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The Merits of Universal Scholarships: Benefit-Cost Evidence from the Kalamazoo Promise
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ABSTRACT

As higher education costs rise, many communities have begun to adopt their own financial aid strategy: place-based scholarships for students graduating from the local school district. In this paper, we examine the benefits and costs 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, scholarship cost data, and projections of future earnings by education, we examine the Promise's benefit-cost ratios for students differentiated by income, race, and gender. Although the average rate of return of the program is 11 percent, rates of return vary greatly by group. The Promise has high returns for both low-income and non-low-income groups, for non-whites, 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. They also highlight the importance of disaggregating benefits and costs by subgroup when performing benefit-cost analysis when the treatment is heterogeneous.

JEL Classification Codes: I21, I22, I24

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

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I. INTRODUCTION

Many voters and policy analysts perceive increased college completion rates as a high return investment, for both individual students and the U.S. However, increased college completion is burdened by rising college costs. These rising costs have sparked questions about whether traditional college financial aid policies—usually targeted on financial need, merit, or both—can increase college completion in a way that is both effective and efficient. Are such policies "effective," sizably increasing overall college completion rates? Are such policies "efficient," yielding large increases in college completion relative to costs?

On the one hand, need-based financial aid, such as the federal Pell grant program, targets groups that are under-represented among college completers. Such aid is complicated to administer at scale, lowering take-up rates and limiting its effectiveness in reaching students. In addition, such aid often goes to students who do not complete college, reducing its efficiency. On the other hand, merit-based financial aid that ignores need is often used by students who would have gone to college anyway (Cornwell and Mustard 2006, 2007), which limits merit aid's efficiency and effectiveness in increasing college completion. Targeting both need and merit, by targeting the low-income, academically-ready population, is likely efficient, boosting marginal college completion by a large amount relative to costs. But such tightly targeted aid reaches relatively few people, as the population that is both low income and academically ready is small.[1]

[1] These issues have arguably led to discussion and experimentation with "free college" programs, typically covering tuition and fees for 2-year colleges, *after* other grant aid has been received (such as Tennessee's program, http:/tennesseepromise.gov, begun in Fall 2015, or Oregon's program, beginning in Fall 2016). Such programs generally require neither academic merit criteria nor economic means-testing, making them simple to administer and apply for. However, it is unclear how the relatively small level of benefits will translate into either effectiveness or efficiency.

Place-based college scholarships can provide empirical evidence on the benefits and costs of different scholarship designs. These scholarship programs, of which several dozen currently exist in 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 such programs have merit or need requirements, some do not. In particular, the Kalamazoo Promise scholarship has no achievement or need requirements. Moreover, it pays up to 100 percent of four years of college tuition and is easy to understand and apply for. Therefore, the Kalamazoo Promise 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 the companion paper, we find that the Promise increases college completion, but heterogeneously by demographic group. In this paper, we incorporate these college completion results into a benefit-cost model that analyzes the benefits and costs of the Promise for groups defined by family income, ethnicity, and gender.[2] This requires projections by group of earnings effects of a college education, as well as analysis of Promise costs by group. Our analysis shows that the Promise has high benefit-cost ratios and rates of return for both low-income and non-low-income groups, for nonwhites, and for women. Although 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 aggregate rate of return, we conclude that

[2] Our companion paper briefly in the conclusion referred to one of the overall benefit-cost ratios we present here, but without exploring these benefit and cost estimates in detail or examining heterogeneity. The current paper is the first in-depth analysis of benefit-cost results for the Kalamazoo Promise for different groups.

the Kalamazoo Promise easily passes a benefit-cost test. Hence, the lack of targeting of merit or need by the Kalamazoo Promise does not inhibit the program from having high cost-effectiveness. Furthermore, its universality means that by definition the Kalamazoo Promise operates at a large scale, with wide take-up. The Promise's high returns for both low-income and non-low-income groups suggest that broad-based financial aid programs, under certain conditions, can cost-effectively boost college completion for a wide range of students.

The remainder of this paper is organized as follows. The next section provides background on the Kalamazoo Promise, and reviews its impact on college completion. We then describe how we use these effects and other data to calculate costs and earnings benefits for each subgroup of family income, race, and gender. We consider the sensitivity of these benefit-cost estimates to alternative assumptions. We discuss these results' implications for proposed scholarship designs. Finally, we conclude by arguing for the importance of disaggregated analysis of the impacts of financial aid programs, as it can lead to surprising conclusions.

II. THE IMPACTS OF THE KALAMAZOO PROMISE ON COLLEGE SUCCESS

For our benefit-cost analysis of the Kalamazoo Promise, the following features of the Promise are most salient:

- Generous. The Promise pays for up to 130 credits of college tuition and fees at any public college or university in Michigan.[3,4]
- Universal. The Promise is awarded without academic need or merit requirements.[5]

[3] Private Michigan colleges were added in 2015, but this is irrelevant to the early cohorts examined in this paper.

[4] The Promise is also "first-dollar," with no requirement that students exhaust other aid before Promise funds can be used. In addition, students have up to 10 years after high school graduation to use their scholarships.

[5] Once in college, students must maintain a 2.0 GPA; for students who fall below this threshold, Promise eligibility can be regained if GPA is subsequently brought up.

- Place-based. The only requirements for Promise award are that the recipient must graduate from Kalamazoo Public Schools (KPS), and must have continuously attended KPS and lived in the district since at least the beginning of 9th grade.[6]
- Simple. The Promise application is a one-page form in which students provide contact information, their KPS attendance history, and where they will attend college.
- Mature program. The Promise began with high school graduating class of 2006; thus there is now considerable follow-up data on outcomes.
- High take-up. Roughly 90 percent of KPS graduates are eligible for the Promise, and more than 85 percent of eligibles have received Promise funds. The program's steady-state spending level on scholarships is roughly $12 million per year, with about 1,400 students receiving scholarships at any one time.
- Diverse students. Because KPS has considerable numbers of both low-income and middle-income students, and both nonwhite and white students, Promise outcomes can be estimated for diverse groups.
- Privately-funded. The Promise is funded by anonymous private donors. The donors' motivation was to not only help students, but to boost the Kalamazoo's economic development by enhancing the quality of the local workforce (Miller-Adams, 2009).

Previous research has found many Promise effects, including: increased KPS enrollment of all ethnic groups (Bartik, Eberts, and Huang 2010; Hershbein 2013); improved disciplinary outcomes of high school students and improved high school GPA for African-American students

[6] Promise scholarships start at 65 percent of tuition for students attending since 9th grade, and rise with length of continuous attendance, with 100 percent awards for students attending and living in KPS since kindergarten.

(Bartik and Lachowska 2013); increased student applications to more selective state public universities (Andrews, Desjardins, and Ranchhod 2010. For the current benefit-cost analysis, however, we rely mainly on results from Bartik, Hershbein, and Lachowska (2015) that estimate that the Promise has significantly increased post-secondary credential attainment rates—particularly bachelor's degree receipt—both overall and among various demographic groups. These estimates rely on a difference-in-differences strategy, comparing eligible to ineligible students, before and after the Promise took effect.[7]

Table 1 summarizes these estimated Promise effects on post-secondary attainment. The overall effects are 12 percentage points on attainment of any credential (degrees or certificate), and 10 percentage points on attainment of a bachelor's degree. Both effects are about one-third of the pre-Promise mean for a similar population.

These overall effects are larger than found in recent studies of scholarships that are more targeted. For the merit-based West Virginia PROMISE program, Scott-Clayton (2011) finds a 4–5 percentage point increase in bachelor's completion, a little more than 10 percent of the pre-treatment mean. For the need-based Florida Student Access Grant, Castleman and Long (forthcoming) find bachelor's completion effects of about 4–5 percentage points (22 percent).

For the need-based Wisconsin Scholars Grant, Goldrick-Rab et al. (2015) find a 4–5 percentage point (29 percent) increase in bachelor's attainment.[8]

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[7] The DD strategy compares eligible vs. ineligible students after the Promise with pre-Promise cohorts who *would have been* eligible or ineligible based on length of KPS enrollment.

[8] In addition, Dynarski (2008) finds a 2.5 percentage point increase in bachelor's attainment due to Arkansas and Georgia merit-based scholarships, although 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 on degree attainment from a merit scholarship in Massachusetts, which they argue results from inducing students to attend lower-quality colleges, a context that may be unique to New England.

Table 1 Promise Effects on Degree Attainment at Six Years After High School Graduation

	Any credential		Bachelor's	
	(1)	(2)	(3)	(4)
Panel A: Overall Results				
Promise effect	0.123**		0.097**	
	(0.050)		(0.045)	
Mean dependent variable	0.357		0.296	
Panel B: Income Groups	*Non-Low*	*Low*	*Non-Low*	*Low*
Promise effect	0.119	0.086	0.089	0.059
	(0.077)	(0.058)	(0.076)	(0.043)
p-value of group difference	0.733		0.723	
Mean dependent variable	0.477	0.201	0.398	0.104
Panel C: Race/Ethnicity	*White*	*Non-White*	*White*	*Non-White*
Promise effect	−0.007	0.135**	0.026	0.073
	(0.080)	(0.056)	(0.069)	(0.048)
p-value of group difference	0.146		0.570	
Mean dependent variable	0.463	0.237	0.397	0.158
Panel D: Gender	*Male*	*Female*	*Male*	*Female*
Promise effect	−0.012	0.185***	−0.003	0.133**
	(0.060)	(0.069)	(0.055)	(0.059)
p-value of group difference	0.030		0.088	
Mean dependent variable	0.336	0.376	0.294	0.297

NOTE: Source is Bartik, Hershbein, and Lachowska (2015). (Group-specific results for any credential were not reported in that paper.) Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicates *p* less than 0.01, 0.05, or 0.10. All regressions include controls for graduation year, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation (except when subgroup is restricted on one of these dimensions); results in panel A also use inverse propensity score reweighting to make post-Promise cohorts resemble pre-Promise cohorts on the same set of observables (estimates without reweighting are similar, but slightly smaller). Income groupings pertain to student eligibility for free/reduced price lunch. The mean of the dependent variable for each group is calculated over the eligible population in the pre-Promise period. Sample sizes are: overall, 3851; non-low, 1641; low, 1259; white, 1545; non-white, 1360; male, 1388; female, 1517.

For the groups, Promise effects show a diverse pattern. Across income background, effects for either completion outcome vary only slightly in magnitude, about 3 percentage points.[9] Both types of students have effects that are large in both absolute and proportional terms (6 to 12 percentage points, or 22 to 57 percent).

However, across race and gender, there are larger differences. The Promise has (an imprecisely estimated) null effect on credential completion of white students, but it substantially boosts college completion among students of color, especially in proportional terms (around 50 percent). Due to smaller sample sizes, we cannot rule out the same treatment effect across ethnic groups at conventional levels of statistical significance. However, because the differences are large in magnitude, and because Bartik, Hershbein and Lachowska (2015) do find statistically significant differences across ethnicity for other dimensions of post-secondary success (e.g., credit completion after two years, for which more cohorts of data are available), we regard the estimates that allow for ethnic differences to be preferable to estimates that do not.

In the case of gender, 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 to 19 percentage points (45 to 49 percent).

Comparable group-specific estimates in the literature are rare. The need- or merit-based nature of other scholarships can make it difficult to find diverse income groups using the same

[9] Although point estimates for low-income students are slightly lower than for non-low-income students, the differences are not close to statistical significance ($p = 0.72$), and in proportional terms, effects are much larger for low-income students. Bartik, Hershbein, and Lachowska (2015) also find no statistically significant differences across student income background on other college outcomes (e.g., credits attempted), even using shorter follow-up periods with larger samples.

scholarship, and few other scholarship studies have looked at heterogeneous effects 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.

III. BASELINE METHODOLOGY FOR COMPARING BENEFITS VERSUS COSTS FOR THE KALAMAZOO PROMISE

Our baseline methodology for comparing Promise benefits versus costs is simple. Based on the estimated increase in bachelor's degrees and associate degrees, we compute the resulting increase in expected lifetime earnings, both overall and for different groups. The present value of these lifetime earnings increases is then compared with the costs of the Promise scholarships.

Such a benefit-cost analysis is incomplete. Focusing on earnings understates benefits because it omits education's nonpecuniary returns: improved health, reduced crime, and increased civic participation (Currie and Moretti 2003, Moretti 2004, Oreopoulos and Salvanes 2011). Earnings does 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 both reduce stigma effects of unemployment and reduce leisure time.[10] Individual earnings may understate collective earnings increases if there are spillover benefits of some workers' skills on other workers' productivity, due, for example, to agglomeration economies (Moretti 2003, 2004, 2012). Gross earnings increases also have distributional effects, such as increased tax revenues, and reduced transfers, that should be considered in an ideal analysis.

On the cost side, the total financial costs of a scholarship program should include administrative overhead. Scholarships that increase education may come with opportunity costs due to reduced leisure and reduced earnings while in college. In addition, increased educational

[10] Bartik (2012) reviews the literature on the costs of unemployment.

costs may arise from external subsidy costs to the government if the actual costs of providing additional education exceed tuition. Treating scholarships as a pure cost ignores the benefits of this income transfer for students and their families.

All this being said, a straightforward comparison of earnings benefits with scholarship costs has several virtues: relative ease of estimation; relevance to salient benefits and costs; and comparability with the literature. Earnings benefits and scholarship costs can be measured relatively objectively, whereas estimating other possible benefits and costs (e.g., nonpecuniary benefits/costs of education or work) is more subject to disagreement. In any complete benefit-cost analysis, earnings benefits and scholarship costs would be highly important components. Finally, comparing earnings benefits and scholarships costs is similar to what other researchers have done (e.g., Dynarski 2008 and Scott-Clayton 2009), which allows results to be compared.

Although our baseline methodology compares only earnings benefits and scholarship costs, later we consider the robustness of our findings to additional benefits and costs.[11]

IV. COST ANALYSIS OF PROMISE SCHOLARSHIPS

We calculate average scholarship costs per student, in 2012 dollars, and discount costs to the time of high school graduation, both for the overall sample and for the six groups in our educational attainment analysis.

[11] One factor we do not include in our benefit-cost analysis at present is Promise effects on increasing college quality, as suggested by the work of Andrews, Desjardins, and Ranchhod (2010). We do not include such an analysis in part because we have not yet used our DD analysis to explicitly look at college quality degree attainment effects. Because the Promise increases *enrollment* at Michigan's flagship universities (University of Michigan and Michigan State University) relative to other 4-year colleges, this will probably increase both Promise costs and Promise earnings benefits. Such an additional analysis would require both new estimates of Promise effects on college quality, and linking college quality to earnings benefits.

To match the educational estimates presented earlier, we calculate costs for only the first six years after high school graduation.[12] We use cost data for only the 2006 and 2007 graduating cohorts because full cost data are unavailable for the last cohort in the attainment analysis (2008), but as shown below, there is little change over time in costs per student. Otherwise, we include cost data for every student in the educational attainment analysis sample: 388 students from the class of 2006 and 462 students from the class of 2007.[13]

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]

Most of the analysis uses a real discount rate of 3 percent.[15] However, we also consider the internal rate of return that equates Promise earnings benefits with Promise scholarship costs. We emphasize that our costs per Promise-eligible student include eligible students who never receive Promise funds. This makes our cost estimates comparable with our educational attainment estimates, which include all Promise-eligible students, not just Promise users.

Table 2 shows the present value of Promise scholarship costs per student, both overall and by group. The largest source of variation in Promise costs per student is the highest credential attained. Students who earn bachelor's degrees have higher Promise costs, both

[12] Costs and benefits may change with a longer follow-up, as students can receive Promise aid for up to 10 years.

[13] The Promise is open to graduates of alternative education programs that do not result in a high school diploma, but these students are excluded in both the educational attainment analysis and benefit-cost analysis.

[14] We interpolate the deflator and discount rate log-linearly, assuming Promise paydates of June 15 (Summer), September 15 (Fall), and January 15 (Winter/Spring).

[15] Such a discount rate is often used. Bartik (2011) and Moore et al. (2004) review the discount rate literature.

because they attend college for more years, and because 4-year colleges are more expensive than 2-year colleges. Students earning a bachelor's degree have Promise costs more than three times as large as students earning an associate degree.

Table 2 Costs of the Kalamazoo Promise per Promise-Eligible Student, by Subgroup

Overall Costs ($)		Costs by Highest Credential ($)	
Total	17,620	Bachelor's	33,359
2006 grad class	17,756	Associate	9,634
2007 grad class	17,483	Certificate	5,075
		No credential	7,644

Costs by Groups ($)	
Non-low-income	24,018
Low-income	9,924
White	22,608
Non-white	11,891
Male	16,775
Female	18,419

NOTE: Costs are present discounted values as of high school graduation, using a 3 percent discount 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. All entries, except for the 2006 and 2007 grad class lines, represent averages for the two graduating classes. Demographic characteristics are from KPS data; highest credential (within six years of high school graduation) is from KPS data merged with National Student Clearinghouse data. Low-income students are those who in high school were eligible for a free or reduced-price lunch (family income below 185 percent of poverty). White students exclude Hispanic students.

For more-advantaged groups, Promise costs are higher. Costs per student are over twice as great for non-low-income students ($24,000) as for low-income students ($9,900). 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).

V. CONSTRUCTING EARNINGS PROFILES TO EVALUATE THE PROMISE'S EARNINGS EFFECTS

A. Overall Logic of Earnings Benefits Computations

To estimate the Promise's earnings benefits, we first use cross-sectional microdata (described in the next subsection) to compute, both overall and for our six groups, average earnings by age and by three educational categories: individuals with a high school diploma but no higher degree; those with an associate degree; and those with at least a bachelor's degree. To infer the earnings benefits of obtaining an associate degree due to the Promise, we compute the net present value of having an associate degree relative to a high-school diploma and multiply this value by the estimated effect of the Promise on obtaining an associate degree. To infer the earnings benefits of obtaining a bachelor's degree 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.

These calculations rely on two assumptions. First, that cross-sectional variation in earnings by educational attainment can be interpreted as the causal effects of education on earnings. Second, that the marginal student whose education increases because of the Promise will experience the same earnings increase as is true in the cross-section.[16] For example, perhaps Promise-induced college graduates will be less likely to go on to get graduate degrees than is true in the cross-sectional data.

[16] We also ignore that an increase in the supply of education may reduce its return (i.e., general equilibrium effects). Even if the Promise were much larger, the expected reduction in education returns would be very slight (Acemoglu and Autor 2011).

Much evidence exists on education's causal relationship to earnings (Card 1999 and Zimmerman 2014 offer reviews). Causal estimates are similar to cross-sectional differences. In later robustness checks, we adjust estimates to causal effects based on prior research.

As for whether marginal students induced into obtaining more education by the Promise experience average (cross-sectional) returns, the literature is divided. Structural models suggest that returns for marginal students are likely to be lower than observed in cross-sectional data (Willis and Rosen 1979; Carneiro and Lee 2004; Carneiro, Heckman, and Vytlacil 2011). But studies that incorporate credit constraints and other frictions into college selection suggest that marginal returns may be higher (Card 2001, Brand and Xie 2010). While we believe that the assumption of marginal returns equaling average observed returns is reasonable, we explore sensitivity to different education returns in a later section.

B. Detailed Construction of Earnings Paths for Race, Sex, and the Overall Sample

For the overall sample and the race and gender groups, constructing education-specific career earnings paths is straightforward. As described above, we use cross-sectional variation in earnings by age and education, and then adjust for mortality and secular wage growth.

Our cross-section annual earnings data come from the 2012 American Community Survey (ACS), which provides a large enough sample to estimate average earnings for groups by single year of age. Our annual earnings data reflect only wages and salaries and exclude self-employment income. Observations with imputed earnings are dropped. Observations with zero earnings are included, to reflect both wage and employment rate differences.

Our three education groups are: high school diploma, but no post-secondary degree; associate degree but no higher degree; bachelor's degree or higher. For each education group, we

calculate mean earnings for cells defined by single year of age and demographic group, applying the ACS sample weights. We include ages 25 through 79. Many individuals have not completed schooling before age 25, and our estimated Promise effects are six years after high school graduation, when the modal student would be 24. Therefore, it is difficult to know how to treat earnings at younger ages, and so we exclude them.[17] Earnings after age 79 are assumed negligible.

Next, these cross-sectional earnings profiles are adjusted for secular wage growth. We adopt the assumption of the Social Security Administration (SSA) Board of Trustees of 1.2 percent annual real wage growth over the next 60 years (Table V.B.1. 2015 OASDI Trustees Report). Our adjustments are for a Promise student graduating in 2006, the first Promise class. A typical student in this class would be 25 in 2013. Earnings at age 25, received in 2013, are increased by 1.2 percent from the 2012 cross-section estimate for age 25; earnings at age 26, received in 2014, are increased by 2.414 percent (1.012^2) from the 2012 estimates for age 26; and so on for subsequent ages.

These wage projections could be biased. SSA may overstate future earnings growth. Earnings in 2012 may be depressed by the Great Recession, which will cause earnings benefits to be understated. Younger cohorts may obtain more postgraduate degrees than past cohorts, with the result that cross-sectional data for older ages will understate education differences in earnings for younger cohorts as they age. The number of persons in prison in different groups may change

[17] Our baseline analysis thus does not account for college's opportunity costs in foregone earnings. In the ACS, average annual earnings of 18–24 year olds in Michigan with only a high school diploma and who are not in college are $9,700; earnings for college enrollees are $5,500. However, students who complete their degrees under the Promise regime would likely have been enrolled in college for a considerable time even in the absence of the Promise. In addition, some of the advantages of Promise-induced increased college graduation rates may begin prior to age 25. In a later section, we briefly consider the implications of adding in opportunity costs of education, and find that they do not substantially change the results.

over time, also altering relative earnings.[18] The returns to education may be subject to secular changes, up or down. To address these issues, we perform some sensitivity tests later.

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. We use the relevant life tables for the overall population and the race and gender groups.[19] These Life Tables may also be subject to some bias, as mortality rates will presumably decline over time.

Finally, we apply the same present value discounting as we did with Promise costs.

These calculations suffice for race and gender groups. However, groups defined by family income background require a more complex procedure, described next.

C. Calculating Expected Earnings Benefits for Low-income Students

Calculating future earnings for individuals who grew up eligible for the federal free or reduced-price lunch program is challenging. Cross-sectional data sources, including the ACS, do not measure a person's family income from years ago.

Therefore, we turn to the Panel Study of Income Dynamics (PSID), which has tracked the same individuals and their descendants since 1968. In the PSID, we can identify whether individuals at ages 13 through 17 lived in families whose income fell below or above 185 percent of poverty.[20] As these individuals age, we observe their earnings and education. We calculate

[18] The ACS does include individuals in prison and other group quarters. On the other hand, the ACS does not include individuals living abroad, which may bias estimates.

[19] We use the black life tables for the nonwhite group, as most non-white KPS graduates are black.

[20] A family income of 185 percent of poverty determines eligibility for reduced-price lunch. Our procedure averages family income to poverty ratios from ages 13–17, reducing the influence of transient fluctuations or measurement error (Haider and Solon 2006). Earnings profiles are predicted values from regressions of annual earnings (including zeros) on a quadratic in potential experience and a set of year-of-observation dummies.

average real earnings for each age and education level separately for individuals who grew up in low-income families and those who did not.[21]

The resulting earnings profiles appear in Figures 1a and 1b. Several features are evident. First, as expected, more education is associated with greater earnings, especially with a bachelor's degree. Second, individuals who grew up in low-income families earn substantially less than those who did not. Third, the disadvantage from a low-income background increases with age. Fourth, the relative disadvantage of growing up low-income is larger for individuals with a bachelor's degree.

However, age-earnings profiles may have changed over time from the cohorts in the PSID, making these earnings profiles less representative today. We therefore adjust the PSID profiles to be compatible with the ACS 2012 profiles. We do so by assuming that although absolute earnings may have evolved, the relative earnings of individuals who grew up in low-income families, compared to those who did not, has remained the same. In the PSID, for each specific age and education level, we calculate the ratio of income-background-specific earnings to average earnings (pooled across income backgrounds). We multiply these ratios by the equivalent average overall earnings cell for the respective age and education group in the ACS to yield our calculated earnings stream for individuals from different family-income backgrounds.[22]

[21] We fix educational attainment to the level first reported when someone was age 25 or older.

[22] Because PSID data do not permit us to calculate such ratios beyond age 62, we carry forward the age-62 ratio for later ages. Additionally, because we do not observe separate life tables by family income background, we apply the overall population life table estimates to both income groups when adjusting for mortality. If socioeconomic gaps in life expectancy continue (Bosworth and Burke 2014), we will slightly overstate mortality-adjusted earnings for the low-income group, but Oreopoulos and Salvanes (2011) suggest that greater education increases life expectancy, which will lead us to understate mortality-adjusted earnings differences by education for the low-income group.

Figure 1A Earnings Profiles by Education for Individuals Who Grew Up With Family Incomes *Below* 185% of the Poverty Line

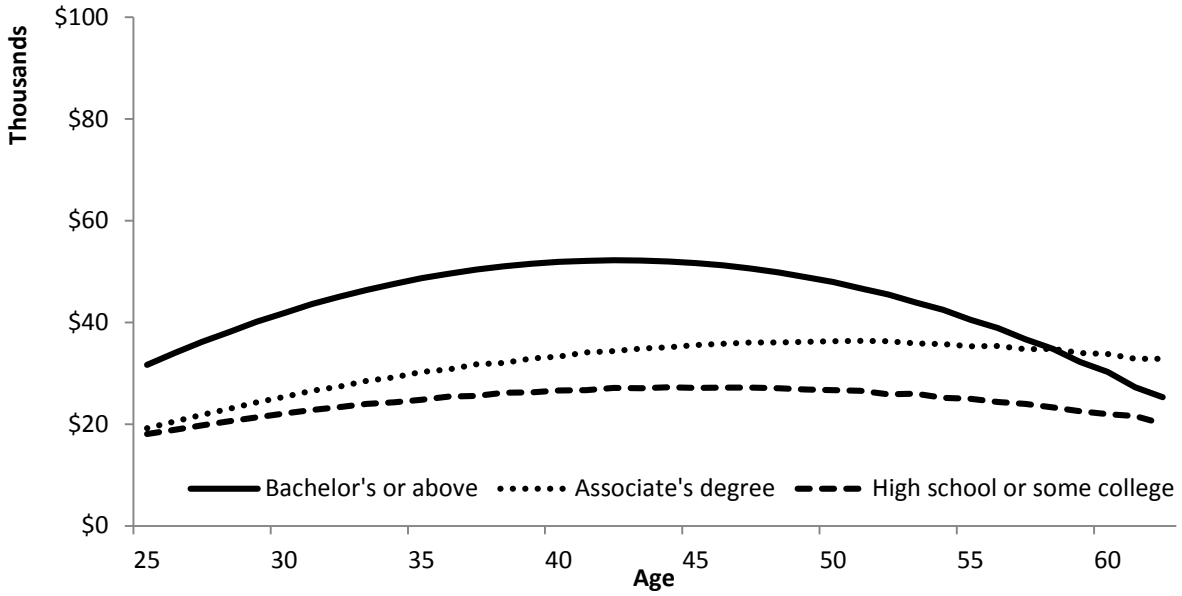
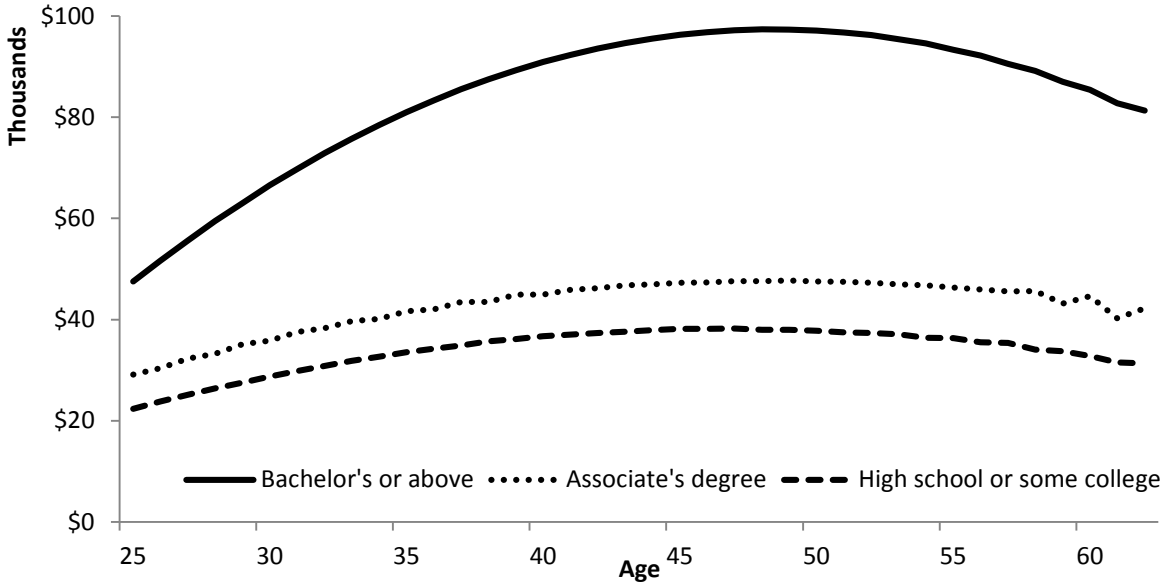


Figure 1B Earnings Profiles by Education for Individuals Who Grew Up With Family Incomes *Above* 185% of the Poverty Line



SOURCE: Authors' calculations from the PSID.
NOTE: Adjusted to year 2014 dollars 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 at age 25; "Associate's degree" includes respondents who had 14 or 15 years of education at age 25; "High school or some college" includes respondents who had 12 or 13 years of education at age 25. Family-income classification is based on average family income when respondent was 13-17. Profiles are fitted values from regressions of annual earnings on a quadratic in potential experience and year-of-observation dummies with the latter netted out.

D. Returns to Education by Group

Before turning to the benefit-cost estimates, we consider what these estimated earnings paths imply for the returns to education by group. Table 3 shows, as one would expect, that there are high earnings returns to increased education, especially for obtaining 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 high for most groups, with one notable exception: individuals from a lower-income background. For these individuals, the observed bachelor's premium is just under 70 percent—half the average.

This surprising new finding, that individuals who come from low-income families have lower returns to education, obviously deserves further investigation (in a separate paper). This disadvantage could be due to unmeasured skills, job networks, college quality, college majors, occupational choice, health, regions, neighborhoods, and other factors. Whatever the causes, these differences in education returns with family background should be reflected in benefit-cost studies of educational investments.

In contrast, different racial groups have similar dollar benefits from educational credentials. Because non-whites have lower earnings, similar dollar benefits translate to higher percentage benefits. Finally, women and men have similar percentage returns to a bachelor's degree, but women have lower dollar returns. For associate degrees, women have somewhat higher percentage returns than men, but similar dollar returns.

Table 3 Projected PDV Earnings and Returns to Education, by Group

	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			*Non-white*		
	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 in 2012 dollars, rounded to nearest hundred, is calculated as of age 18 and based on a 3-percent discount rate. Discounted career earnings cover ages 25 through 79, adjusted for secular earnings increases and mortality as described in the text. “HSG” includes regular high school diplomas and those with some college but no postsecondary degree. “AA” includes associate degree holders with no higher degree. “BA” includes bachelor’s degree holders and holders of higher degrees.

VI. BENEFITS VS. COSTS OF THE KALAMAZOO PROMISE, OVERALL AND BY GROUP

We combine the estimated returns to education by group (section V), and the estimated Promise costs by group (section IV) to calculate benefit-cost ratios and rates of return. We analyze two scenarios. Scenario 1 assigns group-specific Promise effects on educational attainment (Table 1). Scenario 2 restricts each group to have the same Promise effects, based on Table 1's overall estimates. Thus, calculated net benefits in scenario 1 account for heterogeneous effects from the intervention, as well as heterogeneity across groups in costs and earnings effects of education. In scenario 2, differences in net benefits reflect variation only from the latter sources of heterogeneity, which are still quite important.

Which scenario is preferable? As discussed above, for the income groups, the Promise effects are clearly quite similar, so scenario 2 is preferable. For the gender groups, group differences are large and statistically significant, so scenario 1 is preferable. For the ethnic groups, we prefer scenario 1, due to the large substantive differences across ethnic groups and evidence of statistically significant differences for other college outcomes, as previously discussed. However, for all these group analyses, we consider both scenarios for completeness.

Table 4 shows how each scenario affects educational attainment for each group.[23] The preferred Promise effects scenario for each group breakdown is bolded in the table.

Table 5 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," the highest discount rate

[23] In addition to the Table 1 estimates, Table 4 requires information on the share of non-bachelor credentials for each group that are associate degrees. Estimating this outcome directly is infeasible due to small sample sizes. Instead, we assume the *marginal* share of associate degrees equals the observed *average* share for each group among Promise eligibles in the post-Promise period. The benefit results are insensitive to reasonable variations from this assumption, as most Promise returns are due to bachelor's degrees.

Table 4 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 Distribution	**0.571**	**0.031**	**0.398**	**0.849**	**0.047**	**0.104**
S1: Group-specific Promise effects	0.464	0.050	0.487	0.775	0.062	0.163
S2: Homogeneous Promise effects	**0.458**	**0.047**	**0.495**	**0.736**	**0.063**	**0.201**
	Promise effects by race					
	White			Non-white		
	HSG	AA	BA	HSG	AA	BA
Pre-Promise Distribution	**0.567**	**0.035**	**0.398**	**0.825**	**0.071**	**0.104**
S1: Group-specific Promise effects	**0.564**	**0.012**	**0.424**	**0.717**	**0.106**	**0.177**
S2: 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 Distribution	**0.659**	**0.047**	**0.294**	**0.672**	**0.031**	**0.297**
S1: Group-specific Promise effects	**0.668**	**0.041**	**0.291**	**0.510**	**0.060**	**0.430**
S2: 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 education group prior to the Promise (classes of 2003 through 2005), among students who would have been eligible for the Promise had it existed then. S1 considers a post-Promise scenario that assumes differential Promise effects on education across groups (Table 1, panels B through D). S2 instead imposes the same Promise effect on education across all groups (Table 1, panel A).

under which the present value of benefits is equal to or exceeds the present value of costs. Our preferred scenario for each group breakdown is bolded.

For both income groups, under either scenario, the Promise has a high benefit-cost ratio and rate of return. Regardless of family income, students get future earnings benefits that are much higher than scholarship costs. 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. Results are even stronger under our preferred scenario (same Promise effects for both income groups), with net benefits exceeding $20,000 per student for both groups, benefit-cost ratios greater than 3, and real rates of return exceeding 9 percent.

Table 5 Benefit-Cost Analysis of the Promise, by Demographic Groups

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	78,929
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 ($)	**−4,522**	**87,427**	105,913	62,716
Costs ($)	**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: 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 percent 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 effects are the same for both groups. Our preferred scenarios are bolded.

However, the benefit-cost picture is less favorable for the low-income group than implied by the aggregate analysis. This is due to the lower returns to education for students from low-

income families. As a result, the (weighted) average benefits for both groups combined are lower than the aggregate estimates, though still considerable.[24]

For non-whites, 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 three reasons. First, Promise effects on educational attainment for non-whites are large. Second, Promise costs for non-whites are low, because relatively few non-whites obtain a bachelor's degree. Third, education returns for non-whites are high.

For whites, Promise benefits differ greatly across scenarios. In our preferred scenario, when group effects 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 education returns. On the cost side, because a large share of white students earned a bachelor's degree even before the Promise, scholarship costs are high, with many scholarships going to whites who would have completed a bachelor's degree without the Promise. Promise scholarships may benefit these white graduates by reducing debt burdens, but it does not boost their bachelor degree attainment and thereby earnings.[25] 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.[26]

[24] Using weights proportional to the sample counts in Table 1, in our preferred scenario 2, increased earnings average $62,161, average costs are $17,900, average benefit-cost ratio is 3.47, average net-of-costs benefits are $44,261, and the average rate of return is 9.3 percent.

[25] The Promise does, however, increase college quality (as measured by attendance), which may increase earnings (Hoekstra 2009). Ignoring quality effects will thus understate net benefits.

[26] Using weights proportional to the sample counts in Table 2 for whites and non-whites, in our preferred scenario 1 increased earnings average $38,880, average costs are $17,591, average benefit-cost ratio is 2.21, average net-of-costs benefits are $21,289, and the average rate of return is 6.8 percent.

However, if we restrict the two racial groups to have the same Promise effect (Scenario 2), which cannot be rejected at conventional significance levels, then white results are favorable. Under this scenario, the Promise for white students has a benefit-cost ratio exceeding 3. We do not trust this scenario, as it is inconsistent with evidence suggesting different Promise effects by ethnic group. But regardless of what scenario is believed, the Promise's high benefits for non-whites are robust, whereas the benefits for whites are sensitive to specification.

For women, under either scenario, the Promise has large net benefits. Under our preferred Scenario 1, with different education effects across genders, 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. For men, under the same scenario, the Promise has no positive earnings benefits: the program does not improve educational attainment for men, and thus provides no earnings increase, but it has large costs because of the high baseline number of men attending college and getting degrees. High returns for men occur if Promise effects across gender are restricted to be the same (Scenario 2); however, this restriction is clearly rejected by our data.[27]

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

How do our results compare with previous scholarship studies? Scott-Clayton (2009) finds a benefit-cost ratio from the West Virginia merit-based 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

[27] Using weights proportional to the sample counts in Table 2 for men and women, increased earnings average $43,494, average costs are $17,633, average benefit-cost ratio is 2.47, average net-of-costs benefits are $25,861, and the average rate of return is 7.7 percent.

Arkansas and Georgia state merit scholarships. The Kalamazoo Promise's relatively large returns are due to its larger effects on educational attainment.

VII. SENSITIVITY TO ALTERNATIVE CALCULATIONS OF BENEFITS AND COSTS

In this section, we examine how our estimates change by considering additional or alternative costs and benefits and distributional concerns.

A. Additional Costs

Our baseline costs include only scholarships outlays. Here we add two other types of costs: Promise administration and public subsidies of additional public college attendance.

Administration costs for the Kalamazoo Promise are relatively low: just 3.6 percent of annual scholarship costs, according to figures provided by the program director.[28]

The actual costs to provide public higher education exceed tuition and fees, as community colleges and universities receive subsidies for instructional purposes from local taxes (community colleges) and state government (community colleges and universities). We estimate these public subsidies using institution-level expenditure and tuition data from the Integrated Postsecondary Education Data System (IPEDS), produced annually by the National Center for Education Statistics. We first compute school-specific subsidies by taking the ratio of academic expenditures per full-time equivalent student and published tuition and fees of institutions attended by Promise recipients.[29] We then calculate the marginal subsidy costs induced by the Promise by estimating the Promise's effects on cumulative credits attempted six years after high

[28] This cost figure may overstate administrative costs, in that some Promise costs are for efforts to organize community support for KPS students, not for administering the scholarships. On the other hand, the rent for the Promise office is donated, and no figures are available for this in-kind subsidy.

[29] Specifically, academic expenditures include expenditures on instruction, academic support, and student services. Subsidy rates are much higher for community colleges than universities.

school graduation at Promise-eligible community colleges and Promise-eligible universities, converting these marginal credits into an FTE basis (dividing by 24), and multiplying by a weighted average subsidy rate, where the weights are based on the mix of colleges attended by Promise recipients. We note this approach will tend to overestimate external costs for two reasons: first, it excludes the *reduction* in subsidy costs from students switching from non-Promise to Promise colleges; second, true marginal external subsidies from additional students attending college are likely less than average costs due to existing fixed costs.

Table 6 shows how the baseline benefit-cost ratios, in our preferred scenarios, are modified by adding these costs.[30] The benefit-cost ratios only change slightly. Why? First, administrative costs are modest. Second, the public subsidy costs of additional college credits only apply to a portion of Promise recipients, those who increase their college credits.

Table 6 How Benefit-Cost Ratios Change With Additional Costs Added

	Baseline	*Add admin costs*	*Add admin costs + external subsidies*
Overall	4.66	4.50	3.88
Non-low-income	3.53	3.41	2.96
Low income	3.29	3.18	2.77
White	0.75	0.72	0.66
Nonwhite	5.37	5.18	4.37
Male	−0.27	−0.26	−0.23
Female	4.75	4.58	3.98

NOTE: Baseline results for all groups are for preferred scenarios from Table 5.

[30] Although only benefit-cost ratios are shown, to save space, the interested reader can easily compute effects on costs and net benefits, as these changes affect only the cost portion of the benefit-cost ratio.

B. Modifying Benefit Assumptions

This section considers modifications to how recipients' future earnings are projected, in two ways: using causal education estimates rather than cross-section correlations of education with earnings; and modifying the secular growth assumption. In addition, this section adds in possible spillover benefits of education on others' earnings.

Our "causal" estimates of education returns are based on two recent studies that have reasonable identification procedures, and are in accord with the research literature: Zimmerman (2014) for bachelor's degrees and Bahr et al. (2015) for associate degrees. Based on Zimmerman (2014), we scale back our estimated returns to a bachelor's degree by multiplying our differentials for each group by 93 percent. Based on Bahr et al. (2014), we rescale our estimates of the return to an associate degree by multiplying our differentials for men by 90.2 percent, our differentials for women by 129.1 percent, and our differentials for mixed-gender groups by a weighted average. An appendix available on request gives more details on how these specific rescaling percentages were derived. As this above discussion suggests, the available causal estimates do not permit us to have causal adjustments that vary across all six groups in our analysis, as might be ideally desired. Instead, we have to do uniform percentage adjustments, for example for both non-whites and whites, or different income groups. Fortunately, the available evidence suggests that adjusting for causation usually does not significantly alter educational returns.

Our baseline estimates assumed a secular real earnings increase of 1.2 percent annually. As an alternative, this section makes the more-pessimistic assumption of zero real earnings growth. Alternatively, this more pessimistic assumption has the same effect as assuming the real

dollar value of education returns will stay fixed, even if overall earnings increase. It also is equivalent to scaling back education returns to reflect the possibility that marginal returns may be less than average returns.

As mentioned above, increased education may have social and economic spillover benefits that accrue for others, not just degree recipients. For example, work by Moretti (2003, 2004) implies that in a local economy, an increase in college completion that directly raises earnings by 1 percent will raise overall local earnings by close to 2 percent; the spillover effect is of similar magnitude to the direct effect. This spillover effect might arise due to skill complementarity through several mechanisms: teamwork effects within a firm, agglomeration economy benefits from a more productive cluster of firms, or innovation spillovers due to skilled workers contributing ideas that boost productivity. We add in spillover benefits of education, using magnitudes from Moretti (2004).[31]

Table 7 reports how our benefit-cost ratios, for different groups, are modified by these changing benefit assumptions. We begin with the final benefit-cost ratios from Table 6, inclusive of additional costs, and then add cumulatively add in each benefit assumption.

Despite these different benefit assumptions, the results do not change qualitatively. Benefit-cost ratios still significantly exceed 1 for the overall sample, and for both income groups, nonwhites, and females, while falling short of 1 for whites and males. The Promise's educational attainment effects and estimated returns to education are large enough for most groups that

[31] Moretti's (2004) estimates cluster around a 1.2 percent external earnings effect for a 1-percentage-point increase in the share of the workforce with a bachelor's degree. We estimate that the private return to college is such that a one percentage point increase in the share with a bachelor's degree, instead of a high school diploma, increases overall earnings by 1.4 percent (= 1 percentage point times 140.3 percent, from Table 3). Therefore, we scale our earnings estimates for each group and overall by (1.4 + 1.2) / 1.4 = 1.86.

scaling back these returns somewhat does not alter results. Furthermore, while adding in spillovers approximately doubles benefit-cost ratios, spillover benefits are not large enough to overcome the small or negative estimated effects of the Promise on the educational attainment of whites and males.

Table 7 How Benefit-Cost Ratios Change with Modified Benefit Assumptions

	Baseline, with add'l costs	*Causal education returns*	*+ 0 percent real wage growth*	*+ economic spillovers*
Overall	3.88	3.64	2.87	5.32
Non-low-income	2.96	2.77	2.17	4.03
Low income	2.77	2.54	2.07	3.84
White	0.66	0.58	0.45	0.84
Nonwhite	4.37	4.17	3.31	6.15
Male	−0.23	−0.21	−0.17	−0.31
Female	3.98	3.80	3.06	5.68

C. Distribution of Benefits and Costs Between Participants and Non-Participants

So far, we have quantified various benefits and costs, without focusing on who is affected. This section divides up benefits and costs between Promise participants and non-participants. This division helps clarify how the original benefit-cost estimates relate to a more complete benefit-cost analysis.

Participants are simply anyone eligible for the Kalamazoo Promise. Non-participants are all others who might be affected by paying some costs, or receiving some spillover benefits.

We add two key distributional effects. The first effect is cost savings to Promise participants from the tuition scholarships. The second effect is the fiscal benefit to non-participants because Promise earnings increases lead to increased taxes and reduced transfers.[32]

The cost savings to Promise participants consist of two components. The first is the cost savings on the tuition they would have paid without the Promise. The second is half of the tuition cost reductions received on any additional Promise-eligible college credits taken.[33] These two components are calculated from estimates of Promise effects on credits attempted at Promise-eligible two-year and four-year colleges.[34]

The fiscal benefits to non-participants depend on the marginal tax and transfer rate on increased earnings facing Promise participants. Based on Kotlikoff and Rapson (2007), we assume this rate is 36 percent.[35]

Table 8 presents this distributional analysis, showing benefits and costs for participants and non-participants, which together sum to net social benefits. All benefits and costs are stated on a per-participant basis, in present value 2012 dollars. The benefits of Promise scholarships to participants are counted, which reduces net overall costs of the tuition from a social perspective.

[32] One possible distributional effect could arise if Michigan imposes a cap on college enrollments, and Promise-eligible students replace other students at Michigan colleges. However, Michigan does not have such caps. Having said that, there might be general equilibrium effects of the Promise on overall college attendance as well as the returns to college which we ignore in our analysis.

[33] The two components sum to the consumer surplus gain from the Promise-induced decline in tuition price, which is equal to the area to the left of demand curve between the old and new price.

[34] These same regressions are used to calculate external costs of the Promise. The regressions rely on the difference-in-difference methodology used in Bartik, Hershbein, and Lachowska (2015) to estimate college degree effects.

[35] Kotlikoff and Rapson find that after accounting for all federal state and local taxes and transfers, the lifecycle marginal net tax rate for couples whose income is between $20,000 and $150,000 ranges from 32 to 42 percent; for single individuals in this income range, the marginal net tax rate ranges from 32 to 40 percent.

Direct earnings are divided into a portion that provides fiscal benefits to non-participants, and the remainder which boost the income of participants.

Table 8 Distributional Effects of Kalamazoo Promise, Participants vs. Non-Participants

	Overall		
	Participants	*Non-participants*	*Total*
Tuition	$14,217	−$18,252	−$4,034
External costs		−$2,879	−$2,879
Direct earnings	$38,761	$21,803	$60,564
Spillovers		$51,912	$51,912
Net benefits	*$52,978*	*$52,584*	*$105,563*

	Non-low income			**Low-income**		
	Participants	*Non-participants*	*Total*	*Participants*	*Non-participants*	*Total*
Tuition	$19,870	−$24,879	−$5,010	$7,803	−$10,280	−$2,477
External costs		−$3,726	−$3,726		−$1,517	−$1,517
Direct earnings	$39,764	$22,367	$62,132	$15,601	$8,776	$24,376
Spillovers		$53,256	$53,256		$20,894	$20,894
Net benefits	*$59,634*	*$47,018*	*$106,652*	*$23,404*	*$17,873*	*$41,277*

	White			**Nonwhite**		
	Participants	*Non-participants*	*Total*	*Participants*	*Non-participants*	*Total*
Tuition	$19,912	−$23,418	−$3,506	$9,251	−$12,318	−$3,067
External costs		−$2,261	−$2,261		−$2,270	−$2,270
Direct earnings	$7,438	$4,184	$11,622	$30,904	$17,383	$48,287
Spillovers		$9,961	$9,961		$41,389	$41,389
Net benefits	*$27,350*	*−$11,534*	*$15,816*	*$40,155*	*$44,184*	*$84,339*

	Males			**Females**		
	Participants	*Non-participants*	*Total*	*Participants*	*Non-participants*	*Total*
Tuition	$14,337	−$17,377	−$3,039	$14,938	−$19,079	−$4,141
External costs		−$2,125	−$2,125		−$2,901	−$2,901
Direct earnings	−$2,098	−$1,180	−$3,278	$43,017	$24,197	$67,213
Spillovers		−$2,810	−$2,810		$57,611	$57,611
Net benefits	*$12,239*	*−$23,492*	*−$11,253*	*$57,954*	*$59,829*	*$117,783*

NOTE: All figures are present value from the perspective of an 18-year-old participant in the specified group, using a 3 percent real discount rate. All values incorporate broader costs and benefits as reflected in the last column of Table 7.

The Promise has sizable distributional effects. Much of the scholarship costs, previously counted only as a cost, reflect a transfer of wealth from non-participants to participants. A

sizable portion of earnings benefits go to non-participants, through increased taxes and reduced transfers. Non-participants also benefit from spillover benefits of more education, while paying greater college subsidy costs.

Overall net benefits of the Promise, counting everyone equally, becomes much more favorable, compared to the baseline of Table 5. Net social costs are lowered by tuition cost savings for participants, and external benefits of education are added in; these two changes outweigh added costs from scholarship administration and public college subsidies.[36]

The original benefit-cost analysis, however, is a rough indicator of the benefits and costs for non-participants *if* it is the case that the sum of the fiscal benefits and external benefits from increased education is of a similar magnitude to the direct gross earnings benefits, which is the case here. We assume fiscal benefits of 36 percent of direct earnings, and external benefits of 86 percent of direct earnings, so the benefits to non-participants end up being similar to the direct earnings benefits. And because the total costs for non-participants are mostly the costs of providing the scholarships, the original benefit-cost picture is a rough guide to the net benefits from the perspective of non-participants.

The overall qualitative picture from this more complete benefit-cost analysis—albeit one based on more assumptions—is similar to the original simple comparison of earnings effects

[36] These highly favorable net benefits would still persist even if we added in the opportunity costs of education. As mentioned above, average annual earnings for Michigan 18–24-year-olds who are enrolled in college are around $4,200 below earnings of non-enrollees with only a high school diploma. Our DD estimation procedure finds that the Promise increases credits attempted for the overall sample after six years by 14.35 credits (standard error = 5.27), from a baseline rate of 58.81 credits. If a standard academic year is 24 credits, 14.35 additional credits attempted is equivalent to being enrolled for an additional 59.8 percent of a full-time academic year over this six-year period. If an equivalent drop in earnings occurred, this would decrease earnings by $2,510 over those six years (2,510 = 59.8 percent times 4,200). Even without applying discounting, this is less than 3 percent of the net overall benefits of $106,000 estimated in Table 8. Furthermore, this ignores that some of the benefits of increased educational attainment post-college may begin prior to age 25.

with scholarship costs. The Kalamazoo Promise clearly has a large payoff from a variety of perspectives for the overall student body, as well as both income groups, non-whites, and females. Whether the Promise pays off for white students and male students is more questionable, and depends on how one weights benefits for participants versus non-participants.

D. Local Benefits of the Kalamazoo Promise

As discussed above, the Kalamazoo Promise is funded by anonymous donors, in part to promote local economic development. What local development effects are plausible?

One development effect occurs because of how the Promise affects the Kalamazoo area's future workforce, due to effects on Promise students. As college graduates are more likely to participate in national job markets, the proportion of college graduates staying in their metro area of origin is less than for non-college graduates, by about 20 percentage points (Bartik 2009). Therefore, the Kalamazoo Promise's college graduation effects would be expected to reduce the probability that local students remain in the area. However, attending college in one's home state increases the share of college attendees that remain in the state by roughly 10 percentage points (Groen 2004). Because the Kalamazoo Promise successfully encourages students to attend college in Michigan rather than elsewhere, a greater share of Kalamazoo students would be expected to remain in Michigan, and some of these will stay in the Kalamazoo area. Additionally, based on observed migration behavior, the historical proportion of college graduates from the Kalamazoo area who remain there for most of their career is approximately 35–45 percent (Bartik 2009). Thus, it seems likely that the Promise will result in a sizable proportion of graduates staying in the local area.

Consequently, the long-run net effect of the Promise on the local workforce is that more of Kalamazoo's children will leave the area, but a higher proportion of those who stay or return will be college graduates. The Promise's direct effect will be to reduce the local workforce, but improve its quality. This improved quality of the local workforce will increase average local wages, both directly, and through spillover benefits (Moretti 2004). Some of these spillover benefits will occur because a more skilled workforce will attract more and better jobs to the Kalamazoo area, which in turn will have effects in attracting additional population.

A second local development effect of the Kalamazoo Promise arises from the possibility of more immediate migration effects, due to parents being attracted (or induced to stay) by the scholarships. Earlier research has shown that the Kalamazoo Promise increased the school district's enrollment by about 30 percent compared to what it otherwise would be (Bartik, Eberts, and Huang 2010), with about one-quarter of this increased enrollment from outside the state (Hershbein 2013). Although no detectable impact of the Promise on housing prices has been found in Kalamazoo, broader studies of Promise-style programs have found them to increase housing prices by 6–12 percent (LeGower and Walsh 2014) and to reduce out-migration so as to increase a local area's population by about 1.7 percent within a few years (Bartik and Sotherland 2016). Simulations by Hershbein (2013) suggest that the immediate migration effects of the Kalamazoo Promise might be sufficient to raise gross regional product by 0.7 percent.

In sum, the current evidence suggests that a considerable portion—but by no means all—of the Kalamazoo Promise's benefits will be captured in some form by the local area. Future empirical work may allow more exact quantification of these geographic distributional effects.

Because local areas do not capture all benefits and costs, an optimal public finance argument can be made that such scholarship programs might be better run at a state or national level. However, this omits possible advantages from greater local flexibility and creativity, which might be realized as areas compete to provide better scholarship programs. For example, the large benefits of the Kalamazoo Promise might stem in part from its simplicity. Such simplicity might be more likely in scholarships that are locally-run, compared to scholarships designed by a federal bureaucracy.

VIII. CONCLUSION

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

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 based on race or gender, we note that the Promise effects we have estimated are for 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. 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 non-white 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 non-white 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.[37]

Overall, our benefit-cost analysis suggests that the Kalamazoo Promise has high benefits relative to costs. Even with a benefit measure that omits spillover 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. When such additional benefits are included, the Promise has even larger net benefits.

These large net benefits of the Kalamazoo Promise, with some striking differences across ethnic groups and gender groups, deserve further examination. The large net benefits of the Promise stem mostly from its sizable effects on college completion, which are larger than those from other scholarships programs that have been studied. Are the Promise's relatively large

[37] It is for this reason that 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 lifecycle earnings are rarely observable, the importance of heterogeneity has large implications on benefit-cost measurement for social programs.

completion effects and expected net benefits due to its universal and simple nature? Such a hypothesis is plausible, but should be further examined by looking at the effects of other place-based scholarships, ideally of different designs and in different local contexts.

The much stronger results for women than for men, and for nonwhites than for whites, also should be explored. Such exploration could be done in part by analyzing other Promise-style programs to see whether they follow similar patterns. In addition, as additional data are collected on the Kalamazoo Promise, larger sample sizes may permit precise examination of Promise effects for smaller sub-groups, such as nonwhite men, white women, or low-income men, which may help further clarify who does and who does not benefit from the scholarship. Finally, the Kalamazoo Promise program itself has ongoing efforts to increase the success rate of all Promise-eligible students. Qualitative or quantitative evaluation of these efforts to help improve the Promise success rate might indicate what, in addition to financial scholarships, is needed to effectively and efficiently improve American college completion rates.

Appendix

Additional Information on Modifying Cross-Sectional Estimates of the Return to Education to Reflect Causal Effects of Education

In the paper's Table 7, we base our causal estimates of education returns on Zimmerman (2014), for bachelor's degrees, and Bahr et al. (2015), for associate degrees. Both papers 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 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.[38]

[38] Zimmerman 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.)

Bahr et al.'s causal estimates rely on individual fixed effects of students in Michigan. Specifically, they compare quarterly earnings before and after an individual earns an associate degree.[39] 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 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.[40] 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 degree attainment, Bahr et al. 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-degree holders have an earnings premium over high-school-diploma holders of $6,519 for men and $7,416 for women.[41] Therefore, the causal 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

[39] Their fixed effect estimates (Table 4) find causal effects of an associate degree on quarterly earnings (in 2011 dollars) of $2,346 for females and $1,441 for males.

[40] This calculation compares the sum of average earnings for all persons between ages 26 and 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.

[41] Bahr et al. 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 age ranges (ages 19, 23, 36, and 53). We assume most earnings effects will be measured as of 4–8 years after initial enrollment. Therefore, we look at the earnings gains from associate degrees in our cross-section data for ages 23–27, ages 27–31, ages 40–44, and ages 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. study.

estimates to adjust at all ages the differential between associate 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 associates 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 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).

Bartik, Hershbein, and Lachowska (2015) find the largest effects of the Promise on bachelor's degree attainment, not associate 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 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 will be slight, as the causal estimates adjust down the return to a bachelor's degree by only 7 percent. These expectations are confirmed in Table 7, in which causal estimates only slightly reduce the estimated benefit-cost ratios.

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