOURCE: Author's calculations using WVHEPC data on West Virginia resident first-time freshmen entering two- or four-year public WV institutions in fall 2002 or fall 2003, who had at least a 3.0 high school GPA.
NOTES: Each dot indicates the rate of PROMISE receipt for students with a given ACT score, 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 3.

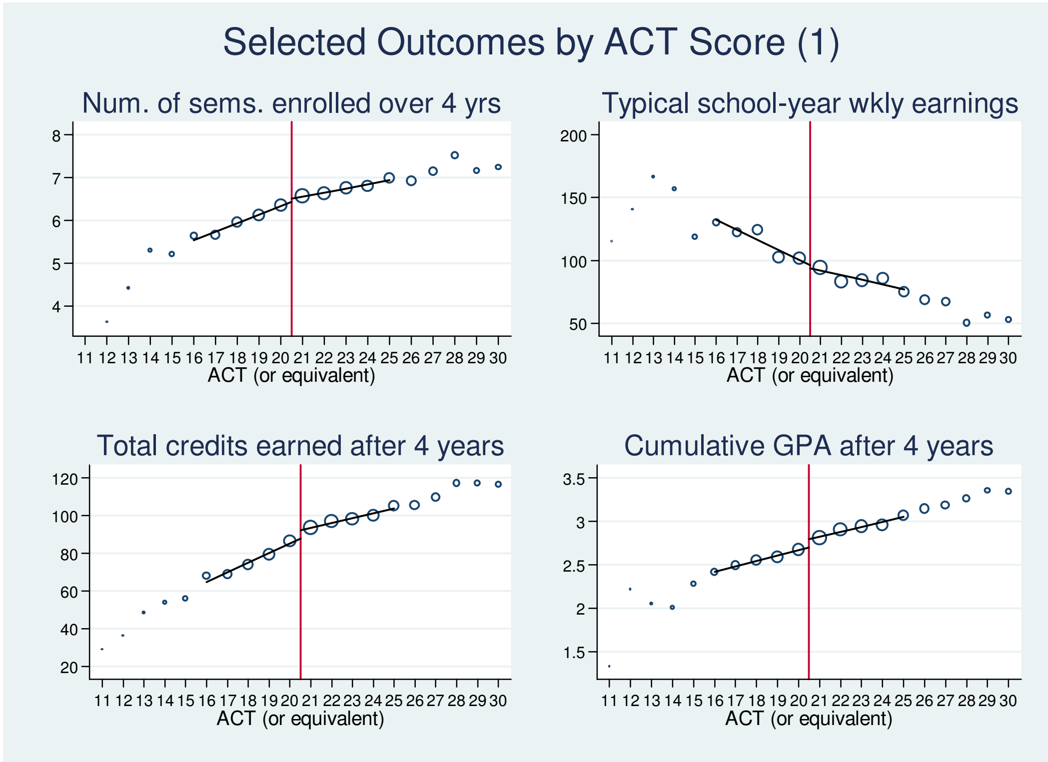


(same graphs, zooming in)

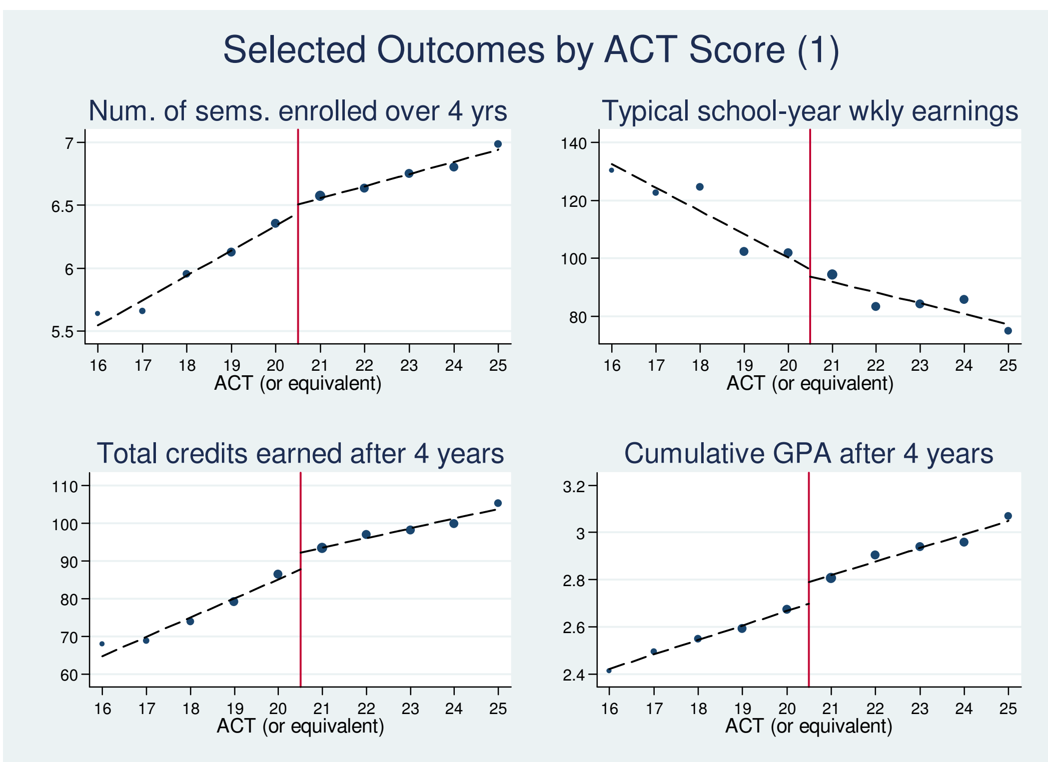


SOURCEand NOTES: See Figure 2. [None of these differences at the threshold are statistically significant.]

Figure 4.

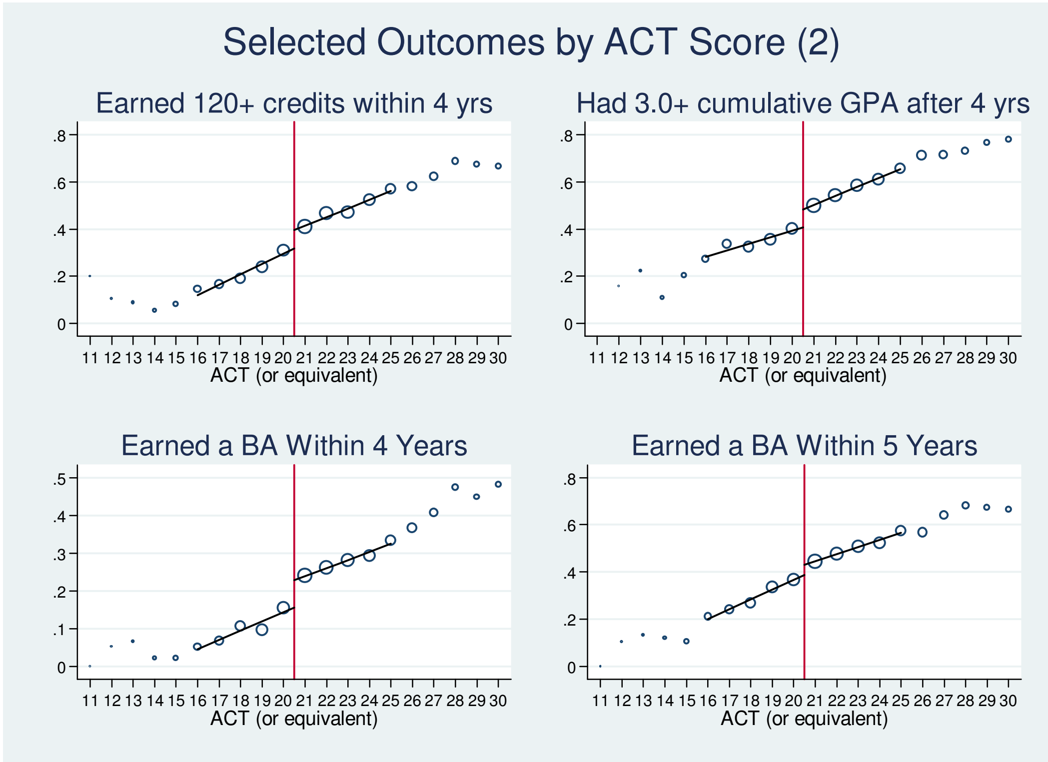


(same graphs, zooming in)

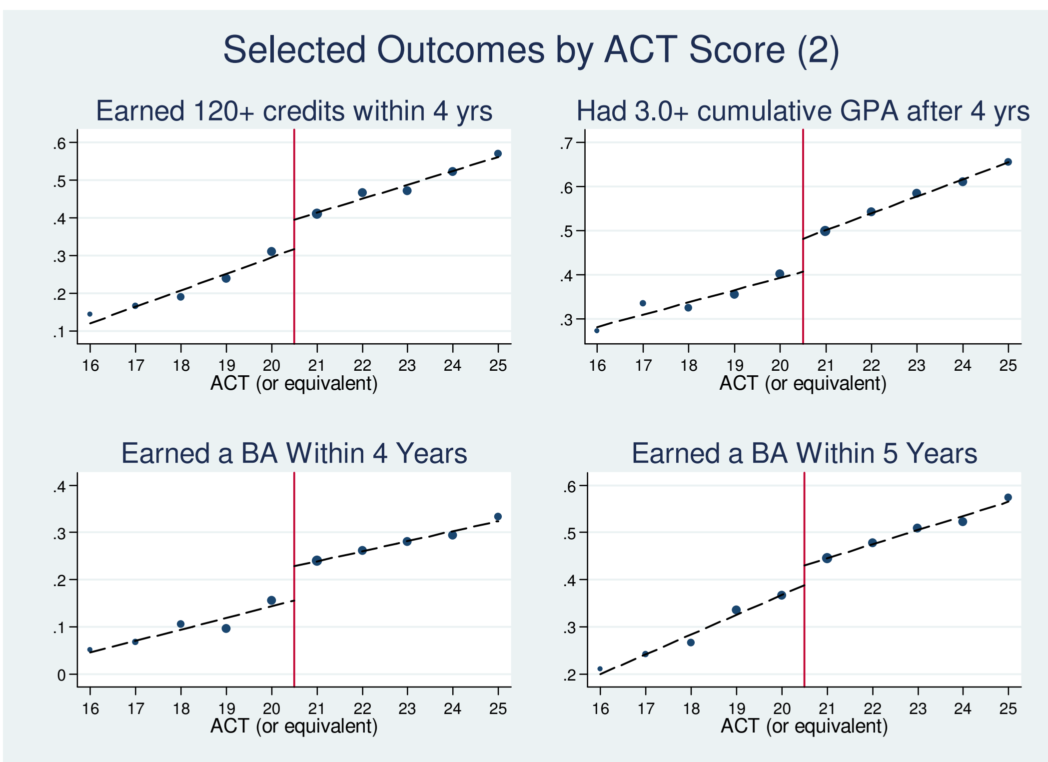


SOURCE and NOTES: See Figure 2.

Figure 5.



(same graphs, zooming in)



Source and Notes: See Figure 2.

Figure 6.

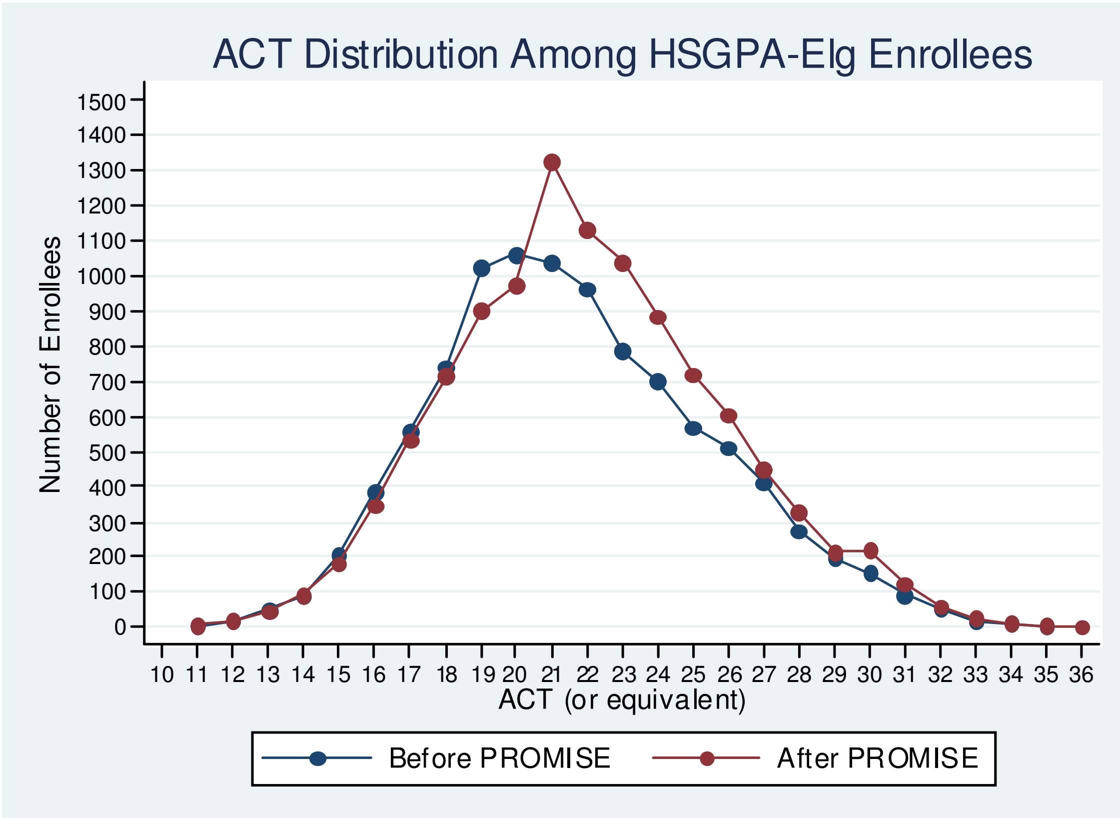
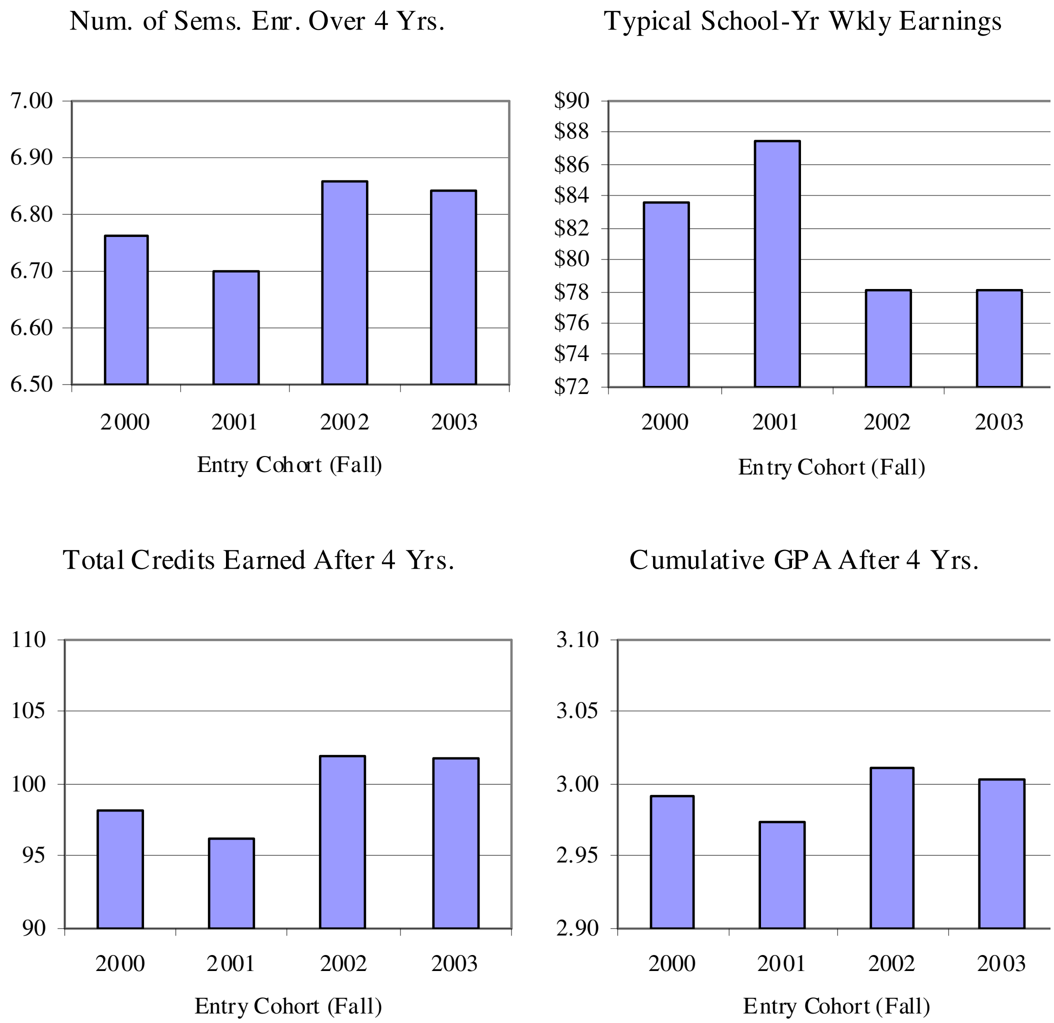
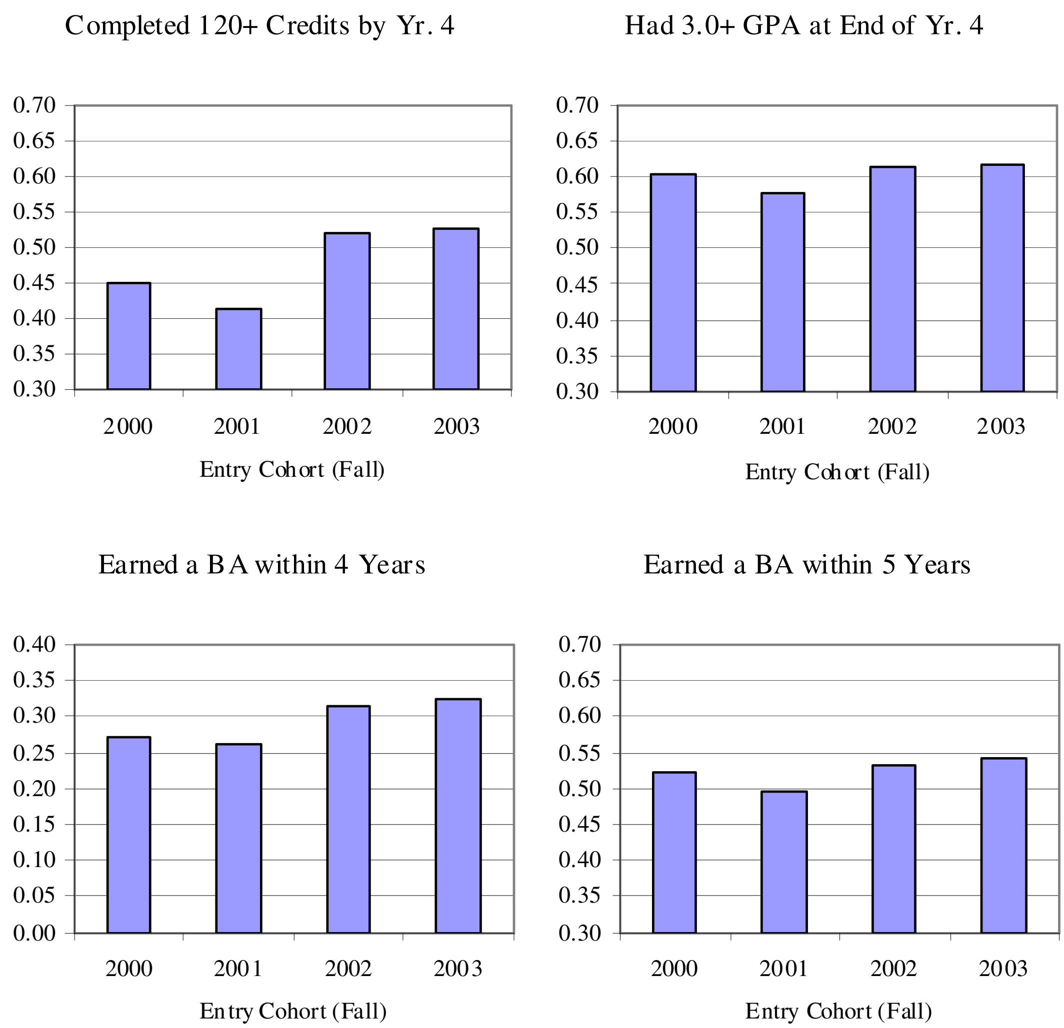

ACT Distribution Among HSGPA-Elg Enrollees
Number of Enrollees
0
100
200
300
400
500
600
700
800
900
1000
1100
1200
1300
1400
1500
10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36
ACT (or equivalent)
Before PROMISE
After PROMISE

Figure 7. Selected Outcomes By Entry Cohort (1)
For Entrants Who Qualified for PROMISE



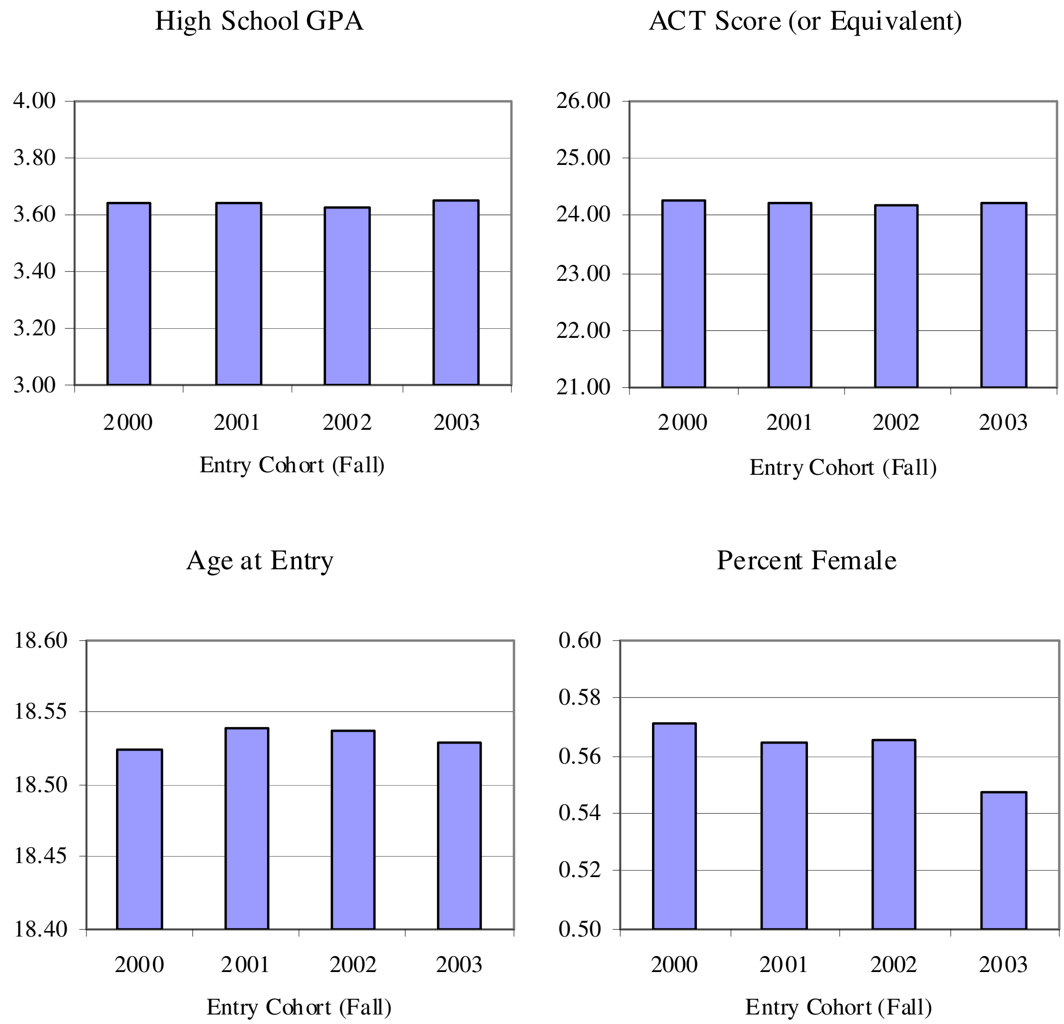
SOURCE: Author's calculations using WVHEPC administrative data on first-time degree-seeking West Virginia residents aged 19 and younger, who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.
NOTES: Bars indicate unadjusted means by cohort. Typical weekly 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 8. Selected Outcomes By Entry Cohort (2)
For Entrants Who Qualified for PROMISE



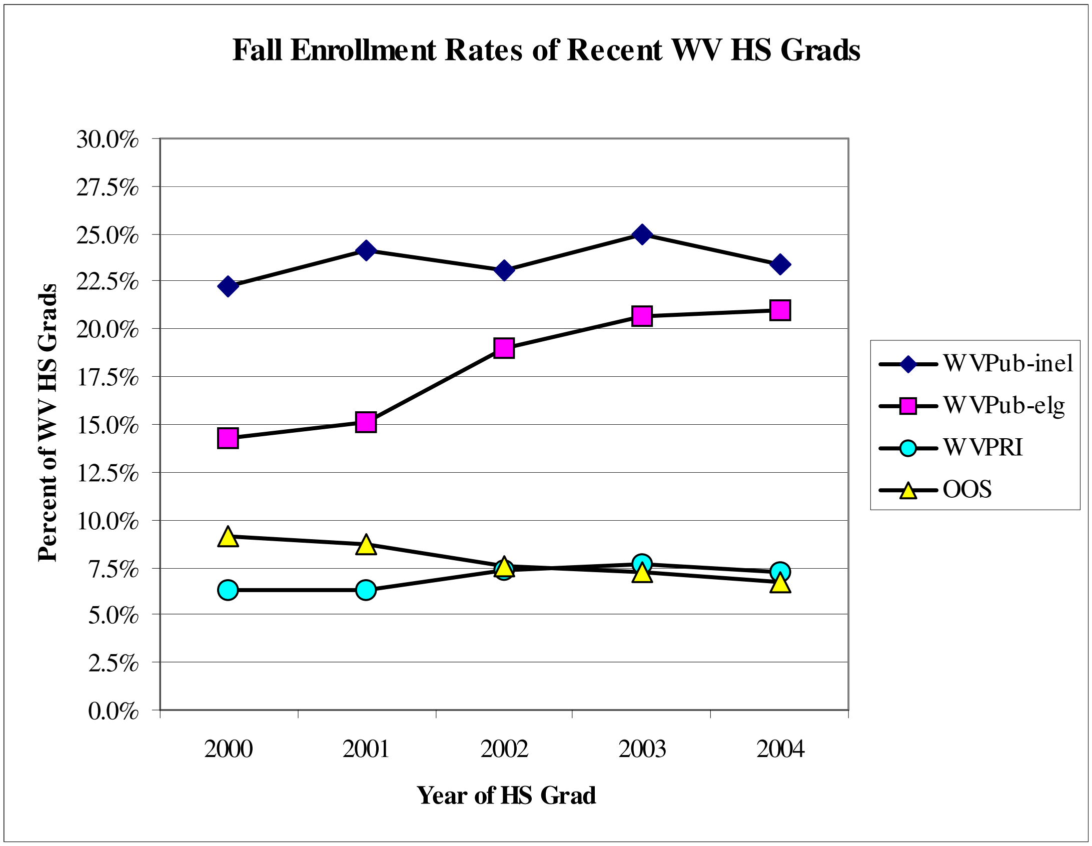
SOURCE: Author's calculations using WVHEPC administrative data on 12,911 first-time degree-seeking West Virginia residents aged 19 and younger, who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.
NOTES: Bars indicate unadjusted means by cohort.

Figure 9. Selected Covariates By Entry Cohort
For Entrants Who Qualified for PROMISE



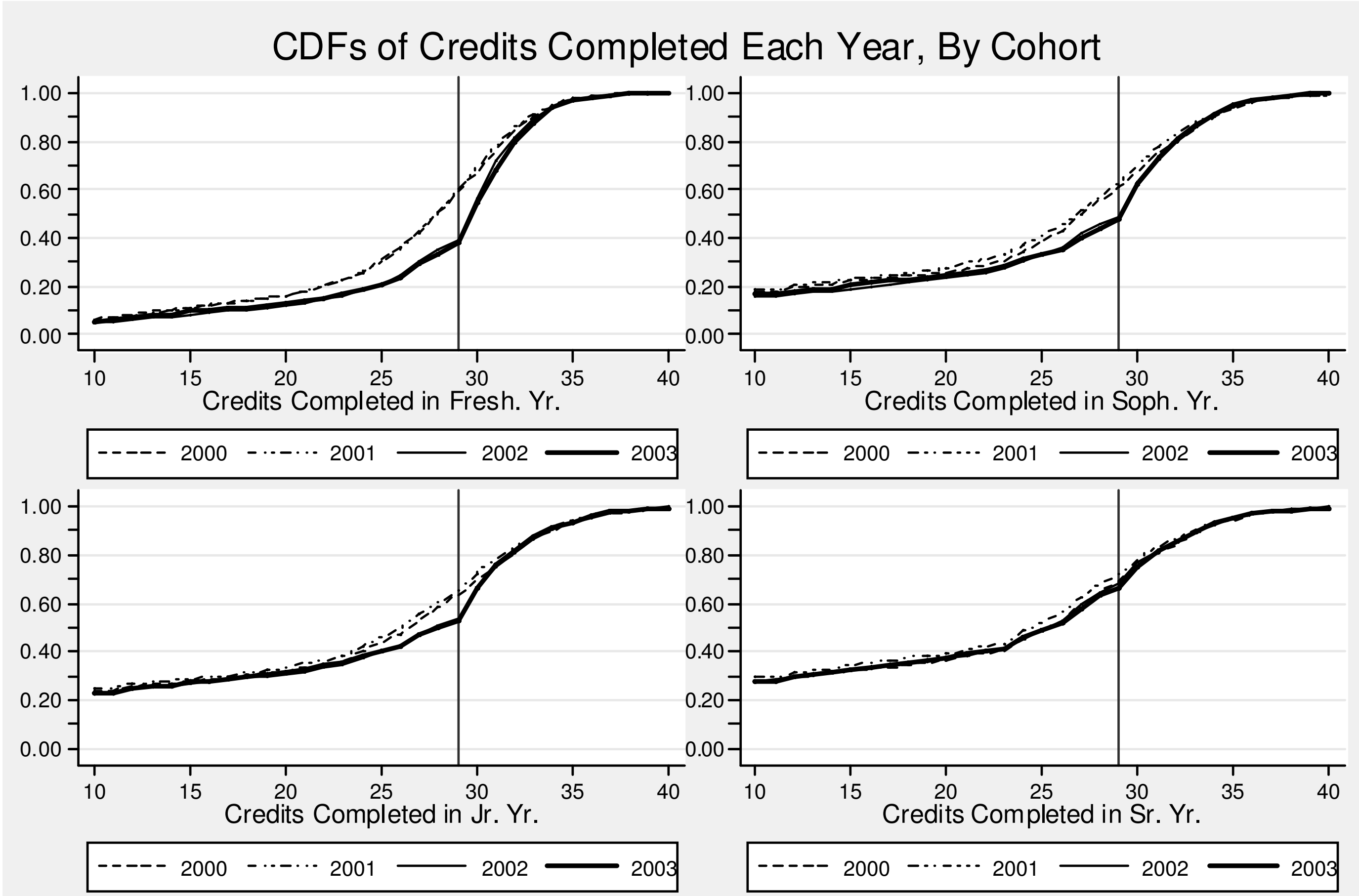
SOURCE: Author's calculations using WVHEPC administrative data on 12,911 first-time degree-seeking West Virginia residents aged 19 and younger, who met the high school GPA (3.0+) and ACT/SAT (21/1000+) score requirements for PROMISE eligibility.
NOTES: Bars indicate unadjusted means by cohort. About 10 percent of this sample took only the SAT; their scores are converted to ACT scores using a national concordance table.

Figure 10.



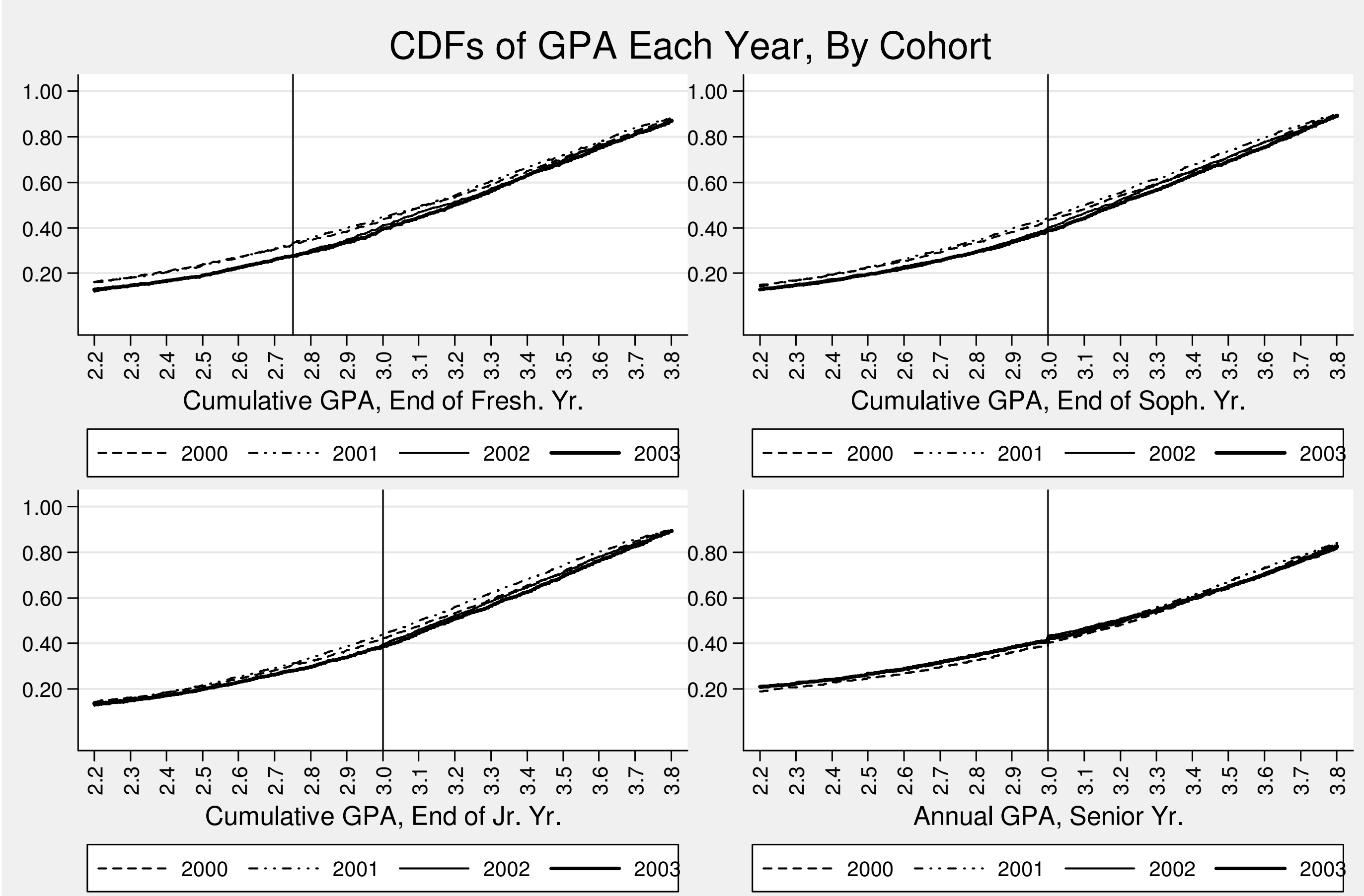
SOURCE: Author's calculations using the following data sources: HSGRADS - WV Department of Education, as reported in annual WVHEPC College Going Rate reports. WVPUB - Author's calculations using WVHEPC administrative data on enrollments. Includes first-time freshmen seeking a 2/4-yr degree who graduated 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. OOS - IPEDS Residence and Migration reports, and WVHEPC surveys of high school administrators as reported in annual College Going Rate reports.

Figure 11.



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

Figure 12.



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