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The merits of universal scholarships: Benefit-cost evidence from the Kalamazoo promise

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ABSTRACT

As the costs of higher education rise, many communities have begun to adopt their own financial aid strategy: place-based scholarships for students graduating from the local school district. Some place-based scholarships impose merit- and/or need-based restrictions, while others require little more than residency and graduation. In this paper, we examine the reach and cost-effectiveness of the Kalamazoo Promise, one of the more universal and more generous place-based scholarships. Building upon estimates of the program's heterogeneous effects on degree attainment, individual-level scholarship cost data, and projections of future earning profiles by education, we examine the Promise's benefit-cost ratios for different types of students differentiated by income, race, and gender. Although the average break-even rate of return of the program is about 11 percent, rates of return vary greatly by group. The Promise has high returns for both low-income and non-low-income groups, for nonwhites, and for women, while benefit assumptions matter more for whites and men. Our results show that universal scholarships can reach many students and have a high rate of return, particularly for places with a high percentage of African American students.

JEL Classification Codes: I21, I22, I24

Key Words: place-based scholarship, enrollment, college completion, natural experiment, difference-in-differences, financial aid policy, benefit-cost analysis

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Many key constituencies—middle-class voters, the business community, politicians on both the left and right, and policy analysts—perceive increased college completion rates as a high-return investment for individual students as well as for the United States. But increased college completion is burdened by rising college costs. These rising costs have sparked questions about whether traditional college financial aid policies—which have usually been targeted on financial need, merit, or both—are successful in increasing college completion rates in a way that is both efficient and effective, and that inspires political support.

On the one hand, need-based financial aid, such as the federal Pell Grant program, targets groups that have traditionally been underrepresented among college completers. However, such aid is often complicated to administer at scale, leading to lower than intended take-up rates and raising questions about its effectiveness in reaching students. Also, the aid often goes to students who do not successfully complete college, raising questions about its efficiency. These low college completion rates not only reduce need-based aid's efficiency and effectiveness, but also reduce such aid's political support. On the other hand, merit-based financial aid that ignores need is available to any student who meets a minimum achievement requirement (Dynarski 2004), and such students are more likely to be successful in completing college. Merit-based aid reduces costs for middle-class families who place a high value on college, which increases merit aid's political support. But merit aid is often utilized by students who would have gone to college anyway (e.g., Cornwell and Mustard 2006, 2007), which also raises questions about its efficiency and effectiveness in increasing college completion rates.¹

¹ Some programs target both need and merit. Aid targeted to the low-income, academically ready population is likely to be efficient, in the sense of boosting marginal college completion. But such tightly targeted financial aid reaches relatively few people, as the population that is both low-income and academically ready is small. So although such aid may be efficient per dollar spent, it is unlikely to significantly boost overall national college completion rates.

This dilemma of how to achieve political support, efficiency, and efficacy of traditional student financial aid programs has, in part, provided an impetus for proposals for “free college,” which rely less on targeting need or merit. For example, Tennessee began a program in the fall of 2015 in which the first two years of community college are tuition-free.² Additionally, the Obama administration proposed an ambitious plan that would make community college tuition-free, without merit requirements or means-testing (see Executive Office of the President 2014).

In this context, place-based scholarships provide an interesting alternative for financing college. These scholarship programs, of which several dozen currently exist around the United States, are “place based” in that the scholarship is based on a high school graduate’s locality—most often the local school district. Although many of these programs have merit or need requirements, some do not. In particular, the Kalamazoo Promise scholarship does not require any minimum achievement or demonstration of financial need. Moreover, it pays up to 100 percent of four years of college tuition and it is also easy to understand and apply for. Therefore, the Kalamazoo Promise arguably represents a good local laboratory for studying the efficiency and effectiveness of proposals calling for more universal access to college aid.

In this paper, we complement our companion paper (Bartik, Hershbein, and Lachowska 2015) by conducting a detailed benefit-cost analysis of the Kalamazoo Promise for different groups of students. In our previous paper, we find that the Promise increased college completion. We also find that, for our sample overall, the present value of lifetime earnings rises by 4.7 times Promise costs paid, which corresponds to a break-even rate of return of 11.3 percent, a large return for any educational intervention. In this paper, we disaggregate these overall effects on college completion, earnings, and costs separately by family income, ethnicity, and gender.

² See <http://tennesseepromise.gov/>. Oregon’s similar program is scheduled to begin in 2016.

Using these group-specific calculations, we find that the Promise has high benefit-cost ratios and rates of return for different income groups, for nonwhites, and for women. Although the benefit-cost ratios and rates of return are smaller for men and for whites, and the weighted average of group-specific rates of return is lower than the simple average, we conclude that the Kalamazoo Promise easily passes a benefit-cost test. Hence, the lack of targeting of either merit or need by the Kalamazoo Promise does not inhibit the program from having high cost-effectiveness. And its universality means that by definition the Kalamazoo Promise operates at a large scale with large take-up rates. The Promise's high rates of return for both low-income and non-low-income groups suggest that broad-based financial aid programs can cost-effectively boost college completion for a wide range of students.

The remainder of this paper is organized as follows. The next section summarizes the evaluation of the Kalamazoo Promise's effects on college outcomes in Bartik, Hershbein, and Lachowska (2015), emphasizing the heterogeneous impacts for different groups of students. We then describe how we conducted a back-of-the-envelope benefit-cost analysis in the earlier paper, on which we build herein. The following section explains how we calculate educational attainment effects, expected earnings, and costs for each subgroup of family income, race, and gender, and how we construct benefit-cost measures. We discuss these results and some of their limitations in the context of existing and proposed college scholarships. Finally, we conclude by arguing for the importance of such disaggregated analysis of the impacts of financial aid programs, as it can lead to surprising conclusions.

THE IMPACTS OF THE KALAMAZOO PROMISE ON COLLEGE SUCCESS

The Kalamazoo Promise is a 10-year-old universal college scholarship for graduates of southwestern Michigan's Kalamazoo Public Schools (KPS), a midsized, mostly urban district that is considerably more ethnically diverse and poorer than surrounding areas. Announced in November 2005 and first applying to the high school class of 2006, the Promise pays for up to 100 percent of required tuition and fees at any public college or university in Michigan.³ The Promise differs from most college scholarships because its requirements are few: students must live within the school district, continuously attend it, and graduate from high school. The longer the student has been in the district, the more generous the scholarship, from 65 percent of tuition and fees covered for students who start at the beginning of high school, up to 100 percent for students continuously enrolled since kindergarten. Students who enter KPS after the beginning of ninth grade are ineligible. Importantly, the scholarship is “first-dollar,” meaning that it is applied *before* any other financial aid from other sources, which can be used for additional expenses such as room and board or books.

Financed by anonymous private donors, the Promise is considerably easier to apply for than federal financial aid, which requires the FAFSA. Students at the beginning of their senior year fill out a one-page form consisting of basic contact information, three questions on their attendance at KPS, and where they intend to go to college. After students confirm their college choice when they graduate, the Promise office takes care of the rest. Nearly all graduating students submit a Promise application—whether or not they are among the roughly 90 percent of students who are eligible—and more than 85 percent of eligible students have received Promise

³ Beginning in the fall of 2015, the Promise also covers several Michigan private colleges. However, benefit and cost estimates in this paper are based on earlier cohorts, for which this program expansion is irrelevant.

money. This percentage might rise, however, as students have 10 years from high school graduation to use the scholarship, which pays for up to 130 credits. To keep the scholarship, students must be enrolled full time (part-time enrollment is allowed at the local community college) and maintain a 2.0 GPA; if they fall below these thresholds, they can regain eligibility by subsequently meeting them again. By the end of 2014, the Promise had paid out \$61 million in scholarships, with an apparent steady state of about 1,400 KPS graduates using it.

Early research on the Kalamazoo Promise found significant impacts on the district's enrollment, which had been steadily declining before the Promise announcement and abruptly reversed course afterward, in large part because of reduced exit rates from the district (Bartik, Eberts, and Huang 2010). Subsequent studies found improvements in disciplinary outcomes of high school students and higher GPAs for African American students (Bartik and Lachowska 2013), as well as increased ACT score-sending—a proxy for applications—to the state's selective public universities, especially among poorer students (Andrews, DesJardins, and Ranchhod 2010).

More recently, Bartik, Hershbein, and Lachowska (2015) examine how the Kalamazoo Promise has affected college-going outcomes.⁴ Using administrative data from KPS and the Kalamazoo Promise, merged with National Student Clearinghouse (NSC) data on college outcomes, they employ a difference-in-differences strategy comparing eligible to ineligible students, before and after the Promise took effect. Consistent with previous literature on the effects of scholarships, they find that the Promise substantially increased college enrollment and reoriented it toward four-year colleges: they estimate that college enrollment within six months of high school graduation rose by 8.5 percentage points (14 percent) and that enrollment at a

⁴ This previous paper also offers a more thorough review of studies of the Kalamazoo Promise, studies of related Promise programs, and studies of the effects of college scholarships more generally.

four-year institution rose by 10 to 13 percentage points (25 to 33 percent). Promise-eligible students also enrolled in additional classes, between seven and nine credits' worth four years after high school graduation.

Perhaps most relevant to the current policy concern about students' college success, the authors also investigate the impact the Promise has had on degree attainment. Table 1 replicates Table 6 from the earlier paper. The four panels show the Promise effects for four outcomes: the receipt of any credential—certificate, associate's degree, or bachelor's degree—within four years of high school graduation, receipt of any credential within six years of high school graduation, receipt of a bachelor's degree within four years of high school graduation, and receipt of a bachelor's degree within six years of high school graduation. The two columns list the coefficient estimates from the linear difference-in-differences model in two specifications: the left column controls for demographic characteristics of the student (sex, ethnicity, and subsidized lunch eligibility, as well as high school and year of graduation dummies), while the right uses inverse probability weights to make post-Promise cohorts resemble pre-Promise cohorts within eligibility status.⁵

The estimates indicate that there is little effect of the Promise on credential completion within four years of high school graduation, but that there are large, positive, and statistically significant impacts at the six-year horizon. The probability that a graduate earns a credential increases by 9–12 percentage points (25–34 percent), and most of this increase is due to increased bachelor's degree attainment, which rose 7–10 percentage points (23–33 percent).

⁵ The weights are derived from saturated logit models, separately by eligibility status, of a pre-Promise cohort indicator on all interactions of student demographic characteristics.

Table 1 Promise Effects on Degree Attainment from Bartik, Hershbein, and Lachowska (2015)

	(1)	(2)
Panel A: Any credential at four years		
(Mean of outcome after=0, elig.=1) = 0.184		
After × eligible	0.004 (0.032)	0.019 (0.035)
R^2	0.085	0.075
Panel B: Any credential at six years		
(Mean of outcome after=0, elig.=1) = 0.357		
After × eligible	0.091* (0.047)	0.123** (0.050)
R^2	0.145	0.132
Panel C: BA/BS at four years		
(Mean of outcome after=0, elig.=1) = 0.141		
After × eligible	0.004 (0.024)	0.016 (0.028)
R^2	0.113	0.098
Panel D: BA/BS at six years		
(Mean of outcome after=0, elig.= 1) = 0.296		
After × eligible	0.067* (0.041)	0.097** (0.045)
R^2	0.178	0.159
Use inverse probability weighting (IPW)?	No	Yes

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors robust to heteroskedasticity are in parentheses. Outcome timing is since high school graduation. Regressions include dummies for post-Promise, individual (pseudo) eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation. The mean of the dependent variable is for the eligible population in the pre-Promise period. Sample sizes are 3,869 (3,851) at four years for non-IPW (IPW) sample, and 2,905 (2,896) at six years for non-IPW (IPW) sample. Smaller IPW sample reflects lack of matches in the fully saturated propensity score model.

These effects are somewhat larger, in both absolute and relative terms, than those found in recent studies of college scholarships' effects on completion outcomes. Scott-Clayton (2011), for example, finds a 4–5 percentage point increase in bachelor's completion after five years from high school graduation, from a pretreatment mean of 37 percent, in her analysis of West Virginia's merit-based PROMISE program. She finds slightly larger estimates for completion

within four years of high school graduation, suggesting a quicker time to degree that fades over time, the opposite of the Kalamazoo Promise results.

Similarly, Castleman and Long's (2013) study of Florida's need-based Student Access Grant finds bachelor's completion effects of about 4–5 percentage points (22 percent) within six years of high school graduation, with the effects concentrated among students with higher high school GPAs. Furthermore, Goldrick-Rab et al. (2015) find a 4–5 percentage point (29 percent) increase in bachelor's attainment within four years of high school graduation in their analysis of the need-based Wisconsin Scholars Grant.⁶

The West Virginia scholarship was approximately as generous as the Kalamazoo Promise in covering tuition and fees but was available only to students who had significant high school achievement (a high school GPA of at least 3.0 and ACT of at least 21). The Florida and Wisconsin scholarships are slightly less generous than the Kalamazoo Promise, covering roughly 60 percent of tuition and fees, but are available only to relatively poor students (those coming from families with roughly \$40,000 or less in annual income). The magnitude of the Promise effects is thus noteworthy, especially since the Promise is not specifically targeted toward a group of students who might be expected to have higher completion rates when financial constraints are eased.

For this reason it is important to examine the effects of more universal scholarships on different groups of students. Table 2 shows estimated Promise effects on credential completion at six years after high school graduation for three paired groups of graduates: low-income and non-

⁶ In addition to these studies, Dynarski (2008) finds a 2.5 percentage point increase in bachelor's (or more) attainment for cohorts of students exposed to the Arkansas and Georgia merit-based scholarships, although questions of statistical inference with her methodology were noted by Conley and Taber (2011), and Sjoquist and Winters (2014) find no impact when generalizing Dynarski's study to other merit-aid states or controlling for students' academic characteristics. Cohodes and Goodman (2014) find negative impacts of degree attainment from a merit scholarship in Massachusetts, which they argue result from inducing students to attend lower-quality colleges, a context that may be unique to New England.

Table 2 Promise Effects on Degree Attainment: Heterogeneity by Groups

	Any credential at six years		Bachelor's at six years	
	(1)	(2)	(3)	(4)
Panel A: Income Groups	Non-low	Low	Non-low	Low
After × eligible	0.119 (0.077)	0.086 (0.058)	0.089 (0.076)	0.059 (0.043)
<i>N</i>	1,641	1,259	1,641	1,259
p-value of group difference		0.733		0.723
Mean of outcome	0.477	0.201	0.398	0.104
Panel B: Race/Ethnicity	White	Nonwhite	White	Nonwhite
After × eligible	-0.007 (0.080)	0.135** (0.056)	0.026 (0.069)	0.073 (0.048)
<i>N</i>	1,545	1,360	1,545	1,360
p-value of group difference		0.146		0.570
Mean of outcome	0.463	0.237	0.397	0.158
Panel C: Gender	Male	Female	Male	Female
After × eligible	-0.012 (0.060)	0.185*** (0.069)	-0.003 (0.055)	0.133** (0.059)
<i>N</i>	1,388	1,517	1,388	1,517
p-value of group difference		0.030		0.088
Mean of outcome	0.336	0.376	0.294	0.297

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimates calculated without inverse probability weighting (IPW); results using IPW are similar and available on request. Standard errors robust to heteroskedasticity are in parentheses. Timing is since high school graduation. All regressions include dummies for post-Promise, individual (pseudo-) eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation (except when subgroup is restricted on one of these dimensions). The income groupings pertain to whether the student is eligible for free/reduced price lunch or not. The race groups are white non-Hispanic versus other groups. The mean of the dependent variable for each group is calculated over the eligible population in the pre-Promise period.

low-income students (determined by eligibility for the federal free and reduced-price lunch program⁷); white, non-Hispanic students and students of any other race or ethnicity; and men and

⁷ Students qualify for the free lunch program if their family incomes are less than 130 percent of the federal poverty guideline, and they qualify for reduced-price lunch if their family incomes are between 130 percent and 185

women. The right side of Table 2 shows bachelor's completion for each subgroup, copied from Table 7 of Bartik, Hershbein, and Lachowska (2015). The left side of Table 2 shows completion of any credential, an outcome that did not explicitly appear in the earlier paper.

The estimates show that the Promise has a diverse pattern of credential completion effects across groups. In the first panel, in which we compare low-income students with more-affluent students, effects for either completion outcome vary only slightly in magnitude, about 2.5–3 percentage points.⁸ Both types of students have effects that are large in both absolute and proportional terms (6–12 percentage points, or 22–57 percent). However, across race and gender, there are larger differences. The Promise has (an imprecisely estimated) null effect on the credential completion of white students, but it substantially boosts college completion among students of color, especially in proportional terms (around 50 percent). Although smaller sample sizes mean that we cannot rule out the same treatment effect across ethnic groups, the differences are compelling.

On the other hand, the differences in estimated effects between men and women are even larger and are statistically significant at conventional levels. While men's completion appears unaffected by the Promise, women experience very large gains of 13–19 percentage points (45–49 percent).

It is not straightforward to compare these estimates to others in the literature, both because the need- or merit-based nature of other scholarships can make student income an infeasible margin of comparison and because few other studies have explicitly looked at

percent of the poverty guideline. The vast majority of students in KPS who qualify for one of the programs qualify for free lunch, and we combine them into one group.

⁸ Although the point estimates for low-income students are slightly lower than for non-low-income students, the differences are not close to being statistically significant ($p = 0.72$), and in proportional terms, the effects are actually larger among low-income students.

heterogeneous effects of scholarship eligibility on completion outcomes by ethnicity or gender. An exception is Goldrick-Rab et al. (2015), who find stronger effects for women than for men but weaker effects for students of color than for whites, although their estimates for these groups are even less precise than those for the Kalamazoo Promise. Nonetheless, this apparent heterogeneity in treatment effects suggests the possibility of heterogeneity in benefit-cost analysis as well.

SUMMARY OF BENEFIT-COST ANALYSIS IN BARTIK, HERSHBEIN, AND LACHOWSKA (2015)

Bartik, Hershbein, and Lachowska (2015) conclude their paper by conducting an overall, back-of-the-envelope benefit-cost analysis of the Promise by using the estimated Promise effect on obtaining any college degree. In this section, we briefly describe the procedure used in the earlier paper, and in the next subsection we discuss how we extend this analysis to different subgroups.

As with similar benefit-cost analyses of college scholarships (Dynarski 2008; Scott-Clayton 2009), the basic idea is to estimate the increase in expected lifetime earnings that stem from increased educational attainment and compare these benefits to the costs of the scholarship program.

Such a benefit-cost analysis is obviously incomplete. Focusing on earnings alone understates benefits because it does not include nonpecuniary returns to education such as improved health, reduced crime, and increased civic participation (Currie and Moretti 2003; Moretti 2004; Oreopoulos and Salvanes 2011). Focusing on individual earnings also may understate collective earnings increases if there are spillover benefits in some workers' skills on

other workers' productivity, due to, for example, agglomeration economies (Moretti 2003, 2004, 2012). Furthermore, a focus on earnings may not fully capture an individual worker's change in well-being, as increased earnings come in part from reduced unemployment and increased labor force participation, which may reduce both stigma effects of unemployment and leisure time.⁹ Beyond the financial costs of providing scholarships, increased educational attainment may come with opportunity costs due to reduced leisure and reduced earnings while in college. Finally, treating the financial payout of scholarships as a pure cost ignores the benefits of this income transfer for students and their families.¹⁰

Having said that, a comparison of earnings benefits with scholarship costs has the merit of simplicity. This simple comparison avoids making assumptions about some controversial elements in a more formal social benefit-cost analysis. This "earnings versus scholarship" comparison also provides a useful building block to policy analysts who want to construct a more complex benefit-cost analysis. Finally, our simple comparison extends what previous researchers have done; see, for example, Dynarski (2008) and Scott-Clayton (2009).

To compute the earnings benefits of the Promise, we use microdata from the 2012 American Community Survey (ACS) to compute average unconditional earnings by age, for ages 25–79, and by three educational attainment categories: individuals with a high school diploma (excluding GED) and perhaps some college but without a postsecondary degree; those with an

⁹ Bartik (2012) provides reviews of the relevant literature on the costs associated with stigma and leisure effects of unemployment.

¹⁰ In a simple model in which we assume students are on their demand curve for higher education services, the total benefit of a price reduction in college tuition is equal to the trapezoid to the left of the demand curve between the old, higher price and the new, lower price. Assuming equal distributional weights, the net social cost of the scholarship would be derived by subtracting this benefit from the financial cost of the scholarship, equal to the rectangle described by the difference between the old and new price times the final quantity demanded of education. The net social cost of the scholarship would then be the "Harberger triangle" equal to approximately one-half the change in price times the change in quantity demanded. This net cost would be far less than the financial cost of the scholarship. However, given imperfect information and capital markets, students may not be on their true demand curve for higher education, which probably further lowers the net cost of the scholarship.

associate's degree; and those with at least a bachelor's degree. To infer the earnings benefits of obtaining an associate's degree that are due to the Promise, we use the ACS to compute the net present value of having an associate's degree relative to having a high school diploma and multiply this value by the estimated effect of the Promise on obtaining an associate's degree. To infer the earnings benefits of obtaining a bachelor's degree that are due to the Promise (including the option value of obtaining a graduate degree), we compute the net present value of having a bachelor's or higher degree relative to a high school diploma and multiply this value by the estimated effect of the Promise on obtaining a bachelor's degree. We sum these net present discounted values to obtain the earnings benefits of greater degree attainment due to the Promise.¹¹

These calculations implicitly make several assumptions about how the Promise's effects on educational attainment affect earnings. First, the analysis assumes that the cross-sectional variation in earnings by educational attainment can be interpreted as the causal effects of educational attainment on earnings, now and in the future. Second, the analysis assumes that the marginal student whose education increases because of the Promise will experience the same earnings increase as is true on average in the cross-section.¹²

¹¹ In other words, we assume the Promise affects earnings only by changing the educational composition of the population, and that earnings streams within educational category did not change. For small programs, this is a reasonable assumption, but if applied on a national scale general equilibrium effects would tend to reduce—albeit only slightly—the earnings premium from higher education (Acemoglu and Autor 2011).

¹² There is, of course, much evidence on the causal returns to education on earnings; Card (1999) reviews the older literature, and Zimmerman (2014) reviews more recent evidence, including his own. This literature generally finds returns on the order of 8–15 percent per year of college, with the higher end accounting for sheepskin effects. However, estimates allowing for heterogeneous returns on the dimensions in the current paper are less consistent. In Appendix A, we modify our estimates to use causal estimates of the return to education drawn from the recent literature. However, we do not regard these causal estimates as preferable, for three reasons. First, they are not separately available for all the different subgroups of students we examine here, forcing us to assume a homogeneous return. Second, some of these causal estimates are local average treatment effects (LATE) for students just passing an academic cutoff or being born just after a certain date, and thus may not reflect the average treatment effects that are of interest. Third, in line with Card (1999), they do not differ much from cross-sectional differences.

To compute the costs, we use actual cost data from the Kalamazoo Promise for every eligible student observed in the 2006 and 2007 cohorts. We discount costs back to the June of a KPS student's senior high school year.

Using a real discount rate of 3 percent, this procedure generates a ratio of benefits to costs of about 4.7. At a 5 percent discount rate, the ratio reduces to 3.0. We also calculate the Promise's internal rate of return (IRR), the real discount rate at which the present value of Promise earnings benefits is equal to the present value of Promise scholarship costs. This rate is 11.3 percent, which is high for an educational policy. For comparison, Scott-Clayton (2009) finds a benefit-cost ratio from the West Virginia PROMISE scholarship of 1.48, and Dynarski (2008) calculates a benefit-cost ratio of about 2 (or an IRR of 7.9 percent) from the Arkansas and Georgia state merit scholarships. That the Promise has such an overall high rate of return is remarkable given its universal nature.

In the next section, we build on the original cost-benefit analysis by studying the benefits and costs of the Promise separately for different subgroups.

HETEROGENEITY IN BENEFIT-COST ANALYSIS OF THE PROMISE

Cost Analysis of Promise Scholarships

We calculate scholarship costs for each group of students, in 2012 dollars, and discount their present value to the time of high school graduation. But beyond this simplicity are some details. First, we calculate costs for only the first six years after high school graduation. This accords with the timing of our educational attainment estimates. Both costs and benefits of the Promise may change with a longer-run follow-up, as students are eligible for scholarships for up to 10 years. Second, we use cost data for only the 2006 and 2007 graduating cohorts because full

cost data are not available for later cohorts. (There is little sign of change over time in real costs per Promise-eligible student.) Third, we include only students who are in our educational attainment analysis sample. As can be seen in Table 3, we are able to match over 90 percent of Promise-eligible students in our analysis sample.¹³

Table 3 Numbers and Percentages of Promise Eligibles with Cost Data Matched to Analysis in Sample

KPS graduating class	Number of Promise-eligible students with cost data on scholarships paid	Number of such students who can be matched to our analysis sample	Percentage matched
2006	410	382	93.2
2007	504	467	92.7
Total	914	849	92.9

NOTE: The second column shows the number of Promise-eligible students with scholarship cost data in Kalamazoo Promise administrative files. The third column shows the intersection between such students and our analysis sample, for whom we have administrative data from KPS. We include students with KPS data as matched even if they have no record of postsecondary attendance from the National Student Clearinghouse.

The cost data provided to us by the Kalamazoo Promise report payments per student for three time periods each year: summer, fall, and winter/spring. We adjust these dollar amounts for inflation by calendar quarter using the personal consumption expenditures (PCE) deflator, and we apply various discount rates, setting $t = 0$ to June 15 of students' graduation year.¹⁴

In most of the present value analysis we use a real discount rate of 3 percent.¹⁵ However, we also sometimes consider the internal rate of return that equates Promise earnings benefits with Promise scholarship costs. We emphasize that the cost calculations per Promise-eligible student include eligible students who never receive any Promise funds at all. This makes our

¹³ Most of the nonmatches are by design. The Promise is open to graduates of alternative education programs that do not result in a high school diploma, but we did not include these students in the earlier educational attainment analysis.

¹⁴ We interpolate the deflator and discount rate log linearly, assuming Promise pay dates of June 15 (summer), September 15 (fall), and January 15 (winter/spring).

¹⁵ Such a discount rate is often used; Bartik (2011, p. 178) reviews the relevant discount rate literature.

estimates comparable with our educational attainment estimates, which include all Promise-eligible students, not just those who make use of the Promise.

Table 4 shows the present value of Promise scholarship costs per student, both overall and for various subgroups. Costs are slightly higher for the analysis sample than for all Promise-eligible students, reflecting that unmatched students tend to be marginal students who do not make much use of the Promise (see footnote 13). The rest of our calculations use matched students, for whom we have complete demographic information, useful both for model estimation and analyzing results by group. Among the few students obtaining only a certificate, costs are just over \$5,000, less than the costs among students who earned no credential at all, who on average spent more time in college.

Table 4 Costs of the Kalamazoo Promise per Promise-Eligible Student, Overall, by Highest Credential, and by Socioeconomic/Demographic Group

Overall costs (\$)		Costs by highest credential (\$)	
Total	16,456	BA	33,359
Matched total	17,620	AA	9,634
2006 grad. class	17,756	Certificate	5,075
2007 grad. class	17,483	No credential	7,644
Costs by groups (\$)			
Non-low-income	24,018		
Low-income	9,924		
White	22,608		
Nonwhite	11,891		
Male	16,775		
Female	18,419		

NOTE: All costs represent the present discounted value (relative to high school graduation date), at a 3% rate and in 2012 dollars, of Kalamazoo Promise scholarship payments made during first six years after high school graduation. Costs are per Promise-eligible student, including those who have not received Promise funds. All entries, except for the 2006 and 2007 graduating class lines, represent averages for the two graduating classes. Except for total entry, all costs are derived from the analysis sample. Demographic characteristics are taken from KPS data; highest credential (within six years of high school graduation) is taken from NSC. Low-income students are those who in high school were eligible for a free or reduced-price lunch (had family income below 185 percent of poverty guidelines). White students exclude Hispanic students.

The largest source of variation in Promise scholarship costs per student is the highest credential attained. Students who earn bachelor's degrees have higher Promise costs, both because they attend college for more years, and because four-year colleges are more expensive than two-year colleges. As shown in Table 4, Promise-eligible students earning a bachelor's degree have Promise costs more than three times as large as students earning an associate's degree.

For the various socioeconomic and demographic categories, per-student costs of Promise scholarships are higher for more-advantaged groups. Costs per student for the non-low-income group (\$24,000) are more than twice the costs for the low-income group (\$9,900). Similarly, per-student costs are almost twice as great for white non-Hispanics (\$22,600) as for other racial groups (\$11,900). Costs are only slightly greater for women (\$18,400) than for men (\$16,800).

Constructing Earnings Paths

For the overall sample and groups defined by race or gender, constructing education-specific career earnings paths is straightforward. We use cross-sectional variation in earnings by age and educational attainment—a synthetic-cohorts approach—and adjust for secular growth in wages and mortality.

More specifically, we first obtain cross-sectional annual earnings from the 2012 American Community Survey (ACS). The ACS is a 1 percent annual sample of the U.S. population, and the sample size is adequate to estimate real earnings by single year of age for demographic groups differentiated by educational attainment. Annual earnings reflect only wage and salary earnings; we exclude self-employment income, as well as all observations that have imputed earnings. However, we do include observations with zero earnings to capture both wage differences and propensity-to-work differences.

We categorize educational attainment into three groups. The first group consists of those with a regular high school diploma and some college, which may include certificates but not a postsecondary degree.¹⁶ The second group consists of those with exactly an associate's degree. The third group consists of those with at least a bachelor's degree.

For each educational group, we calculate sample-weighted mean earnings for cells defined by single year of age and demographic group. We include ages 25–79, inclusive. Many individuals have not completed their schooling before age 25, and our estimates show Promise completion effects at the six-year margin, when the modal student would have been 24. Therefore, it is difficult to know how to treat earnings at younger ages in the analysis, and so we exclude them.¹⁷ We assume that earnings after age 79 are negligible for the analysis, particularly once we discount and adjust for mortality.

We next adjust these cross-sectional earnings profiles for secular wage growth. We follow the assumption made by the Board of Trustees of the Social Security Administration (Board of Trustees 2015, Table V.B.1) and project 1.2 percent annual real wage growth over the next 60 years. In calculating this secular real earnings increase, we take the perspective of a Promise-eligible student graduating in 2006, the first Promise graduating class. We assume that the student was 18 in 2006, and thus 25 in 2013. We apply a 1.2 percent increase from the 2012 cross-sectional earnings to calculate the student's earnings for year 2013 and compound accordingly for future years.

¹⁶ The Promise is generally not awarded based on GED receipt; Heckman and LaFontaine (2010) document how GED recipients differ from those who received regular high school diplomas.

¹⁷ We thus do not account for the opportunity costs from forgone earnings of attending college. According to the ACS, the annual earnings of individuals ages 18–24 in Michigan who have only a high school diploma and are not enrolled in college are about \$9,700; the corresponding earnings for individuals in the same age range who are enrolled in college is about \$5,500. However, it is likely (based on unreported regressions) that the educational completion outcomes due to the Promise are concentrated among students who otherwise would have enrolled but not completed, making the determination of opportunity costs difficult.

We then use the 2010 U.S. life tables (Arias 2014) to adjust earnings profiles at later ages for expected mortality (by group) since age 18. For the overall sample, we simply use life tables for the overall population. For the racial groups, we use the white non-Hispanic life tables for the white group and the black life tables for the nonwhite group.¹⁸ For the gender groups, we use the male and female life tables. Finally, we apply the same present value discounting as we did with Promise costs.

The above calculations suffice for race and gender groups. However, groups defined by family income while in high school require a more complex procedure, which we describe in the next subsection.

Calculating Expected Earnings Benefits for Low-income Students

Calculating an earnings stream for individuals who grew up eligible for the federal free or reduced-price lunch program, the most common proxy for low income among K–12 students, is challenging. Cross-sectional data sources, including the ACS, do not contain a variable that captures the family income of its respondents when they were growing up.

Therefore, we turn to the Panel Study of Income Dynamics (PSID), which since 1968 has tracked the same individuals and the families they join. These data allow us to identify individuals who as teenagers (ages 13–17) lived in families whose incomes fell below or above 185 percent of the poverty guideline in the year of observation.¹⁹ As these individuals age, we observe their reported earnings and educational attainment over their careers. We calculate

¹⁸ The vast majority of nonwhite KPS graduates are black.

¹⁹ An income of 185 percent of the poverty guideline is the threshold of eligibility for reduced-price lunch. Historical poverty guidelines are available at: <http://aspe.hhs.gov/prior-hhs-poverty-guidelines-and-federal-register-references>. Our procedure averages family income (relative to the poverty guideline) over the period the individual is between 13 and 17, which should reduce transient fluctuations in income and other forms of measurement error (Haider and Solon 2006). Actual earnings profiles are predicted values from regressions of reported annual earnings (including zeros) on a quadratic in potential experience and a set of year-of-observation dummies.

average earnings for each age and education level separately for individuals who grew up in low-income families and those who did not.²⁰ Pooling the cohorts, some of whom were teenagers in the late 1960s (and thus are in their sixties today) and others of whom were teenagers in the early 2000s (and are thus in their late twenties today), and adjusting earnings for inflation to year 2014 dollars (using the PCE deflator from the Bureau of Economic Statistics), yield nearly complete earnings profiles by education and presumed eligibility for the federal lunch program as an adolescent.

We present these earnings profiles in Figures 1a and 1b. Two features are evident. First, and well known, individuals with greater education earn substantially more than those with less education throughout the career, and the increase is especially large for those with a bachelor's degree. Second, and considerably less well known, individuals who grew up poor earn substantially less than those who didn't, even *conditional on educational attainment*.

Furthermore, this disparity grows with education and experience. For example, individuals who grew up poor and did not earn a postsecondary credential earn about 80 percent as much in early career as do their similarly educated peers who did not grow up poor. By midcareer, however, they are earning only 70 percent as much. On the other hand, bachelor's degree holders who grew up impoverished start their careers with earnings about two-thirds as much as their more-economically advantaged peers, and this ratio falls to half by midcareer. These ratios also imply that the observed earnings premium for those with a baccalaureate degree relative to those with no postsecondary credential is much higher for individuals who did not grow up poor (about 135 percent) than it is for those who did (about 69 percent), although it is still considerable for both

²⁰ While earnings clearly vary over the career, we fix educational attainment to the level reported by individuals in the first survey wave when they were 25 or older.

Figure 1A

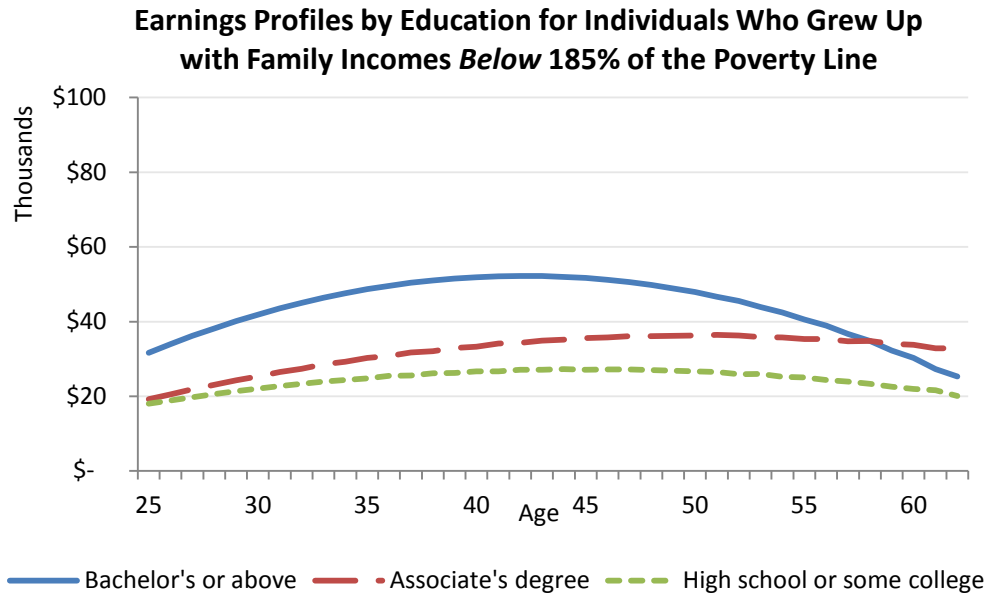
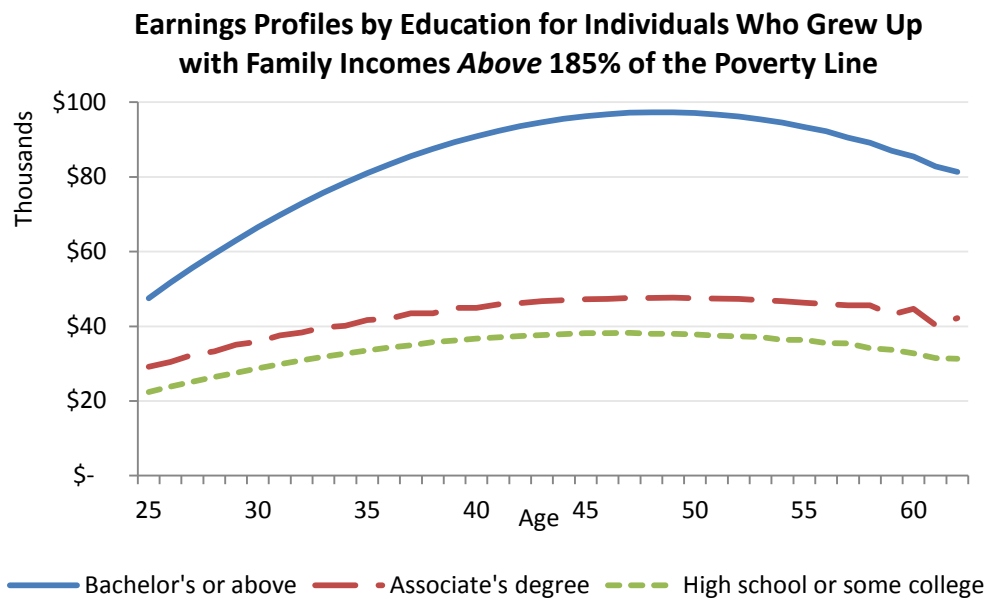


Figure 1B



SOURCE: Authors' calculations from the Panel Study of Income Dynamics.

NOTES: Dollars are adjusted for inflation to year 2014 using the Personal Consumption Expenditures (PCE) Deflator from the Bureau of Economic Analysis. "Bachelor's and above" includes respondents who had 16 or more years of education when observed at age 25; "Associate's degree" includes respondents who had 14 or 15 years of education when observed at age 25; "High school or some college" includes respondents who had 12 or 13 years of education when observed at age 25. Family-income classification is based on average family income when respondent was 13 through 17. Series shown represent fitted values from regressions of annual earnings on a quadratic in potential experience and year-of-observation dummies with the latter netted out.

groups.²¹ To be clear, we are not arguing that the differences in these earnings profiles are necessarily causal, but they are highly suggestive that both absolute and proportional earnings gains from additional education can vary greatly by family economic circumstances, and any cost-benefit analysis that relies on projected earnings should be cognizant of these differences.²²

However, age-earnings profiles may have changed over time from the cohorts in the PSID in ways that make the calculated earnings profiles less representative for cohorts today. For example, changes in unionization, the returns to higher education, growing wage dispersion, and compositional change in the labor force all likely affected earnings profiles within and across education levels. For this reason, we make the less-restrictive (but still strong) assumption that the earnings profiles of individuals who grew up poor *relative* to those who did not remained the same, although absolute earnings may have evolved. Operationally, we implement this assumption by mapping the PSID-derived profiles onto the ACS for each education level and age. That is, for any given age and education in the PSID, earnings can be expressed as

$$y_{ae} = \alpha y_{ae}^{FRL} + (1 - \alpha) y_{ae}^{NFRL} ,$$

where y_{ae}^{FRL} are the age- and education-specific earnings for individuals who grew up eligible for free or reduced-price lunch, y_{ae}^{NFRL} are the age- and education-specific earnings for individuals who grew up ineligible for free or reduced-price lunch, and α is the share of the age-education cell who grew up in the first group. Because α is directly observed in the PSID, it is

straightforward to calculate y_{ae} in the PSID, as well. The ratios $\frac{y_{ae}^{FRL}}{y_{ae}}$ and $\frac{y_{ae}^{NFRL}}{y_{ae}}$ give the

proportional deviation of the family-income-specific earnings cell from the overall earnings cell,

²¹ These returns are taken from Table 5 and represent discounted lifetime earnings, adjusted for mortality and expected real earnings growth.

²² Although we can rule out the role of differential graduate degree attainment driving these differences, fully explaining the factors behind these large earnings gaps is beyond the scope of this paper but one that we consider a subject well worth future research.

and we apply these ratios to the equivalent observed earnings cell in the ACS. This procedure yields our calculated earnings stream for individuals who grew up poor and those who did not.²³

Returns to Education by Group

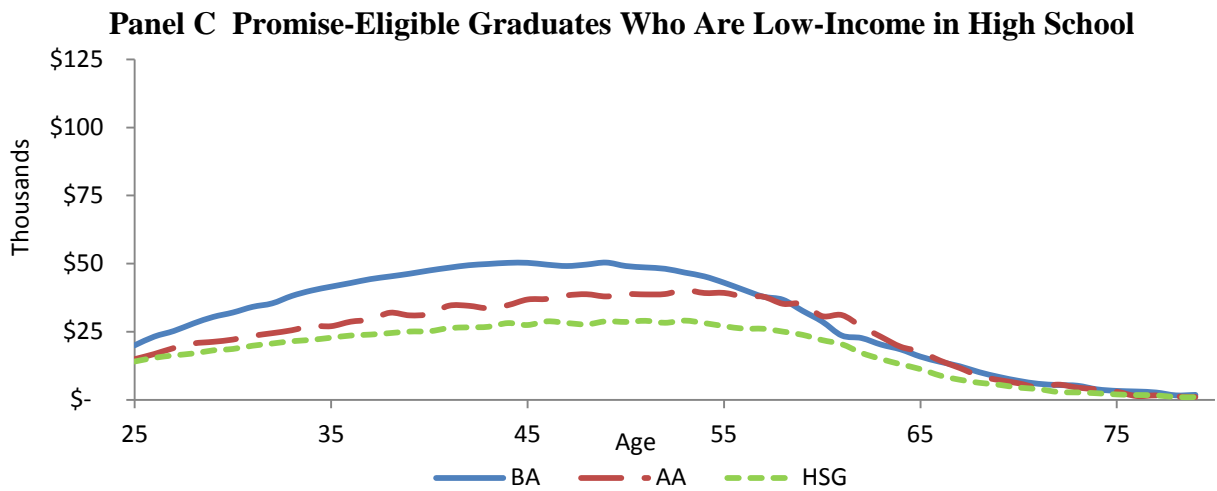
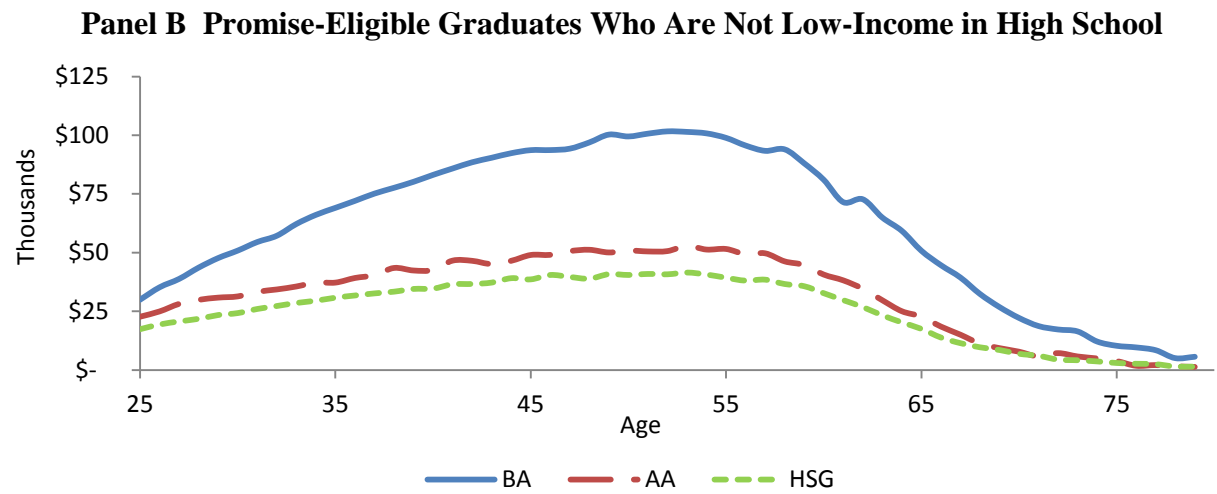
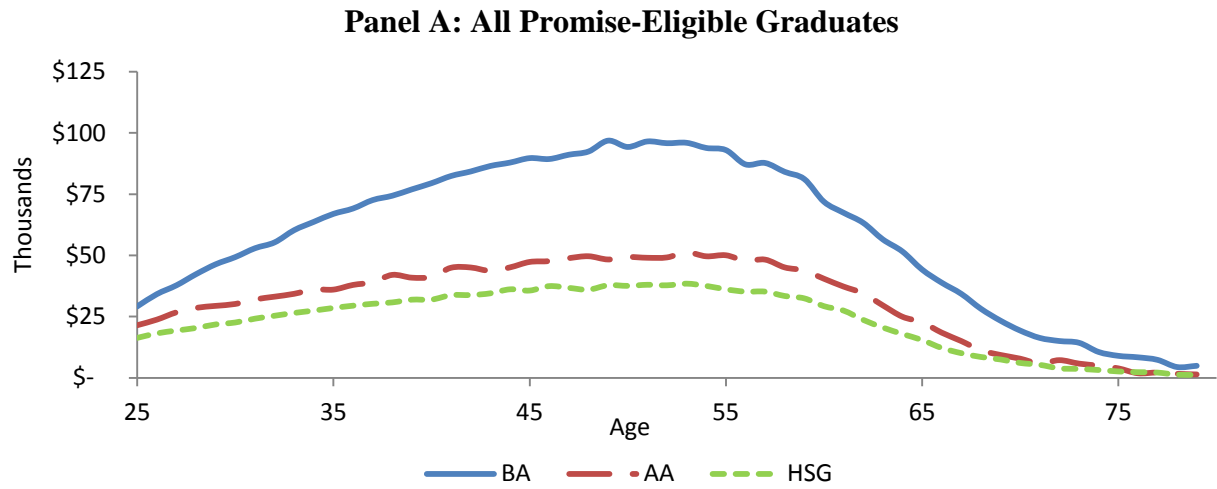
Before turning to the heterogeneous benefit-cost estimates, which are based on differential Promise treatment effect, differential Promise costs by group, and differential earnings by group, we review what the estimated earnings paths imply for the returns to education by group.

Using the procedures outlined above, we calculate career earnings paths by educational attainment for each group (by income, race, and gender). These projected earnings paths are calibrated to a Promise graduate from 2006 and incorporate all adjustments except discounting to present value. For comparison, we also provide an overall average earnings path for all Promise-eligible graduates for the same ages. These calculations are shown in Figure 2, Panels A–G.

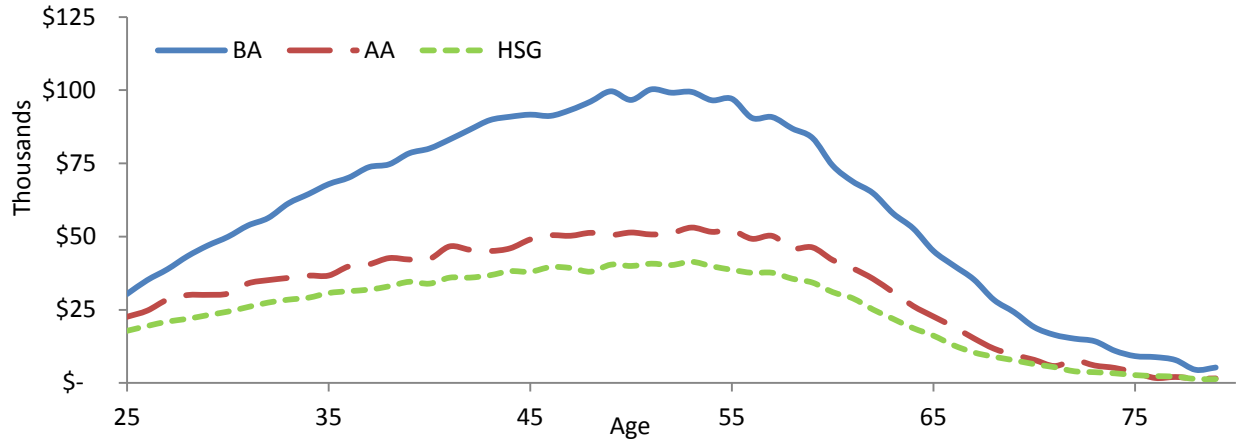
Table 5 summarizes the present discounted value of earnings by educational category for different groups, including the projected educational premia in absolute and proportional terms. Figure 2 and Table 5 show, as one would expect, that there are high earnings returns to increased educational attainment, and these returns are especially high for obtaining at least a bachelor's degree. The average increase in the present value of earnings from getting a bachelor's degree, relative to no degree at all, is over 140 percent. These proportional returns are equally as high for most groups, with one notable exception: individuals who lived in a lower-income family during

²³ Because the PSID data do not permit us to calculate such ratios beyond age 62, we carry forward the age 62 ratio for ages 63 through 79 in the ACS. Additionally, because we do not observe separate life tables for those who grew up poor and those who did not, we apply the overall population life table estimates to both income groups when adjusting for mortality. Consequently, we likely overstate (understate) mortality-adjusted earnings for the low-income (non-low-income) group. Bosworth and Burke (2014) and the references therein document growing socioeconomic gaps in life expectancy. Oreopoulos and Salvanes (2011) suggest that greater education causally improves health and possibly life expectancy; such forces would tend to counteract the understatement of lifetime earnings for the low-income group.

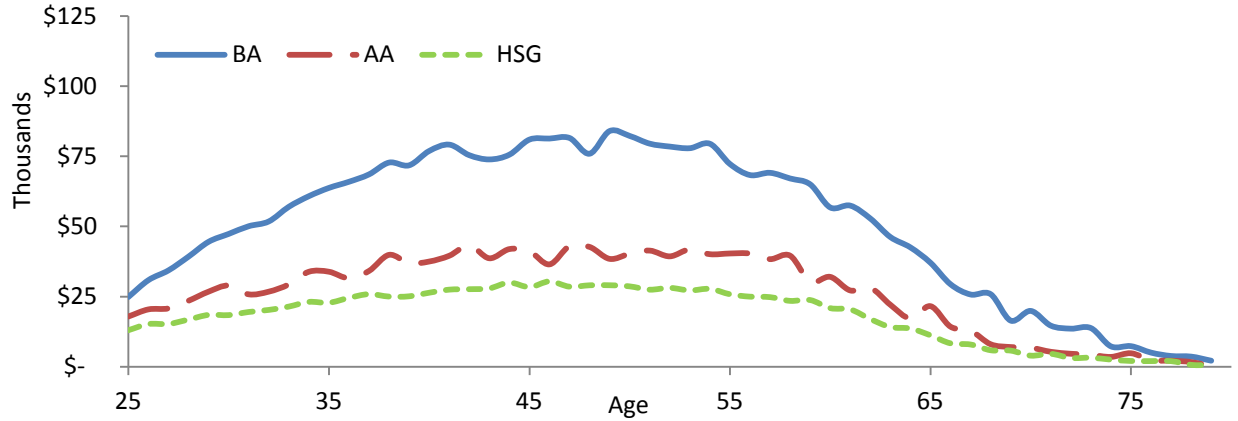
Figure 2 Projected Future Earnings, Ages 25–79



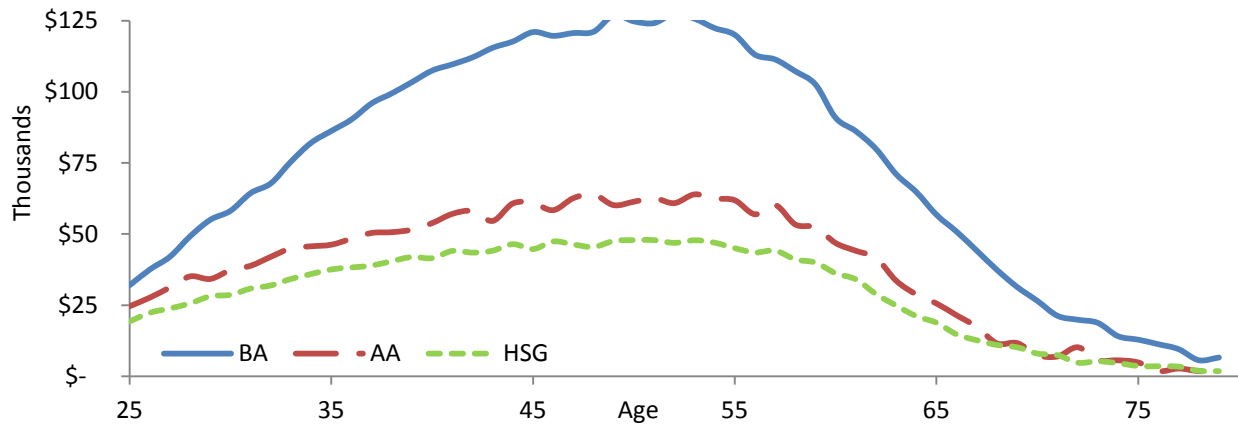
Panel D Promise-Eligible Graduates Who Are White, Non-Hispanic



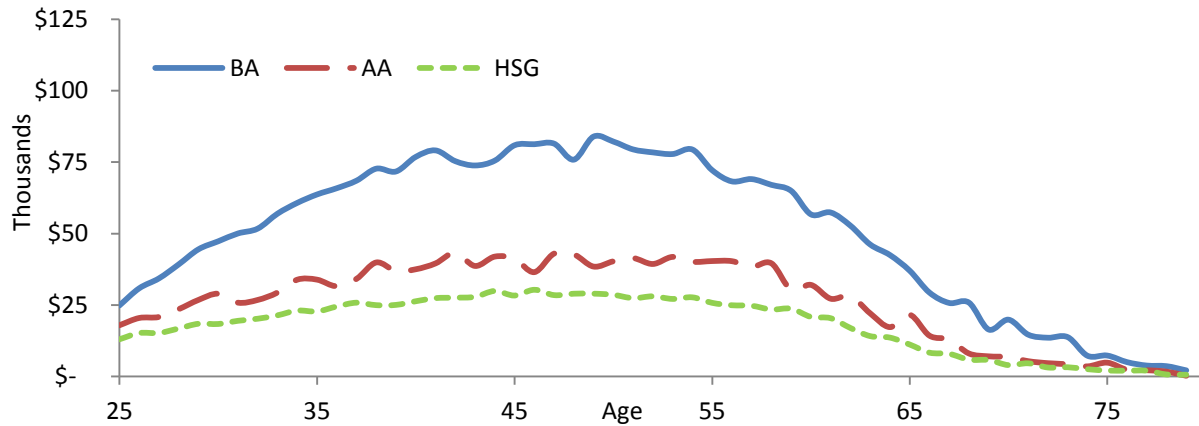
Panel E Promise-Eligible Graduates Who Are Nonwhite or Hispanic



Panel F Promise-Eligible Graduates Who Are Male



Panel G Promise-Eligible Graduates Who Are Female



NOTE: Earnings projections are based on procedures outlined in text. Earnings are in 2012 dollars, calibrated for high school graduates from the class of 2006, and assume 1.2 percent secular annual real earnings increases and mortality rates from age 18 on. Educational attainment is highest credential attained, except BA includes higher degrees. High school graduates exclude GEDs but include some college without a degree.

high school. For these individuals, the observed bachelor's premium is just under 70 percent—half what it is on average. For individuals whose family was not low-income while they were in high school, the premium to a bachelor's degree exceeds 130 percent.

The finding that individuals who come from low-income families have lower returns to education obviously deserves further investigation (in a separate paper). We speculate that these differentials could be due to differences in unmeasured skills (both cognitive aptitude and socioemotional), the quality of job networks, the types of colleges attended and fields studied, the likelihood and types of graduate and professional schools attended, the occupations and industries pursued, mental or physical health, criminal activity, regional location, and neighborhoods. Whatever the causes, these differences in the return to education across individuals of different income backgrounds should be included in estimating benefit-cost ratios of scholarship programs like the Kalamazoo Promise.

Table 5 Projected Returns to Education Credential, Overall and By Groups Defined by Past Income, Race, and Gender, Based on Present Value of Earnings as of Age 18

	Overall		
	HSG	AA	BA
PV earnings (\$)	581,000	768,900	1,396,000
AA – HS (\$)		187,800	
AA / HS (%)		32.3	
BA – HS (\$)			815,000
BA / HS (%)			140.3

	Non-low-income			Low-income		
	HSG	AA	BA	HSG	AA	BA
PV earnings (\$)	627,600	794,000	1,473,900	453,400	587,200	768,200
AA – HS (\$)		166,300			133,800	
AA / HS (%)		26.5			29.5	
BA – HS (\$)			846,300			314,800
BA / HS (%)			134.8			69.4

	White			Nonwhite		
	HSG	AA	BA	HSG	AA	BA
PV earnings (\$)	619,200	796,300	1,429,800	455,500	655,000	1,236,100
AA – HS (\$)		177,100			199,500	
AA / HS (%)		28.6			43.8	
BA – HS (\$)			810,600			780,600
BA / HS (%)			130.9			171.4

	Male			Female		
	HSG	AA	BA	HSG	AA	BA
PV earnings (\$)	736,400	952,200	1,792,500	423,500	634,700	1,035,000
AA – HS (\$)		215,800			211,200	
AA / HS (%)		29.3			49.9	
BA – HS (\$)			1,056,000			611,500
BA / HS (%)			143.4			144.4

NOTE: Present value, rounded to nearest hundred, is calculated as of age 18 and based on a 3% discount rate. Discounted career earnings cover ages 25–79, adjusted for secular earnings increases and mortality as described in the text. PV stands for present value. The rows beginning with “AA – HS (\$)” or “BA – HS (\$)” indicate the difference in the present value of earnings between the first education level and the second; the rows beginning with “AA / HS (%)” or “BA / HS (%)” indicate the percentage gain in the present value of earnings between the first education level and the second. HSG includes regular high school diplomas and those with some college but no postsecondary degree. AA includes associate’s degree holders with no higher degree. BA includes bachelor’s degree holders and holders of higher degrees.

In contrast, different racial groups have similar dollar benefits from higher educational attainment. Because nonwhites have lower overall earnings, the similar absolute benefits from higher educational attainment translate to higher percentage benefits. The contrast between educational returns across family income history and those across race emphasizes that poverty

and minority race are not symmetrical forms of disadvantage that always and everywhere have the same effects.

Finally, women and men tend to have similar percentage returns to a bachelor's degree, but women have lower absolute returns. These lower dollar returns may reflect a greater probability for women to leave the labor force, which both immediately lowers average earnings and leads to lower earnings in the future due to less work experience and work tenure. In contrast, for associate's degrees, women have somewhat higher percentage returns than men but similar dollar returns. The higher percentage returns to associate's degrees for women may reflect the higher return to some health-related occupations that are dominated by women and require associate's degrees.

We again stress that these education differentials are correlational. However, the research literature has supported that much—if not all—of the differences in cross-sectional earnings across education levels reflects causal effects. For example, Card (1999) concludes that the estimates of returns to education based on correlational studies have a 10 percent upward bias compared to twin studies. On the other hand, instrumental-variables studies using educational reforms and admission scores cutoffs as sources of exogenous variation suggest that the correlational relationship is biased *downward* by 20–40 percent for affected individuals. Nonetheless, in Appendix A we consider the implications of modifying our calculations of the returns to education to instead draw from causal estimates from recent, highly regarded studies. This modification ends up having little impact, either on benefit/cost ratios or rates of return.

Benefits vs. Costs of the Kalamazoo Promise, by Group

We combine the estimated returns to education by group and the estimated Promise costs by group to calculate benefit-cost ratios and “internal” rates of return.²⁴ We follow two different scenarios. In scenario 1, we assign the group-specific Promise effects on educational attainment from Table 2. These effects are imprecisely estimated, however, and we cannot reject the hypothesis that effects are the same across the different income groups or the different race groups. Therefore, we also consider scenario 2, which restricts each group to have the same educational attainment effects, as shown in Table 1, column 2. Even though scenario 2 assumes the same Promise effects on educational attainment for all groups, estimates of Promise benefits and costs still greatly vary by group due to differing returns to education and costs.²⁵ Table 6 shows how each of these scenarios affects the distribution of educational attainment for each of the population samples.

Figure 3 describes how, under both scenarios, the earnings paths for the various groups change because of the Promise. Using these earnings paths, Table 7 reports the present value of benefits, the present value of costs, their net difference, and their ratio. The table also reports the “rate of return” for each group under the two scenarios. This rate of return is the highest discount rate under which the present value of benefits is equal to or exceeds the present value of costs. It is the highest discount rate under which this policy would be worth pursuing for that group if policymakers cared only about earnings benefits and scholarship costs.

²⁴ We slightly abuse the language: the internal rate of return we mean here is the discount rate that equates the discounted stream of benefits (earnings) with the discounted stream of costs.

²⁵ In addition to the estimates in Tables 1 and 2, we require one more datum: the share of non-bachelor credentials for each group that are associate’s degrees. Estimating this outcome directly is not helpful, as effects are small and the sample size does not permit meaningful precision. Instead, for each group, we assume the *marginal* share of associate degrees equals the observed *average* share for that group among Promise eligibles in the post-Promise period. Although this assumption may not hold strictly, the benefit-cost results are not sensitive to reasonable variations, as most of the returns to the Promise are due to effects on bachelor’s degrees.

Table 6 Different Scenarios for Promise Effects on Educational Attainment, by Group

	Promise effects by income group					
	Non-low-income			Low-income		
	HSG	AA	BA	HSG	AA	BA
Pre-Promise attainment distribution	0.571	0.031	0.398	0.849	0.047	0.104
CF1: Group-specific Promise effects	0.464	0.050	0.487	0.775	0.062	0.163
CF2: Homogeneous Promise effects	0.458	0.047	0.495	0.736	0.063	0.201

	Promise effects by race					
	White			Nonwhite		
	HSG	AA	BA	HSG	AA	BA
Pre-Promise ratios	0.567	0.035	0.398	0.825	0.071	0.104
CF1: Group-specific Promise effects	0.564	0.012	0.424	0.717	0.106	0.177
CF2: Homogeneous Promise effects	0.453	0.052	0.495	0.712	0.087	0.201

	Promise effects by gender					
	Male			Female		
	HSG	AA	BA	HSG	AA	BA
Pre-Promise ratios	0.659	0.047	0.294	0.672	0.031	0.297
CF1: Group-specific Promise effects	0.668	0.041	0.291	0.510	0.060	0.430
CF2: Homogeneous Promise effects	0.545	0.064	0.391	0.559	0.047	0.394

NOTE: Pre-Promise distributions are based on observed percentages in each educational attainment group prior to the Promise (classes of 2003 through 2005) among students who would have been eligible for the Promise had it existed then. CF1 (counterfactual 1) considers a post-Promise scenario that assumes differential Promise effects on educational attainment across groups, as reported in the estimates in Table 2. CF2 (counterfactual 2) instead imposes the same Promise effect on educational attainment across all groups, based on the estimates in Table 1, column 2.

For both income groups, under either scenario, the Promise has a favorable benefit-cost ratio and high rates of return. Regardless of family income, students get future earnings benefits that are much higher than scholarship costs under reasonable discount rates. All the benefit-cost ratios exceed 2, the differences between benefits and costs always exceed \$10,000 per student, and the real rate of return always exceeds 6 percent.

Figure 3A Kalamazoo Promise Earnings Benefits, by Income Group: Differential Promise Effects

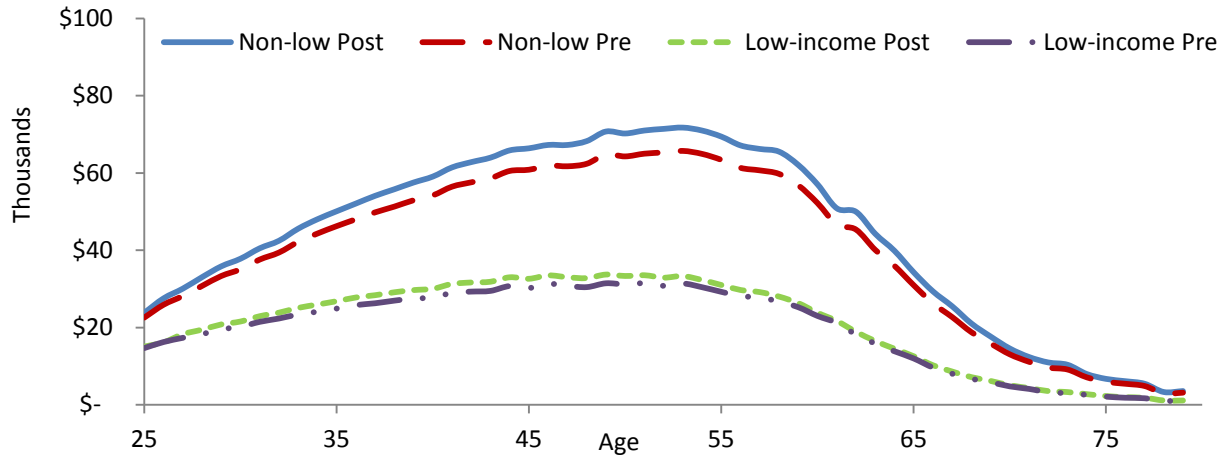


Figure 3B Kalamazoo Promise Earnings Benefits, by Income Group: Same Promise Effects

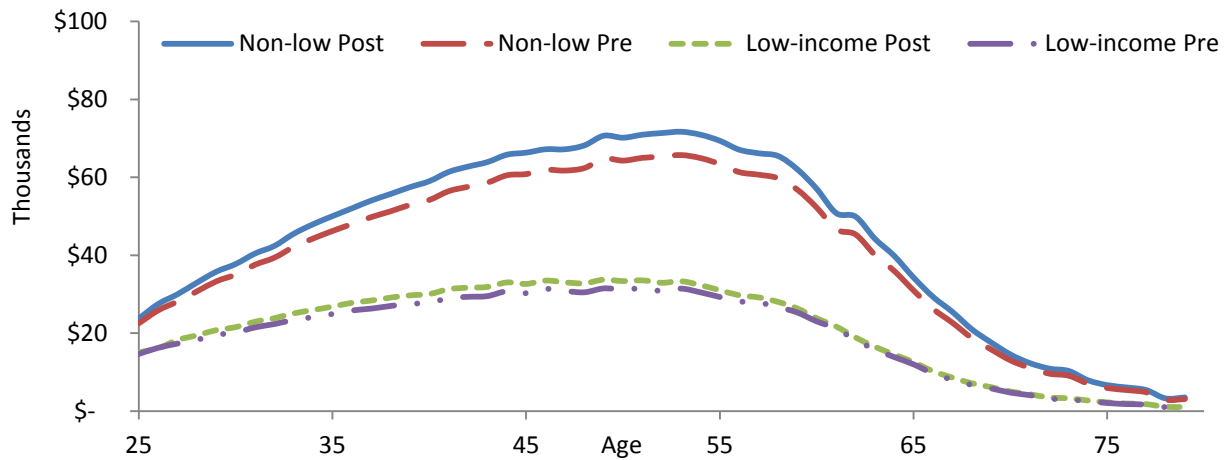


Figure 3C Kalamazoo Promise Earnings Benefits, by Race: Differential Promise Effects

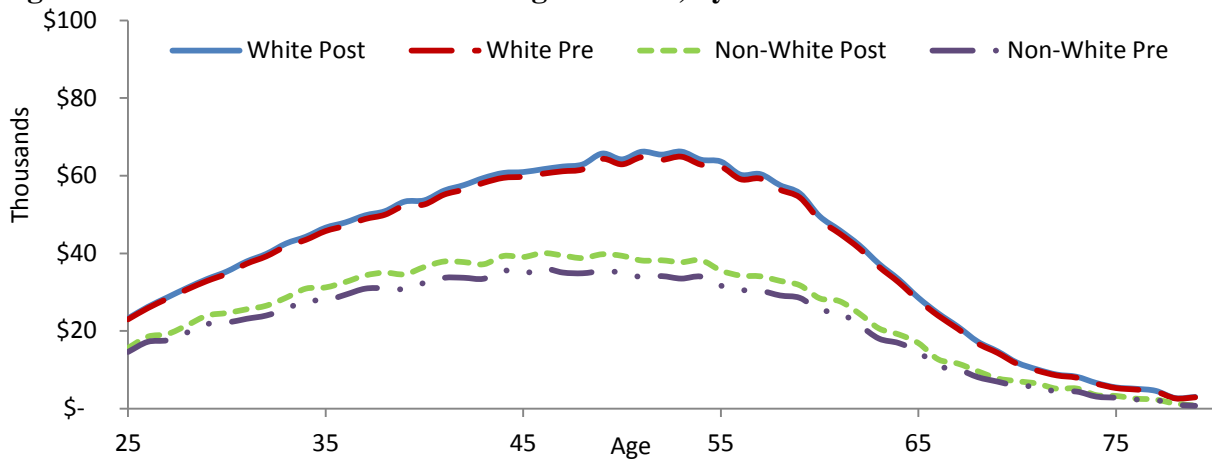


Figure 3D Kalamazoo Promise Earnings Benefits, by Race: Same Promise Effects

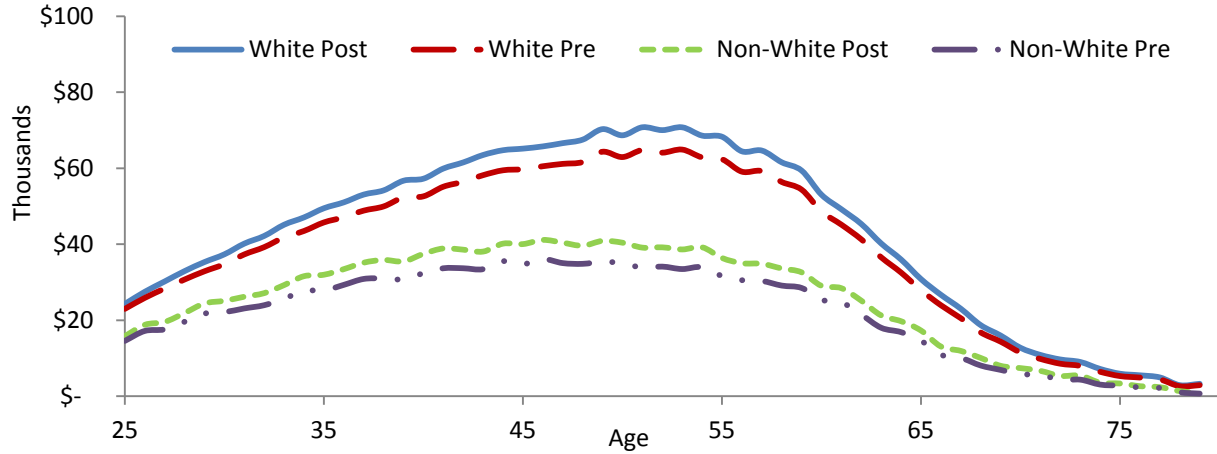


Figure 3E Kalamazoo Promise Earnings Benefits, by Gender: Differential Promise Effects

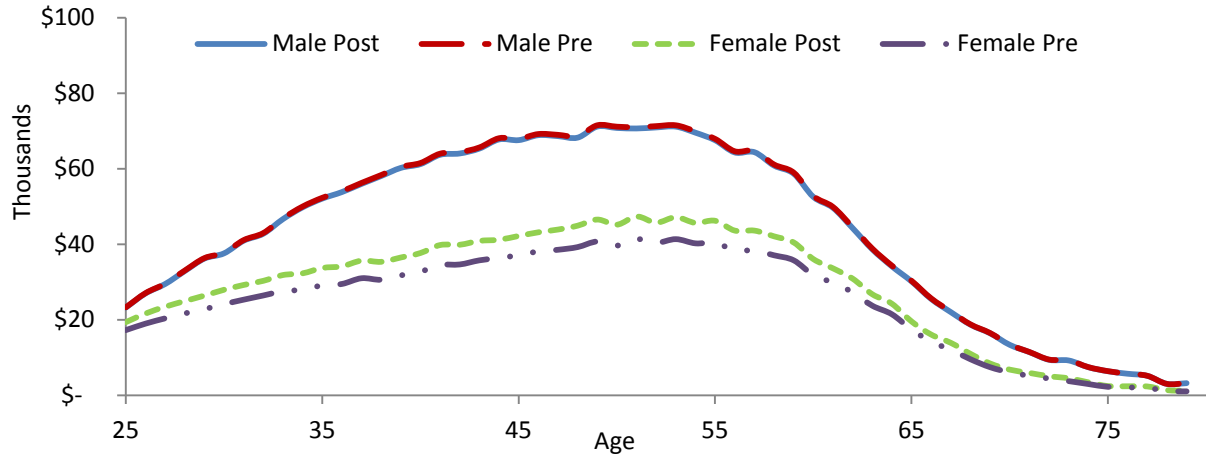
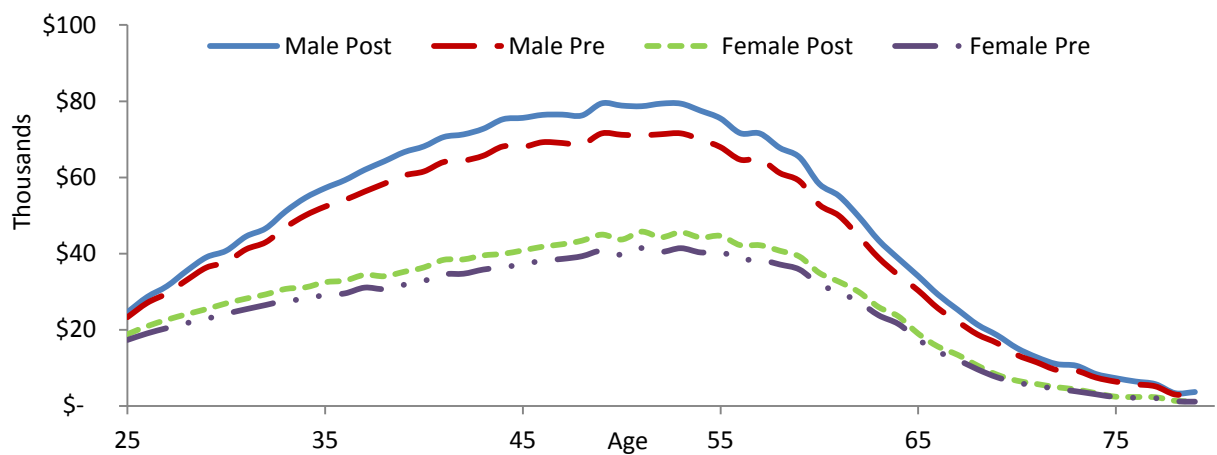


Figure 3F Kalamazoo Promise Earnings Benefits, by Gender: Same Promise Effects



NOTE: Figures show earnings path with and without the Promise, for groups differing by past family income, race, and gender. Scenario 1 (top graph) assumes Promise effects on education attainment vary across groups; scenario 2 (bottom graph) assumes Promise effects on educational attainment are the same across groups. Both scenarios assume that earnings effects on educational attainment for the different groups are as described in the text.

Table 7 Benefit-Cost Analysis of the Promise, by Demographic Groups

Original aggregate results				
Benefits (\$)	82,083			
Costs (\$)	17,620			
Benefits/costs	4.66			
Benefits minus costs (\$)	64,463			
Rate of return (%)	11.3			
	Different Promise effects by group (Scenario 1)		Same Promise effects by group (Scenario 2)	
	Non-low-income	Low-income	Non-low-income	Low-income
Benefits (\$)	78,368	20,681	84,769	32,693
Costs (\$)	24,018	9,924	24,018	9,924
Benefits/costs	3.26	2.08	3.53	3.29
Benefits minus costs (\$)	54,350	10,756	60,751	22,768
Rate of return (%)	8.7	6.8	9.2	9.7
	White	Nonwhite	White	Nonwhite
	Benefits (\$)	16,929	63,816	81,481
Costs (\$)	22,608	11,891	22,608	11,891
Benefits/costs	0.75	5.37	3.60	6.64
Benefits minus costs (\$)	-5,679	51,925	58,873	67,038
Rate of return (%)	1.9	12.4	9.4	14.0
	Male	Female	Male	Female
	Benefits (\$)	-4,522	87,427	105,913
Costs (\$)	16,775	18,419	16,775	18,419
Benefits/costs	-0.27	4.75	6.31	3.41
Benefits minus costs (\$)	-21,297	69,008	89,137	44,297
Rate of return (%)	NA	12.2	12.7	9.8

NOTE: For each group and scenario, the table reports present value of benefits, present value of costs, ratio of present value of benefits to costs, and present value of benefits minus costs. (All present value calculations use a 3% real discount rate.) The rate of return is the maximum discount rate at which the present value of benefits is equal to or exceeds the present value of costs. Scenario 1 assumes the educational attainment effects of the Promise differ by group; scenario 2 assumes the educational attainment effects are the same for both groups, as assumed in the original aggregate calculations of Bartik, Hershbein, and Lachowska (2015).

However, the benefit-cost picture is somewhat less favorable for the low-income group than would be implied by the aggregate analysis. This is a direct consequence of the lower estimates of the returns to education for students from low-income families. As a result, the (weighted) average benefits for both groups combined are somewhat lower—though still considerable—than the original aggregate estimates.²⁶

²⁶ Using weights proportional to the sample counts in Table 2, increased earnings average \$53,324 (\$62,161 under Scenario 2), average costs are \$17,900, average benefit-cost ratio is 2.98 (3.47), average net-of-costs benefits are \$35,425 (\$44,261), and the average rate of return is 8.3 percent (9.3 percent).

For nonwhites, the Promise has very high benefit-cost ratios and high rates of return. Benefit-cost ratios under either scenario exceed 5 to 1, the rate of return is over 12 percent, and net-of-costs benefits per student exceed \$50,000.

These high ratios and rates of return occur for two reasons. First, Promise costs for nonwhites are low. Relatively few nonwhites, even with the Promise, obtain a bachelor's degree, and this keeps average Promise spending per nonwhite student relatively low.²⁷ Second, the returns to educational attainment for nonwhites are quite high.

For whites, the Promise's benefits differ greatly across scenarios. When group effects are allowed to differ (scenario 1), educational attainment effects of the Promise are small for whites. These small education gains translate into small earnings increases, even though whites have high returns to educational attainment. On the cost side, because a large share of white students earned a bachelor's degree even before the Promise, scholarship outlays are high. The Promise spends a great deal of money providing financial aid to whites who would have completed a bachelor's degree without the Promise. Promise scholarships may benefit these white graduates by reducing their debt burden, for example, but it does not boost their bachelor degree attainment and thereby their earnings.²⁸ Consequently, at a discount of 3 percent, Promise earnings benefits for whites are less than scholarship costs; the rate of return that equalizes the two is less than 2 percent.

However, although the point estimates of Promise educational attainment effects are quite different for whites and nonwhites, this difference is not statistically significant. If we restrict the two racial groups to have the same Promise effect (scenario 2), then whites once again show a

²⁷ It follows that a greater share of students of color than white students are marginal degree completers.

²⁸ The Promise does, however, increase the selectivity of the institution of attendance and graduation, and selectivity may increase earnings (Hoekstra 2009). We do not explore earnings effects by selectivity of college in this paper but plan to do so in the future.

quite large benefit-cost ratio, high net benefits per student, and a high rate of return. Under this scenario, the Promise provides white students with a benefit-cost ratio exceeding three, net benefits per student of almost \$60,000, and a rate of return of over 9 percent.²⁹ In sum, the Promise's high benefits for nonwhites are robust across different scenarios, whereas the benefits for whites are sensitive to which Promise effect estimates are used.

For women, the Promise has large net benefits in either scenario. Under scenario 1, benefits exceed costs by more than \$69,000, which corresponds to a benefit-cost ratio of 4.75 and a rate of return in excess of 12 percent. This high rate of return is mostly driven by the sizable educational attainment results for women found in Table 2, but even under homogeneous Promise effects (scenario 2), women experience a high return per dollar spent.

For men, if we allow Promise education effects to vary by gender (scenario 1), the Promise has no positive earnings benefits. Under this scenario, the Promise does not improve educational attainment for men, but it has large costs because of the high baseline number of men attending college and getting degrees. We calculate high returns for men if we restrict Promise effects across gender to be the same (scenario 2); however, this restriction is rejected by our data.³⁰

Given these results, it would be of interest to examine the effects of the Promise for narrower subgroups—for example, for low-income nonwhite men or higher-income white women. Unfortunately, such finer breakdowns are precluded by our modest sample size.

²⁹ Using weights proportional to the sample counts in Table 2 for whites and nonwhites, increased earnings average \$38,880 (\$80,286 under scenario 2), average costs are \$17,591, average benefit-cost ratio is 2.21 (4.56), average net-of-costs benefits are \$21,289 (\$62,695), and the average rate of return is 6.8 percent (11.0 percent).

³⁰ Using weights proportional to the sample counts in Table 2 for men and women, increased earnings average \$43,494 (\$83,355 under scenario 2), average costs are \$17,633, average benefit-cost ratio is 2.47 (4.73), average net-of-costs benefits are \$25,861 (\$65,722), and the average rate of return is 7.7 percent (11.3 percent).

CONCLUSION

We find that the Kalamazoo Promise has high benefit-cost ratios and rates of return for different income groups, for nonwhites, and for women, and these effects are robust to reasonable alternative assumptions. In contrast, whether the Promise has net earnings benefits for whites and for men is more sensitive to the assumptions made about Promise effects.

What implications does this have for policy debates on the relative merits of universal and targeted scholarships? Aside from the legal and ethical difficulties in trying to explicitly target a Promise-style scholarship on the basis of race or gender, we note that the Promise effects we have estimated are in the context of a program that is *not* targeted by group. The lack of group targeting makes the Kalamazoo program simpler and easier to explain, and probably elicits greater public support for the program. These factors likely play some role in the program's effects.

Our findings might be used to rationalize scholarship programs that are “universal” in that they target all students in a school district, but “targeted” on school districts that have a high percentage of nonwhite or low-income students. Such districts will have more modest scholarship costs per student because of low baseline rates of college attendance and persistence. Yet, particularly for nonwhite students, the expected earnings return to increasing educational attainment is quite high.

Our results also point to the importance of disaggregating analyses of educational policies by socioeconomic or demographic group. The rates of return to educational interventions can vary greatly by group, sometimes in surprising ways. For example, differences between disadvantaged and advantaged income groups may not carry over to differences between disadvantaged and advantaged racial groups. Furthermore, because aggregate earnings measures

may contain a different composition of groups than the sample populations of education policy interventions, their use can lead to biased benefit-cost ratios when there are heterogeneous treatment effects or heterogeneous returns to education.³¹

Overall, our refined benefit-cost analysis suggests that the Kalamazoo Promise has high benefits relative to costs, with average benefits exceeding costs by a ratio of at least two to one. Even with a benefit measure that omits nonpecuniary benefits of education, and a cost measure that ignores how scholarships help reduce student and family debt, we find that the universal college scholarship of the Promise, for a wide variety of groups, easily passes a benefit-cost test.

³¹ This is why weighted averages of subgroup net-benefit measures do not equal the aggregate net-benefit measures. Since it is common in the literature to use aggregate earnings, as individual life-cycle earnings are rarely observable, the presence of heterogeneity has large implications on benefit-cost measurement for social programs.

APPENDIX A

ALTERNATIVE “CAUSAL” BENEFIT-COST ANALYSIS OF THE KALAMAZOO PROMISE

In this appendix, we modify the benefit-cost analysis to base the returns to education on causal estimates drawn from the literature rather than on the cross-sectional differences used in the paper. Compared to cross-sectional estimates, these causal estimates are slightly lower. As a result, the benefit-cost ratios and rates of return for the Promise are slightly reduced. However, these reductions do not significantly alter the paper’s conclusions.

We base our causal estimates of education returns on Zimmerman (2014) for bachelor’s degrees and Bahr et al. (2015) for associate’s degrees. Both are recent studies, employ credible identification, and find causal estimates that reasonably accord with the education research literature. They also include persons with zero earnings in at least some of their estimates, whereas some other studies focus on earnings effects only among those with positive earnings. However, neither paper provides causal estimates for all of the demographic groups examined in the current paper, so we rely on aggregate estimates.

Zimmerman’s (2014) causal estimates rely on a regression discontinuity analysis of bachelor’s degree attainment using administrative data from Florida. Specifically, he compares earnings (measured around age 30) and bachelor’s degree attainment among students just above and just below the high school GPA requirement for admission to the Florida state university with the weakest state admission requirements. The ratio of these discontinuities is an estimate of the causal effects of bachelor’s degree attainment on earnings for the marginal student who “barely” gets accepted at the least selective state university. This local average treatment effect (LATE) can then be compared with the predicted earnings of students just below the high school

GPA cut-off to yield a causal estimate of the percentage effect of bachelor's degree attainment on earnings.³²

Bahr et al.'s (2015) causal estimates rely on individual fixed effects of students in Michigan. Specifically, they compare quarterly earnings before and after an individual earns an associate's degree.³³ These estimates are average treatment effects on the treated, as they represent the earnings increases for individuals who obtain degrees (and not all individuals who *could* obtain degrees.)

For bachelor's degree attainment, Zimmerman's (2014) estimates suggest an earnings premium of 90.4 percent between ages 26 and 32. For these same ages, our cross-sectional estimate is that a bachelor's degree increases earnings by 97.2 percent.³⁴ The ratio of Zimmerman's causal estimate to our cross-sectional estimate is 93.0 percent (90.4 / 97.2). We thus adjust the cross-sectional bachelor's degree earning premium over high school by multiplying by 0.93 for all groups and all years of age.

For associate's degree attainment, Bahr et al. (2015) estimate causal effects (in 2012 dollars) for annual earnings of \$5,883 for men and \$9,578 for women. For similar ages, our cross-sectional estimates show associate's degree holders have an earnings premium over high school diploma holders of \$6,519 for men and \$7,416 for women.³⁵ Therefore, the causal

³² Zimmerman (2014) reports a quarterly earnings effect of \$6,547 when scaled by the increased likelihood of earning a bachelor's degree. Expected earnings just below the high school GPA threshold are \$7,241. Under the conservative assumption that students just below the cutoff do not earn a postsecondary degree, the premium to bachelor's degree attainment is 90.4 percent. (If some of these students do complete a degree, and that boosts their earnings, the bachelor's premium relative to high school diploma will actually be higher than 90.4 percent.)

³³ Bahr et al.'s (2015, Table 4) fixed effect estimates find causal effects of an associate's degree on quarterly earnings (in 2011 dollars) of \$2,346 for females and \$1,441 for males.

³⁴ This calculation compares the sum of average earnings for all persons aged 26–32 who have a bachelor's degree with the sum of average earnings for all persons of the same age who have a high school diploma (or some college) but no higher degree.

³⁵ Bahr et al. (2015) report the percentage of community college enrollees by broad initial age categories: less than 20, 57 percent; 20–26, 20 percent; 27–45, 20.3 percent; 46–60, 2.7 percent. Earnings are reported up to eight years after initial enrollment. To derive similar ages, we assume initial enrollment at the midpoint of the four

estimates for men are 90.2 percent of the cross-sectional estimates, and the causal estimates for women are 129.1 percent of the cross-sectional estimates. For the gender-specific analysis, we use these separate estimates to adjust at all ages the differential between associate's degree holders and high school degrees down for men (multiplying by 0.902) and up for women (multiplying by 1.291). For the overall sample and for the racial and income groups, we combine Bahr et al.'s estimates for men and women. In our cross-sectional data, 42.8 percent of associate's degree holders are men and 57.2 percent are women. Applying these weights, the causal estimates are 113.7 percent of the cross-sectional estimates. For the overall sample, and for the groups differentiated by race or family income status, we multiply the observed associate's degree premium over high school by 1.137 for all ages.

These calculations illustrate that causal estimates of the returns to education do not differ much from cross-sectional estimates of the returns to education. This is in line with the conclusions of Card (1999).

Our earlier study found the largest effects of the Promise on bachelor's degree—not associate's degree—attainment. In addition, both the absolute and proportional gains in earnings from credential attainment are much higher for bachelor's degrees than for associate's degrees. Consequently, our Promise benefit-cost analysis is dominated by the returns to a bachelor's degree. We would therefore expect that using causal estimates for the returns to education would reduce the Promise's benefit-cost ratios and rates of return. However, these expected reductions

age ranges (ages 19, 23, 36, and 53). We assume most earnings effects will be measured as of four to eight years after initial enrollment. Therefore, we look at the earnings gains from associate's degrees in our cross-section data for ages 23–27, 27–31, 40–44, and 57–61. The average earnings gain for each of these four age ranges is weighted by the percentage in each age category to get a weighted average that corresponds roughly to the ages considered in the Bahr et al. (2015) study.

will be slight, as the causal estimates adjust down the return to a bachelor's degree by only 7 percent.

Appendix Table A.1 confirms these expectations. This table has the same format as Table 7, but it calculates earnings benefits using the causal education estimates described above. The two tables show a similar pattern of estimates across groups; the benefits are just slightly lower in Table A.1. The same conclusions remain: the Kalamazoo Promise has a large benefit-cost ratio and a high internal rate of return for the overall sample, for both low-income and non-low-income students, for nonwhite students, and for female students, and these are robust across treatment effect scenarios. Benefits for whites and males are more sensitive to assumptions.

We regard these causal estimates as supplementary to our main text estimates because we do not have reliable causal estimates for all the subgroups we consider. We thus necessarily apply uniform percentage adjustments for different groups when using the causal estimates, which may not be accurate. However, these alternative estimates suggest that the overall picture presented by our main text estimates broadly holds.

Table A.1 Benefit-Cost Analysis of the Promise, using Causal Education Estimates

Aggregate results				
Benefits (\$)			76,972	
Costs (\$)			17,620	
Benefits/costs			4.37	
Benefits minus costs (\$)			59,352	
Rate of return (%)			10.9	
	Different Promise effects by group (Scenario 1)		Same Promise effects by group (Scenario 2)	
	Non-low-income	Low-income	Non-low-income	Low-income
Benefits (\$)	73,494	19,159	79,354	29,998
Costs (\$)	24,018	9,924	24,018	9,924
Benefits/costs	3.06	1.93	3.30	3.02
Benefits minus costs (\$)	49,476	9,234	55,336	20,073
Rate of return (%)	8.4	6.4	8.8	9.1
	White	Nonwhite	White	Nonwhite
Benefits (\$)	14,888	60,770	76,375	74,077
Costs (\$)	22,608	11,891	22,608	11,891
Benefits/costs	0.66	5.11	3.38	6.23
Benefits minus costs (\$)	(7,720)	48,878	53,768	62,186
Rate of return (%)	1.4	12.1	9.0	13.5
	Male	Female	Male	Female
Benefits (\$)	-4,086	81,411	95,756	58,023
Costs (\$)	16,775	18,419	16,775	18,419
Benefits/costs	-0.24	4.42	5.71	3.15
Benefits minus costs (\$)	-20,862	62,992	78,981	39,604
Rate of return (%)	NA	11.6	12.1	9.2

NOTE: This table mirrors Table 7 except that education returns are adjusted downward to reflect “causal” effects of education credentials on earnings, following the procedures described in Appendix A. For additional notes, see Table 7.

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