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Timothy J. Bartik
W.E. Upjohn Institute for Employment Research, bartik@upjohn.org

Brad J. Hershbein
W.E. Upjohn Institute for Employment Research, hershbein@upjohn.org

Marta Lachowska
W.E. Upjohn Institute for Employment Research, marta@upjohn.org

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Authors

Timothy J. Bartik, *W.E. Upjohn Institute for Employment Research*
Brad J. Hershbein, *W.E. Upjohn Institute for Employment Research*
Marta Lachowska, *W.E. Upjohn Institute for Employment Research*

Upjohn Author(s) ORCID Identifier

🆔 https://orcid.org/0000-0002-6238-8181

🆔 https://orcid.org/0000-0002-2534-8164

🆔 https://orcid.org/0000-0001-9158-2592

The Effects of the Kalamazoo Promise Scholarship on College Enrollment, Persistence, and Completion

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Timothy J. Bartik, Brad Hershbein, and Marta Lachowska
W.E. Upjohn Institute for Employment Research
300 S. Westnedge Ave.
Kalamazoo, MI 49007
Phone: (269) 343-5541
Email: bartik@upjohn.org

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ABSTRACT

We estimate the effects on postsecondary education outcomes of the Kalamazoo Promise, a generous, place-based college scholarship. We identify Promise effects using difference-in-differences, comparing eligible to ineligible graduates before and after the Promise's initiation. According to our estimates, the Promise significantly increases college enrollment, college credits attempted, and credential attainment. Stronger effects occur for women.

At a time of high returns to college but rising college costs, the Kalamazoo Promise provides simple and generous college financing. This program, often simply called the Promise, was announced on November 10, 2005, and offers large college tuition subsidies to graduates of Kalamazoo Public Schools (KPS). Funded by anonymous private donors, the Promise pays up to 100 percent of tuition and fees for any public postsecondary institution in Michigan and is "first-dollar," so aid is not reduced by other scholarships. The only conditions to qualify for the Promise are that a student be continuously enrolled in KPS since at least ninth grade, that he or she live in the school district and graduate from KPS, and that he or she gets into any public college in the state.

The Kalamazoo Promise, being the first such "place-based" scholarship, has generated much interest in the media and from other school districts.[1] Since 2005, approximately 100 communities around the country have adopted Promise-style programs, in some cases using public funding or imposing additional requirements for eligibility. While there is a large literature on the effects of financial aid—including state merit-based scholarship programs—on postsecondary education outcomes, little is known about the effectiveness of place-based scholarships, which tend to be far more generous and open to a broader swath of students. With place-based scholarships becoming more common and per-pupil state appropriations to public colleges having declined over the past 15 years (Baum and Ma 2016), understanding the potential for alternative approaches to help finance postsecondary education will only grow in importance.

In this paper, we estimate the effects of the Kalamazoo Promise on postsecondary education outcomes, including attendance, persistence, and degree completion. To identify

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[1] See, for example, Bartik and Lachowska (2014); Burke (2014); Caplan-Bricker (2014); CBS News (2007); *Economist* (2008); Fishman (2012); and NBC News (2013).

Promise effects, we estimate difference-in-differences models that compare outcomes of eligible students to those of ineligible students both before and after the announcement of the Promise, conditional on available student characteristics. As the Promise's tuition subsidy varies with the length of continuous enrollment in the school district, we exploit a large change in generosity in which the scholarship pays at least 65 percent of tuition and fees for students enrolled by the beginning of ninth grade but zero for students enrolled afterward.

The unexpected announcement of the Promise in the fall of 2005 created a situation in which some KPS students found themselves eligible for at least 65 percent of future tuition subsidies, while others discovered they were ineligible for the scholarship. This situation thus resembles a natural experiment in which one group of students is entitled to tuition subsidies and another group is not entitled to anything. Using this natural experiment, we compare changes in post-secondary outcomes before and after the Promise's creation between Promise-"eligible" students (or students before the Promise who would have been eligible based on when they enrolled in KPS) and Promise-ineligible students. We examine both shorter-term outcomes, such as college enrollment, as well as medium- and longer-term outcomes, such as credits attempted and credentials earned. Our identifying assumption is that trends in unobservables did not change between Promise-"eligible" and -ineligible students around the time of the Promise announcement, and we explore the sensitivity of this assumption in several robustness checks.

To corroborate our principal "within-KPS" identification strategy, we complement it with a "between-district" analysis. Specifically, using district-level data from the Michigan Consortium for Educational Research (MCER), we create district by high-school-graduation cohort averages of college-going behavior that allow us to compare KPS to other districts in Michigan before and after the announcement of the Promise. The similarity between the "within-

KPS" and "between-district" results suggests some robustness of our estimated Promise effects to varied methodologies and data.

The natural experiment created by the Kalamazoo Promise is valuable to study for several reasons. First, although one might suppose that any generous college scholarship would increase college attendance and degree completion, past research suggests some uncertainty as to whether tuition subsidies targeting specific colleges have a positive effect on degree completion. Theory suggests that a college subsidy targeting public colleges in Michigan will increase demand for these colleges, resulting in increased enrollment. But will it necessarily improve degree completion rates? Cohodes and Goodman (2014) study the Adams scholarship, an in-kind tuition-waiver program available for attending public colleges in Massachusetts, which increased enrollment at state public colleges but also incentivized students to enroll in worse-quality colleges than they would have otherwise. An unintended consequence of the Adams scholarship, according to Cohodes and Goodman (2014), was a decrease in college graduation rates. Therefore, it is interesting to know how the Kalamazoo Promise, which is also an in-kind subsidy incentivizing students to go to in-state public colleges, affects postsecondary outcomes.

Second, universal eligibility scholarships such as the Promise might have dramatically different effects on college completion from more targeted scholarships based on educational merit or financial need. On the one hand, universal programs are simpler, which might increase participation and effectiveness. On the other hand, universal programs may be less effective in targeting marginal students whose college completion can be most efficiently altered by financial aid. How does the bang for the buck differ between universal scholarships and more targeted scholarships? Given the increased interest among the public and policymakers in more universal

scholarships, providing additional evidence on the relative effectiveness of universal

scholarships is policy relevant.

Third, universal eligibility scholarships by their very design do not target disadvantaged

students. Do more advantaged groups or less advantaged groups end up benefitting more from

such universal programs? Because of the economic and demographic diversity of Kalamazoo

Public Schools, the Kalamazoo Promise can provide evidence on these distributional issues.

KPS's diversity also allows us to examine whether the estimated postsecondary effects of the

scholarship are due to students on the margin of college enrollment (as opposed to inframarginal

students who would have continued on to college, public or private, even without the

scholarship.)

We find substantively large and statistically significant effects of the Promise on many

postsecondary outcomes, including the following three: 1) the probability both of any college

enrollment and of 4-year college enrollment, 2) the cumulative number of credits attempted

within two to four years after high school graduation, and 3) the probability both of obtaining

any postsecondary credential and of obtaining a bachelor's degree.

We estimate that the Promise increased the chance of students enrolling in any college

within six months of high school graduation by 14 percent, and the chance of students enrolling

in a four-year college by 23 percent. We find that the cumulative number of credits attempted

increased by 13 percent as of two years after high school graduation, and these effects persist. At

two years out, the effects imply one additional class attempted; at four years out, they imply an

additional two classes attempted. As of six years after high school graduation, the Promise

increased the percentage of students earning any postsecondary credential by 10 percentage

points, from a pre-Promise baseline of 36 percent to 46 percent; this represents a proportional

increase in credential attainment of more than one-quarter. About three-fourths of this boost in

postsecondary credentials is due to more students earning a bachelor's degree.[2]

We examine the heterogeneity of Promise effects by gender, race, and economic

disadvantage. First, we find that the results are particularly strong for women: female students

are more likely to enroll in college and complete a bachelor's degree within six years of high

school graduation. For men, the enrollment effect is positive but imprecisely estimated, while the

degree attainment effect is both small and statistically not different from zero. Why do women

respond more strongly to the Promise than men? One possible explanation is that family

disadvantage has a more long-lasting harmful effect on boys' educational outcomes than on

girls' (Autor et al. 2015). Another possibility is that the Promise-eligible cohorts studied in this

paper consist of students who found out that they are Promise-eligible relatively late in their

schooling. For these cohorts, this left little time after the announcement of the Promise to adjust

their post-secondary preparation. If boys mature later than girls, then the relatively short time to

respond to the changed incentives might be a possible mechanism for our findings.

Second, we find evidence that the Promise effects are not driven by more-advantaged

students. The college completion results (as well as enrollment and credits-attempted results) are

statistically indistinguishable and quantitatively similar for students regardless of whether they

qualified for a lunch subsidy, although the estimates are sometimes imprecise.

Third, the Promise effects (especially the ones obtained using the within-KPS

comparisons) for white students and for students of color are not significantly different, and, if

anything, are greater proportionally for the latter. For example, for students of color, the

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[2] In a companion paper, we estimate that the credential attainment effects imply a large annual rate of return in increased earnings—over 11 percent—relative to the costs of the Promise's tuition subsidy (Bartik, Hershbein, and Lachowska 2016).

estimated effect of the Promise on attaining a bachelor's degree within six years of high school graduation is 7.4 percentage points, on a base of 16.1 percent, which implies a 46 percent increase. For white students, the estimated increase is 3.0 percentage points, on a base of 39.8 percent, which implies a 7.5 percent increase. This pattern is less pronounced, however, in the between-district analysis, and in some of our other robustness checks, so there is more uncertainty about the relative effects of the Promise on different racial groups.

The results for lower-income students imply that the Promise had a relatively strong impact on traditionally disadvantaged groups, possibly because it induced them to go to a more selective college than otherwise. (See, e.g., Andrews, DesJardins, and Ranchhod 2010 for evidence on the effect of the Promise on college-quality choice; Goodman, Hurwitz, and Smith 2017 for evidence on the effects of college quality on degree completion for marginal students; and Zimmerman 2014 for estimates of returns to college for academically marginal students.)

Notably, our preferred, within-KPS Promise estimates are conservative in two ways. First, by comparing Promise-eligible versus -ineligible students, the estimates omit possible community-wide or school-wide spillover effects of the Promise. The Kalamazoo community and KPS have used the Promise to encourage a college-going culture among parents and students, and have added services to increase the likelihood of college success. Any community or district changes might also affect Promise-ineligible students. In addition, Promise-ineligible students may benefit from positive peer effects from Promise-eligible students.

Second, our estimates are restricted to students who are high school graduates. This restriction occurs because we link KPS individual student records with National Student Clearinghouse data on these same students' postsecondary experiences. We can link such data only for KPS graduates, for whom such records have been requested by KPS. Because we

condition on students graduating from KPS, our estimates omit any Promise effects on graduating from high school. Our estimates of postsecondary success thus incorporate the possibility that the Promise results in more marginal students being included among KPS graduates.[3]

Moreover, as mentioned above, we complement our main analysis with one comparing KPS to other school district before and after the Promise. In general, these results corroborate the within-KPS analysis, although estimates are sometimes less precise.

In the next section, we discuss how the current study fits into the large literature on merit scholarships and the smaller literature on place-based scholarships. In the third section, we describe the institutional details of the Kalamazoo Promise. We follow by outlining our data and methodology and then present our results. We also evaluate the strength of our identification assumptions through several robustness checks and examine heterogeneity of Promise effects across different student groups. We conclude by discussing implications of our results for policy.

THIS STUDY IN CONTEXT

Our analysis is related to the voluminous literature on the effects of college tuition subsidies on postsecondary outcomes. Deming and Dynarski (2010) and Page and Scott-Clayton (2016) provide excellent reviews, and for brevity's sake we only skim the surface here.

Most prior research has focused on merit scholarships, as they are common and thus offer plentiful identifying variation. These sorts of scholarships typically require a minimum high

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[3] As described below, a previous study (Bartik and Lachowska 2013) finds evidence that the Promise improved behavior among all students and improved GPA among African-American students. This suggests that the Promise may increase high school graduation rates. Nonetheless, given that the previous study as well as the current study have K–12 data only from KPS, it is impossible to estimate effects of the Promise upon overall high school graduation rates, as we observe graduation only from KPS, not from other high schools. We note here, however, that to the extent that the Promise induces more marginal students who would be eligible for the scholarship into high school graduation, it biases against our finding effects—particularly longer-term effects—of the program.

school GPA, a minimum college entrance examination score (the SAT or ACT), or both. Georgia HOPE is one of the most-studied merit aid programs. Dynarski (2002, 2004) and Cornwell, Mustard, and Sridhar (2006) have found that Georgia HOPE increased college enrollment and also shifted college choices toward HOPE-eligible Georgia colleges. Researchers have found that other merit aid programs, such as the D.C. Tuition Assistance Grant (Abraham and Clark 2006, Kane 2006) and California's Cal Grant program (Kane 2003), also increased college enrollment, generally in the vicinity of 3 to 5 percentage points per $1,000 of annual aid among affected students.

However, effects on outcomes beyond net enrollment have been mixed. For example, unlike Georgia HOPE, the Tennessee Education Lottery Scholarship did not shift student interest toward Tennessee colleges, but did lead to better performance on the ACT college exam and shifted attendance from two-year to four-year schools (Pallais 2009, Bruce and Carruthers 2014). Scott-Clayton (2011) finds that the West Virginia PROMISE (despite its name, a merit program) increased the probability of completing a bachelor's degree within four years. However, Sjoquist and Winters (2015) find that state-based merit aid programs, as a whole, have generally not been successful in increasing college attainment. Cohodes and Goodman (2014) find that the Adams scholarship, a merit-aid tuition-waiver program for Massachusetts public colleges, increased enrollment at MA public colleges, but at the same time, induced students to forgo higher-quality colleges they would have attended in the absence of the scholarship. Furthermore, unintendedly, the Adams scholarship resulted in a decrease in college graduation rates, a result that we discuss further below.

Less research has investigated broad-based scholarships with minimal academic requirements like the Kalamazoo Promise.[4] The study of place-based scholarships is still in its early stages, although Page and Scott-Clayton (2016) offer a brief review of this literature. Much of the existing research on place-based scholarships has focused on community or school district outcomes, such as K–12 enrollment and housing prices. In large part, this is because these types of scholarships are relatively new and these outcomes can be observed sooner. It also reflects the scholarships taking place on a more local scale. Bartik, Eberts, and Huang (2010) and Hershbein (2013), for example, show that the declining enrollment in KPS abruptly reversed following the introduction of the Kalamazoo Promise, and that enrollment changes were concentrated in grades eligible to receive the scholarship. Although Miller (forthcoming) finds the Promise had little effect on housing prices, the contemporaneous timing of the burst of the housing bubble complicates inference. LeGower and Walsh (2017) study nearly two dozen place-based scholarships (including Kalamazoo's) in a panel setting and find positive effects on school district enrollment and housing prices, with generally larger effects for more-generous and less-restrictive programs.

Among the limited work examining postsecondary outcomes, Bozick, Gonzalez, and Engberg (2015) find that the Pittsburgh Promise, a place-based scholarship with merit and attendance requirements, had little effect on overall college enrollment but did increase the likelihood of attending a four-year school, while Page and Iriti (2016) find similar but more broad-based enrollment increases. Carruthers and Fox (2016) study Knox Achieves, the Knox

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[4] As Dynarski (2004) points out, however, some state merit scholarships have modest requirements, and this makes research on merit scholarships pertinent to this paper. Furthermore, a relevant exception is Dynarski's (2003) study on Social Security student benefits, which existed between 1965 and 1982. She finds that when Social Security ended this benefit, which had been available to most students with a deceased parent, the probability of attending college and educational attainment fell for affected students. While these results suggest scholarships without merit requirements can have important effects on completed education, the population studied faced a rather different landscape of educational options and college costs than is in existence today.

county predecessor to the Tennessee Promise community college scholarship program begun in 2015. Using the staggered roll-out of Knox Achieves, they find that the program resulted in increased college enrollment, more accumulated college credits, and a slight shift from four-year colleges to community colleges. Because the scholarship cost was only about $1,000 per student on average, Carruthers and Fox argue that the financial aid per se was not the most important factor affecting students' educational outcomes. Rather, they write (page 99) "the simple message of 'free community college' … may have fundamentally reshaped the postsecondary educational expectations of participants." Hence, although some might argue that place-based college scholarships are similar to state merit-aid programs without the merit requirements, the results in Carruthers and Fox (2016) offer compelling evidence that suggests the impact of a simple-to-understand message of "free college" might have a different impact on students than "broad" merit-aid programs, such as Georgia HOPE.

Nevertheless, a skeptic, bearing in mind the unintended consequences of the Adams scholarship (Cohodes and Goodman 2014), might argue that in-kind subsidies for in-state colleges will fail to encourage postsecondary success. There are, however, important contextual differences between the setting of our study and that of Cohodes and Goodman. Although both scholarships incentivize enrollment at in-state public colleges, the landscape of educational options in Michigan is different from the one in Massachusetts. First, consider that fewer than one-half of recent Massachusetts high-school graduates who enroll in college do so at an in-state public institution, as opposed to over three-quarters in Michigan (and about 66 percent nationwide).[5] Hence, there is much less scope for a private-to-public change in Michigan than in Massachusetts. Second, the reduction in college quality from switching from private to public is

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[5] These numbers are available from the authors and are based on calculations from the Integrated Postsecondary Education Data System (IPEDS) data for 2002 and 2004, prior to the Adams Scholarship and Kalamazoo Promise.

much more salient in Massachusetts than in Michigan. Specifically, while one-third of freshmen from Massachusetts attending a private (or out-of-state public) do so at an institution belonging to one of the top two Barron's selectivity categories, *no* freshmen from Massachusetts attending in-state public colleges do so at institutions that fall into these categories. In contrast, in Michigan, the corresponding numbers are 12 percent and 6 percent, and for the United States as a whole they are 21 percent and 9 percent.[6]

The differences between the two states in the landscape of educational options matter for the incentives generated by the two scholarships.[7] Whereas the Promise relaxes the costs of going to highly selective Michigan publics, the Adams scholarship might diminish the perceived value of attending a higher-quality private college. In other words, the Promise is more likely to push students up the college-quality ladder (a result we document later in the paper), while the Adams scholarship may push them down that ladder. In the results section, we present the estimated effects of the Promise on college choice and refer to estimated effects on college quality. As available evidence (Bowen, Chingos, and McPherson 2011; Zimmerman 2014; Goodman, Hurwitz, and Smith 2017) suggests that enrolling in a higher-quality college increases the probability of degree completion—especially for low-income students—we would expect the Promise to have a positive impact on this college attainment.

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[6] Similar patterns emerge using other measures of selectivity. Notably, the two highest ranked public colleges in Michigan according to U.S. News rankings, the University of Michigan-Ann Arbor and Michigan State University, are described as "most selective" and "more selective." In contrast, the flagship public college in Massachusetts—University of Massachusetts, Amherst—ranks as "selective." In this regard, the top-ranking Adams-eligible university is closer to the local Kalamazoo public college, Western Michigan University, than to the top-ranking Promise-eligible universities. This pattern would likely hold in most areas of the country other than the Northeast, which has a large concentration of selective privates and relatively weak publics. The recently enacted Excelsior Scholarship in New York (https://www.ny.gov/programs/tuition-free-degree-program-excelsior-scholarship), on the other hand, may resemble the Massachusetts context.

[7] Note that, unlike the Promise, the Adams Scholarship does have a merit component, requiring performance in the top quarter (within school districts) on the Massachusetts Comprehensive Assessment System (MCAS) test.

The emerging evidence also suggests that the Kalamazoo Promise has improved high school outcomes and shaped postsecondary school choices. Andrews, DesJardins, and Ranchhod (2010), for instance, study the initial effects of the Kalamazoo Promise on college choice. The authors examine where ACT scores are sent by KPS students before and after the Promise, compared with trends for other Michigan public school students. In accord with the argument above, they find that the Promise increased score-sending to four-year colleges in Michigan, especially the University of Michigan and Michigan State University, and that these effects were concentrated among test-takers from lower-income families. Finally, Bartik and Lachowska (2013) find that the Promise reduced disciplinary incidents, and, for African American students, increased GPAs. If the Promise alters students' high school success and motivation, this effect might carry over to college outcomes.

BACKGROUND ON KPS AND THE PROMISE

Kalamazoo Public Schools

Kalamazoo Public Schools (KPS) is a midsized, mostly urban school district in southwest Michigan, with on the eve of the Promise, just over 10,000 students. Like many urban school districts, KPS is poorer and more ethnically diverse than nearby areas. As of the year before the Promise's announcement, the school-aged population within the district's boundaries had a poverty rate of 28 percent, and African Americans and Hispanics made up 47 percent and 8

percent of district enrollment. For other Kalamazoo area districts, the poverty rate was only 8 percent, and African Americans and Hispanics were just 5 percent and 2 percent of enrollment.[8]

Table 1 High School Graduates in KPS and Promise (Pseudo-) Eligibility

Year	Graduates	Eligible	Ineligible
2003	525	0 (442)	525 (83)
2004	551	0 (448)	551 (103)
2005	392	0 (345)	392 (47)
2006 (1ˢᵗ Promise cohort)	449	388	61
2007	504	462	42
2008	484	430	54
2009	466	420	46
2010	498	452	46
2011	507	459	48
2012	526	477	49
2013	513	483	30
Total	5,415	4,806	609

NOTE: Numbers represent authors' calculations of the number of graduates receiving high school diplomas (excluding alternative education programs) from Kalamazoo Public Schools and eligibility for the Promise. From 2006 onward, eligibility is taken from administrative records from the Kalamazoo Promise. Before 2006, no cohorts were eligible; the numbers in parentheses represent the number of graduates who would have been eligible had the Promise been in effect for those cohorts. See text for eligibility assignment rules. The lower graduate count in 2005 is in large part due to the alternative high school being closed that year.
SOURCE: Authors' calculations from KPS and Kalamazoo Promise administrative data.

Table 1 shows the number of KPS graduates from the district's two mainline and one alternative high school between 2003 and 2013. Graduates are divided into two groups: 1) those who are Promise-eligible (or would have been eligible if the Promise had existed in the past) and 2) those who are ineligible for the Promise because they entered the district too late to be Promise-eligible.

The Kalamazoo Promise

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[8] Poverty rates are from the U.S. Census Bureau's Small Area Income and Poverty Estimates, and enrollment by ethnicity is from Michigan's Center for Educational Performance and Information.

Announced in November 2005 and taking effect for the high school class of 2006, the Kalamazoo Promise is a scholarship available to all students who graduate from KPS, reside in the district, and have been continuously enrolled since the beginning of high school.[9] Unlike most student aid, the Promise has neither merit requirements (high school GPA or test scores) nor financial need requirements. According to the donors who anonymously fund the scholarship, the Promise's purpose is to both promote a college-going culture in KPS and increase the local supply and retention of college graduates, and in so doing enhance Kalamazoo's economic development (Miller-Adams 2009). Applying for the Promise is quick and simple compared to most other forms of student financial aid, especially the Free Application for Federal Student Aid (the FAFSA). In their senior year of high school, students fill out a one-page form asking basic contact information and only a half-dozen substantive—but straightforward—questions (see Figure 1).

Bettinger et al. (2012) find that providing assistance in filling out complicated financial aid forms or simplifying the process can increase aid receipt and improve college outcomes. The simplicity of the Promise application has contributed to its high use rate of more than 85 percent.[10]

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[9] More precisely, the requirement is being enrolled as of the fall count day in ninth grade.

[10] This use rate is the share of eligible students who successfully submit forms, enroll at a Promise-eligible institution, and receive a Promise scholarship for at least one credit hour. Nearly all students—eligible or not— submit applications. For comparison, in 2012, the estimated Kalamazoo County completion rate for the FAFSA was only 63 percent. http://www.thelearningnetwork.org/Scorecard/CollegeCareerReadiness/tabid/370/Default.aspx (accessed June 22, 2015).

Figure 1 Application Form for the Kalamazoo Promise

The Kalamazoo Promise

125 West Exchange Place • Kalamazoo, MI 49007 • (269) 337-0037 • www.kalamazoopromise.com

Application for The Kalamazoo Promise Scholarship

I am requesting funding from The Kalamazoo Promise scholarship plan.

Student: _____   Date: _____

Address: _____

City / State / Zip: _____

Phone Number: _____   Date of Birth: _____

E-mail: _____

The following is my qualification information:

Grade at which **continuous** KPS enrollment AND **continuous** KPS district residency started: _____

This is the date which I expect to graduate from Kalamazoo Public Schools: _____

This is the high school which I expect to graduate from: _____

The schools listed below are the colleges/universities that I am considering for enrollment and use of The Kalamazoo Promise Scholarship. List your choices in order of preference, you are only required to list one. *(See reverse side for list of qualified schools)*

School 1: _____

School 2: _____

School 3: _____

SOURCE: The Kalamazoo Promise.

The Promise pays up to 100 percent of tuition and fees at any public community college or university in Michigan.[11] The award is treated as first-dollar, meaning that it is applied *before* grant money from other sources.[12] The Promise benefit is graduated based on the length of continuous enrollment in the district's schools: students who have been in KPS since

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[11] Beginning with the high school class of 2015, KPS graduates can also use the Promise at 15 Michigan private colleges. For these colleges, the Promise will pay up to the tuition and fees of the University of Michigan, the most expensive public college; the private colleges themselves will pay the remaining tuition costs (Mack 2014). Our analysis period precedes this change.

[12] Although students do not need to apply for other scholarships to receive the Promise, Promise-eligible students are encouraged to fill out the FAFSA, as federal aid (e.g., Pell grants) can be used for college expenses such as room and board, books, and supplies that the Promise does not cover. In fact, FAFSA completion rates are *higher* in KPS than in other school districts in the county, despite the socioeconomic differences mentioned above, suggesting that the Promise does not crowd out federal aid.

kindergarten receive the 100-percent subsidy, students enrolled since first through third grade receive 95 percent; and students first enrolled afterward have subsidy rates decreased by five percentage points for each subsequent grade through ninth (at a 65 percent scholarship). No scholarship is available for students whose last continuous spell in KPS begins after the start of ninth grade.

Figure 2 shows the relationship between the grade first (and continuously) enrolled in KPS and the Promise's generosity. The figure shows the large drop in expected generosity between enrolling before and after ninth grade. As discussed in our data and methods section, our identification strategy exploits this sizable change in generosity.

Eligible students have 10 years from high school graduation to use the scholarship. The Promise pays for up to 130 credits, just above the number typically needed for a bachelor's degree. Students must be enrolled full-time (12 or more credit hours per semester), with the exception of Kalamazoo Valley Community College (KVCC), the local two-year college, where the required enrollment intensity is only half-time.[13] To maintain eligibility, enrolled students must keep a college GPA of at least 2.0 per enrollment period. (As mentioned above, the Promise has no high school GPA requirements for initial eligibility.) Students falling below this college GPA threshold can regain eligibility by attending college for a semester without Promise support and raising their GPA above the cutoff.

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[13] This exception was instituted for enrollment periods beginning in the fall of 2007.

Figure 2 Generosity of the Kalamazoo Promise, by Grade of Initial Enrollment
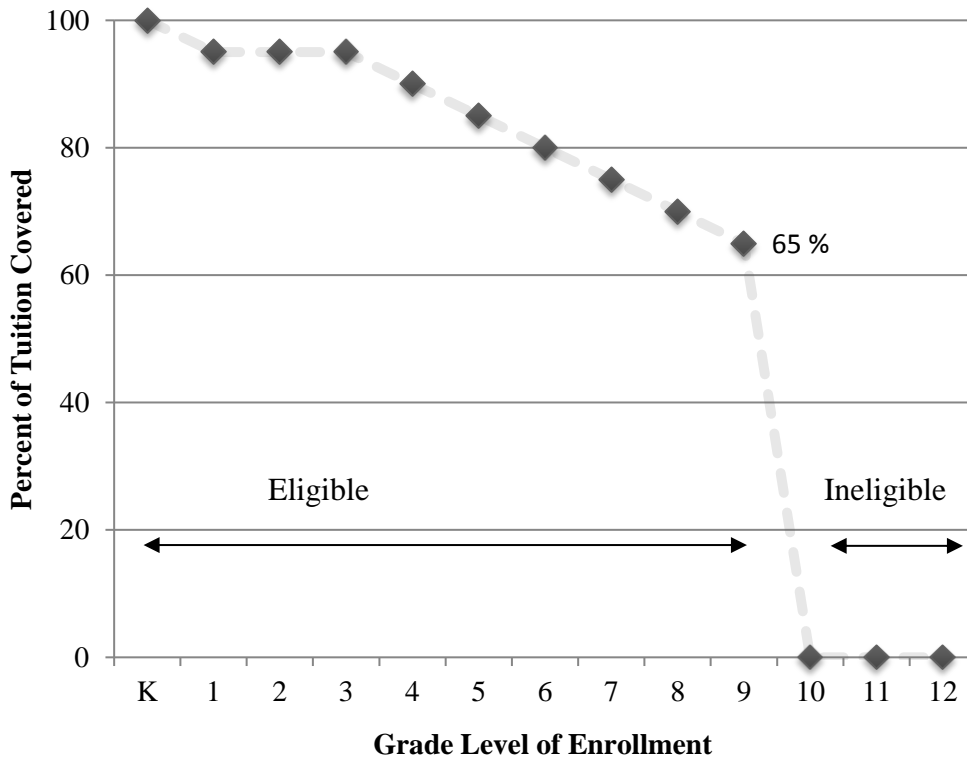
SOURCE: Eligibility rules from the Kalamazoo Promise.

Through the end of 2014, total scholarships paid by the Kalamazoo Promise reached $61 million, with an approximately steady spending level of $10–11 million per year being reached by 2011. As of the fall of 2014, approximately 1,400 KPS graduates were using the Promise, which amounts to average spending per recipient of about $4,000 per semester. The value of the scholarship clearly varies with the specific institution a student attends, however, with per-student spending averaging roughly $1,000 per semester at the local community college (where Promise recipients may enroll less than full time), to nearly $5,000 per semester at a university. During the six years after high school graduation, the average present value of Promise scholarship spending per Promise-eligible graduate is $17,620. For eligible students who end up

getting a bachelor's degree, the average present value of Promise scholarship spending is $33,359.[14]

DATA AND METHODOLOGY

Data

Our primary data come from KPS and Promise administrative records merged with National Student Clearinghouse (NSC) data on college attendance. Our data span high school graduates from the classes of 2003 through 2013. From KPS, we obtain information on student characteristics: sex, race/ethnicity, participation in the federal assisted lunch program at any point during high school, and high school of graduation.

Most important for our identification strategy, the KPS records provide a history of student enrollment and residency in the district, which allows us to construct a Promise eligibility indicator (see Appendix A). Our sample includes three pre-Promise cohorts (the graduating classes of 2003 through 2005) and up to eight post-Promise cohorts (the classes of 2006 through 2013). The enrollment data go back to 1997, which allows us to track enrollment histories for all our cohorts back to sixth grade. Hence, the data allow us to distinguish KPS graduates eligible for *any* tuition subsidies—that is, a subsidy of at least 65 percent—from KPS graduates who are ineligible for any subsidies. However, for the earlier cohorts, we cannot identify the exact fractional scholarship (above 65 percent) for which earlier cohorts would have been eligible had the Promise existed.[15] We are skeptical, however, that many students and their families would be overly sensitive to marginal changes in the percentage of tuition subsidized

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[14] These total cost figures are in 2012 dollars, and use a 3 percent discount rate to calculate present values as of high school graduation. The cost calculations use data from the KPS graduating classes of 2006 and 2007.
[15] Specifically, students with 80 percent or greater scholarships must be grouped together, as data do not go back far enough to precisely assign a scholarship percentage for the 2003 graduates.

relative to the very large change from 0 to 65 percent. Thus, we discretize the Promise eligibility

variable to be binary: any Promise eligibility versus no Promise eligibility.

In the pre-Promise period, we define a dummy that equals one if the student would have

been eligible for any tuition subsidy had the Promise been in effect, and zero if the student would

have been ineligible. In the post-Promise period, we use administrative data on eligibility from

the Promise records.[16] Because these dummies indicate Promise eligibility rather than receipt,

our estimates show intent-to-treat effects of the Promise, not treatment effects of actually

receiving the scholarship.

The KPS high-school-level data are joined to NSC data using a student-level identifier.[17]

The NSC provides for each KPS graduate the specific colleges attended, the dates and intensity

of attendance, and degrees or credentials earned. These data allow us to investigate the patterns

of initial attendance, persistence, and completions.[18]

Table 2 presents means of student characteristics by eligibility, before and after the

Promise. The district's graduates became more ethnically diverse over the sample period, and the

share of students eligible for the federal lunch program increased substantially (largely due to the

Great Recession).

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[16] For the post-Promise period, our calculations of Promise eligibility closely match Promise records, and we use the latter to minimize misclassification error. We thoroughly address the sensitivity of our results to this choice later in the paper and in Appendices D and E.

[17] Details of this matching procedure are found in Appendix A.

[18] As documented by Dynarski, Hemelt, and Hyman (2015), NSC coverage is high but not exhaustive. For this study, the main issue is that the local two-year college, Kalamazoo Valley Community College (KVCC), has NSC records only since the fall of 2005. To avoid excluding earlier KVCC students, we obtained from KVCC enrollment data from the summer of 2003 forward to the summer of 2005 for KPS students who graduated between 2003 and 2005. Our request for these enrollment data were for them to be assembled via the same process used for NSC submissions. Although one might be concerned that the use of different data sources could bias our estimates, this concern is alleviated because our results show most of the Promise effects are on 4-year enrollment and bachelor's attainment, which are unaffected by the inclusion of the KVCC data.

Table 2 Descriptive Statistics of Sample

Variable	All	Before Eligibles	Before Ineligibles	After Eligibles	After Ineligibles	DD [standard error]
Demographics						
Male	0.471	0.470	0.442	0.477	0.439	0.010 [0.045]
Black	0.411	0.346	0.481	0.416	0.532	0.019 [0.045]
Asian	0.026	0.017	0.056	0.026	0.037	0.028 [0.019]
Hispanic	0.070	0.049	0.086	0.075	0.077	0.035 [0.024]
White	0.485	0.584	0.369	0.473	0.340	−0.082 [0.043]*
Subsidized lunch	0.507	0.340	0.541	0.547	0.662	0.086 [0.044]*
High school 1	0.513	0.491	0.399	0.525	0.545	−0.111 [0.044]**
High school 2	0.403	0.446	0.373	0.396	0.338	−0.014 [0.043]
N	5,415	1,235	233	3,571	376	

NOTE: Numbers represent authors' calculations of demographic characteristics of KPS graduates for the classes of 2003 through 2013 (excluding alternative education programs). From 2006 onward, eligibility is taken from administrative records from the Kalamazoo Promise. Before 2006, eligibility is assigned based on Promise rules had the Promise been in effect for those cohorts. "Before" represents the cohorts 2003 through 2005; "After" represents cohorts 2006 through 2013. "DD" represents the difference between eligibles after and before the Promise and ineligibles after and before the Promise. This difference is rounded off. Standard errors are robust to heteroskedasticity.

SOURCE: Authors' calculations from KPS and Kalamazoo Promise administrative data.

A key issue for our quasi-experimental analysis is the potential for bias from a change in the relative composition of the eligible and ineligible groups. Although we later address this concern in detail, including the possible role of selective migration, we briefly examine in Table 2 compositional changes in observables by comparing demographic changes between eligibles and ineligibles, before and after the Promise. We find significant differential changes between the eligible and ineligible groups in three variables: the fractions of white students, students eligible for subsidized lunch, and students enrolled in high school 1.

The change in the fraction of graduates at high school 1 is a consequence of a 2008 redistricting within KPS, which affected newly enrolled students, i.e., ineligible students, disproportionately more than students who had been enrolled previously. Empirically, we control

for this high school differential by including high school-by-year fixed effects, as described below.

Ineligible students are more likely to be students of color and participate in the assisted lunch program relative to Promise-eligible students, both before and after the Promise announcement. However, these gaps significantly lessen after the Promise, as the eligibles become more ethnically diverse and poorer. While we control for these characteristics in our estimation, we note that their direction suggests a negative composition bias on observables, since students of color and poorer students have weaker college outcomes on average. To the extent that selection on unobservables is positively correlated with selection on observables, a common and empirically grounded assumption in the education context (Altonji, Elder, and Taber 2005), it appears unlikely that selection on unobservables will lead us to overstate true Promise effects.[19] Nonetheless, we describe below additional approaches in addressing compositional issues.

Methodology

Our methodological approach is a difference-in-differences analysis, comparing the outcomes of Promise-eligible students to ineligible students, before and after the Promise. This is summarized by the following regression model:

$$(1) \quad y_{ist} = \alpha + \delta_1 Elig_{ist} + \delta_2 After_{ist} + \delta_3 (After \times Elig)_{ist} + \gamma_{st} + x_{ist}\beta + u_{ist},$$

where i denotes the individual student, s denotes the high school, and t denotes the academic year in which we observe the student. The outcome variable of interest is y; "Elig" equals one if,

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[19] The changes in relative composition among eligible and ineligibles shown in Table 2 are consistent with earlier analyses. For example, Hershbein (2013) studies changes in KPS enrollment for grades K–11 after the Promise. He finds that, despite a temporary increase in students entering the district and an enduring decrease in students leaving the district, there were only slight changes in the academic and socioeconomic composition of entering and exiting students, and essentially no change in the stock of students as a whole, compared to the pre-Promise baseline. Similar findings are in Bartik, Eberts, and Huang (2010).

based on length of enrollment in KPS, the student is (or would have been) eligible for a Promise tuition subsidy and zero otherwise; "After" equals one if the student graduated after the Promise was in effect—the class of 2006 and later—and zero if before; and "After × Elig" is an interaction between the two dummies. We also include a set of graduation-year-by-high-school dummies, γ_{st}. The vector x contains student-level observables (listed in Table 2), and u denotes student i's unobservable traits, which we allow to be heteroskedastic.[20]

The coefficient of greatest interest is δ_3: the regression-adjusted difference in average outcomes for Promise-eligible students, compared both to ineligible students' outcomes and to pre-Promise outcomes. This estimator represents an intent-to-treat effect. While it might be desirable to estimate effects for treatment on the treated as well as intent-to-treat effects, the framework of the program does not make this feasible. Because of the difference-in-differences estimation, we cannot, for example, observe what Promise use would have been among the "eligibles" in the pre-Promise period. However, since take-up of the Promise is very high (over 85 percent), we expect the estimates of intent-to-treat effects and treatment-on-the-treated effects to be quite close.

A primary advantage of the within-KPS differences-in-differences strategy is that it implicitly controls for any unobserved effects that vary over time and that affect all KPS students, as long as students are affected equally. Specifically, the validity of our difference-in-differences strategy rests on two assumptions. The first assumption is that outcomes are trending similarly for eligible and ineligible students before the Promise. In a hypothetical world without

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[20] In Appendix Table 3, we report standard errors where we have grouped the students into high school-by-cohort clusters. By doing so, we allow the errors to be correlated between students from the same school-cohort, but not between students from different school-cohorts. Because clustering on high school-by-cohort produces slightly smaller standard errors than the standard Huber-White standard errors (potentially due to the relatively small number of clusters), we adopt the more conservative inference from the latter approach.

the Promise, outcomes of eligible and ineligible students would have followed a common, parallel trend, conditional on observables. The second assumption is that no other change in KPS besides the Promise affected eligible and ineligible students' outcomes in a differential way.

If these two assumptions hold, then in an imaginary auxiliary regression of the disturbance term u on the right-hand-side variables, the coefficient on the "After $\times$ Elig" interaction term would be zero. Unobservables would be uncorrelated with the interaction term after controlling for the x and γ variables and the separate "Elig" and "After" dummies. In this case, the coefficient δ_3 will consistently estimate the effect of Promise eligibility on outcome y.

These identification assumptions are threatened if there is differential trending in student outcomes due to compositional changes over time in the relatively small subset of graduates who entered KPS after ninth grade and are thus ineligible for the Promise. If the ineligibles are systematically different before and after the Promise, relative to the eligibles, estimated effects of the Promise would be confounded with effects due to changing group composition. We deal with changing group composition in part by directly including controls for observables, such as sex, race and ethnicity, participation in the federal subsidized lunch program, and high school of graduation by cohort.

However, it is still possible that selection on unobservables remains, particularly later in the post-treatment period, and that despite the patterns shown in Table 2, this selection could confound our estimates. We address this possibility in several ways.

First, after presenting the results, we examine year-by-year trends in the effects of Promise eligibility. In addition to shedding light on the possibility of differential pretrends across groups, this analysis indicates how quickly the Promise effects appear. Presumably, if selection on unobservables is a problem, it would be expected to worsen over time, as students (and their

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families) have more time to change their behavior (e.g., cumulative selective in-migration and out-migration could have affected a larger share of the student body). If Promise impacts show up immediately, differential selection is likely less salient.

Second, we later restrict the sample to exclude all students who entered KPS after the Promise announcement, which eliminates bias due to selective in-migration.[21] However, this restriction comes at the expense of statistical power.

Third, to the extent that adjusting for the *joint* distribution of observables helps reduce selection on unobservables, we find (Appendix B) that if we reweight post-Promise graduates to resemble pre-Promise graduates, separately by eligibility, (DiNardo, Fortin, and Lemieux 1996), the estimated effects of the Promise are similar but somewhat larger in magnitude than the baseline OLS estimates. This suggests that differential changes in student composition between the eligible and ineligible groups may *downwardly* bias the OLS estimates.

Fourth, we experiment with restricting the eligible sample to students moving into KPS between 7th grade and 9th grade, rather than all grades before high school. This restriction obviously restricts sample size greatly. But it helps address concerns that ineligibles, who by definition are "movers" who entered KPS after 9th grade, may somehow be different in unobservables from "stayers" who have been in KPS since kindergarten or some other early grade.

Finally, we supplement the primary within-KPS analysis with cross-district comparisons of KPS and other Michigan school districts. While such between-district comparisons may also be subject to selection bias on unobservables, they would not be subject to possible biases due to

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[21] We cannot deal similarly with possibly selective out-migration, as we do not have college enrollment and graduation data on students who left KPS before graduation.

other changes in the relatively small group of ineligible KPS students. Therefore, our main

within-KPS analysis is supported if the between-district analysis yields similar results.

RESULTS FOR POSTSECONDARY OUTCOMES

We consider Promise effects on several postsecondary outcomes: enrollment, credits

attempted, and credential completion.

Enrollment

Table 3 presents results for enrollment outcomes. The four panels examine enrollment at

either any postsecondary institution or specifically at a four-year school, within 6 months or 12

months of high school graduation. The table reports estimated coefficients on the interaction

between Promise eligibility and graduation after the Promise, δ_3, which we interpret as the effects

of the Promise scholarship.

The advantage of looking at short-term outcomes is that they can be measured for more

cohorts. Because our postsecondary data run through the 2013–2014 school year, we observe

seven post-Promise graduating classes, 2006 through 2013, as well as three pre-Promise

graduating classes, 2003 through 2005.

For enrollment at any postsecondary institution within six months of high school

graduation, the estimated Promise effect is a net enrollment increase of 8.3 percentage points.

The percentage-point increase is large relative to the mean enrollment probability of 61.2 percent

among eligibles in the pre-Promise period, representing an increase of 14 percent (0.083/0.612 =

0.138).[22] When the enrollment horizon is extended to 12 months, the enrollment effects are

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[22] If we translate this effect into a percentage (or percentage point) increase in enrollment per thousand dollars of
scholarship, as has been done in previous research on the effects of merit scholarships, we find that enrollment

slightly smaller and not statistically different from zero (or from the six-month estimate). This

finding suggests that the Promise may operate in part by accelerating the time to first enrollment,

but the data are not precise on this point, and we do not emphasize it.

Table 3 Promise Effects on Enrollment

	(1)
Panel A: Enrollment within 6 months	
(Mean of DV | after=0, elig.=1) = 0.612	
After × Eligible	0.083**
	[0.042]
R^2	0.150
Panel B: Enrollment within 12 months	
(Mean of DV | after=0, elig.=1) = 0.673	
After × Eligible	0.059
	[0.041]
R^2	0.164
Panel C: Enrollment at 4-yr. within 6 months	
(Mean of DV | after=0, elig.=1) = 0.402	
After × Eligible	0.094**
	[0.038]
R^2	0.184
Panel D: Enrollment at 4-yr. within 12 months	
(Mean of DV | after=0, elig.=1) = 0.411	
After × Eligible	0.089**
	[0.039]
R^2	0.187

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for eligible population in the pre-Promise period. Sample size is 5,415.

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increases by about 4.1 percent (2.5 percentage points) per thousand dollars per semester. These figures come from dividing the estimates by 3.3 ($3300), the average Promise spending (in thousands of 2012 dollars) for the cohorts in the analysis in the fall after high school graduation, aligning with the outcome of enrollment within six months. Note that this spending number is slightly lower than the $4,000 per semester as of 2014 mentioned earlier, mostly due to lower tuition levels in the earlier years.

At either a six-month or 12-month horizon, our estimates suggest that the Promise increases four-year college enrollment by about 9 percentage points. That the four-year enrollment effects are on par with the overall enrollment effects suggests two conclusions: that Promise-induced enrollment is driven by the four-year sector on net, and that the Promise may induce substitution from the two-year to the four-year sector.[23] Because the base enrollment at four-year colleges is lower, the implied percentage effect is larger, at 23 percent $(0.094/0.402 = 0.234)$. That these results differ little across horizon length suggests that the effect of the Promise on overall enrollment timing is driven by the two-year sector, which is plausible.[24]

Previous research shows that aid can affect not just whether a student attends college, but which college she attends. We explore this margin of response in Table 4A, which shows estimates of college attendance at Promise-eligible and Promise-ineligible schools. The first panel shows that attendance at a Promise-eligible institution—public two-year and four-year colleges in Michigan—increases by a large amount: 20 percentage points. This represents an increase over the pre-Promise base of 37 percent $(0.178/0.480 = 0.371)$, which echoes Andrews, DesJardins, and Ranchhod's (2010) findings on ACT score-sending. We obtain similar point estimates when looking at Michigan four-year publics (Panel B), but because the pre-Promise base attendance at Michigan four-year schools is lower, the proportional effect on Michigan four-year attendance is nearly 60 percent $(0.168/0.281 = 0.598)$.

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[23] These conclusions are only suggestive because the four-year enrollment effects are not statistically different from the any-enrollment effects. If we examine two-year college enrollment directly, we find a small, negative point estimate not statistically different from zero, in line with the difference in the estimates between the any-enrollment and four-year enrollment effects. The small point estimate for two-years likely masks offsetting effects, as some students upgrade to four-years and others are induced to attend college at the extensive margin.

[24] In our data, about 98 percent of students who enroll in a four-year college within 12 months of high school graduation do so within the first 6 months. Only 83 percent of students who enroll in a two-year college within 12 months of high school graduation do so within the first 6 months.

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The third panel shows that the gains at Promise-eligible schools are partially due to losses at ineligible institutions. Such attendance declined by 9.5 percentage points, or about 72 percent. Reassuringly, the sum of the estimates in Panels B and C accord quite closely with the net attendance results from Table 3. While not shown in Table 4A, the drop in attendance at noneligible schools is driven by out-of-state schools, not private schools in Michigan.

Table 4A Promise Effects on Enrollment by Type of School

	(1)
Panel A: Enroll at a Promise school within 6 months (Mean of DV | after=0, elig.=1) = 0.480	
After × Eligible	0.178*** [0.042]
R^2	0.138
Panel B: Enroll at a 4-yr. Promise school within 6 months (Mean of DV | after=0, elig.=1) = 0.281	
After × Eligible	0.168*** [0.035]
R^2	0.161
Panel C: Enroll at a 4-yr. non-Promise school within 6 months (Mean of DV | after=0, elig.=1) = 0.132	
After × Eligible	−0.095*** [0.023]
R^2	0.042

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for the eligible population in the pre-Promise period. Sample size is 5,415.

To better understand how the Promise affected enrollment at specific colleges, Table 4B shows estimates for the probability of first enrolling at each of several, relevant postsecondary institutions. We find that the overall increase in six-month enrollment is primarily driven by a 43 percent (=0.072/0.169) increase in the likelihood of attending the local public four-year, Western Michigan University (WMU), and a more than doubling in the likelihood of attending Michigan

State University (MSU), located about 80 miles away (=0.056/0.039). The estimated effect on

enrolling at the University of Michigan (UM) within six months is positive but imprecise.

Together the effect on the likelihood of enrolling at one of the "Flagships" (MSU or UM) within

six months is also quite large, an increase of about 89 percent (=0.066/0.074). Finally, we find no

economically or statistically significant effect on enrolling at Kalamazoo Valley Community

College (KVCC) or at the local, private, and (at the time) non-Promise-eligible liberal arts

school, Kalamazoo College (K). The patterns are nearly identical when extending the college

enrollment horizon to 12 months after graduation.[25]

Table 4B Promise Effects on College First Attended

Panel A: Enroll at a given school within 6 months

	KVCC	WMU	MSU	UM	Flagships	K
After × Eligible	0.011	0.072**	0.056***	0.010	0.066***	0.003
	[0.038]	[0.029]	[0.015]	[0.012]	[0.019]	[0.010]
Mean of DV	0.185	0.169	0.039	0.035	0.074	0.028

Panel B: Enroll at a given school within 12 months

	KVCC	WMU	MSU	UM	Flagships	K
After × Eligible	−0.013	0.078**	0.056***	0.010	0.066***	0.004
	[0.040]	[0.030]	[0.015]	[0.012]	[0.019]	[0.010]
Mean of DV	0.233	0.172	0.039	0.035	0.074	0.028

NOTE: Standard errors robust to heteroskedasticity are in parentheses ***, **, and * indicate p < 0.01, 0.05, or 0.10. See note to Table 4A. KVCC stands for Kalamazoo Valley Community College, WMU stands for Western Michigan University, MSU stands for Michigan State University, UM stands for University of Michigan-Ann Arbor, Flagships stands for either MSU or UM, and K stands for Kalamazoo College. The dependent variable means are for students who would have been eligible in the pre-Promise period.

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[25] The pattern reported in Table 4B is consistent with Andrews, DesJardins, and Ranchhod (2010), who find the Promise led to the largest increases in interest in submitting ACT scores to the flagship universities and WMU, with little change at KVCC or Kalamazoo College.

As discussed in the literature review, there is evidence that attending a higher-quality college has a positive impact on college completion (and, presumably, on subsequent earnings). Although the results in Table 4B are consistent with the Promise increasing average college quality of KPS graduates, it is worthwhile to examine whether the Promise had an impact on the college quality distribution more directly. To do this, we classify colleges based on 2004 *Barron's* selectivity categories, which range from "most selective" to "non-selective." In particular, we define a series of dummy dependent variables that equal one if the student is observed enrolling in a college of a given selectivity category or higher and zero otherwise; we then estimate equation (1) using a linear probability model. The results, shown in Appendix Table 1, indicate that the Promise increased the probability of enrolling in colleges in the "selective" and "very selective" categories, with no decline in the probability of attending colleges in the highest two selectivity categories.[26]

Credits Attempted

These positive enrollment effects align with previous studies, but do impacts persist? Table 5 explores medium-term effects: the number of credits attempted over different horizons after high school graduation.

Although the NSC data do not track credits explicitly, they report intensity of enrollment: full-time, half-time, and less than half-time.[27] We convert terms to a semester equivalent and assign values of 12 credits for full-time students, 6 credits for half-time students, and 3 credits for less than half-time students; these credits are summed over various time horizons since high school graduation, and students who never enroll are assigned zeros. We expect this credits-

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[26] We have also analyzed college quality effects using a Black and Smith (2006)-style quantile index. These results are similar to those using the *Barron's* measures and are available from the authors.

[27] NSC also reports whether the student withdrew before the end of term, which we interpret as 0 credits.

attempted variable to be highly positively correlated with credits actually earned and with eventual degree attainment.

Compared to enrollment outcomes in Tables 3, 4A, and 4B, the credits-attempted variables have fewer observations. The enrollment outcomes have data through the class of 2013, whereas the credits attempted at two years, three years, and four years after high school have data only through the classes of 2012, 2011, and 2010, respectively. Analysis of longer-term outcomes thus comes at the cost of fewer cohorts and less statistical precision.

The first panel of Table 5 shows that, as of two years after high school graduation, the Promise increased cumulative credits attempted by just over three credits, or about one class. In proportional terms, this increase is about 13 percent above the pre-Promise baseline.

The latter two panels of the table indicate that the boost to credits attempted persists over longer horizons, and, despite the smaller samples, maintains in statistical significance. As of three years after high school, the Promise has led students to enroll in 4.3 additional credits (about 12 percent above baseline); at four years after high school, the effect on credits attempted is 6.6 (14 percent). Put differently, after the equivalent of eight semesters, treated students took an additional two classes, which is a sizable gain.

Table 5 Promise Effects on Credits Attempted

	(1)
Panel A: Credits attempted at 2 years	
(Mean of DV | after=0, elig.=1) = 25.00	
After $\times$ Eligible	3.24**
	[1.65]
R^2	0.202
N	4,902
Panel B: Credits attempted at 3 years	
(Mean of DV | after=0, elig.=1) = 36.11	
After $\times$ Eligible	4.31*
	[2.47]
R^2	0.215
N	4,376
Panel C: Credits attempted at 4 years	
(Mean of DV | after=0, elig.=1) = 46.59	
After $\times$ Eligible	6.56*
	[3.36]
R^2	0.209
N	3,869

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for the eligible population in the pre-Promise period.

It is worth emphasizing that these results capture the extensive margin and not just the intensive margin. A natural question is whether credits attempted also increase among students who enroll in college. Although these results are not shown in the table, we have investigated the Promise's effect on credits attempted for both students who enroll in any college within 12 months of high school graduation and those who specifically enroll in four-year colleges. These estimates are not causal, certainly, given the enrollment results in Table 3, but we view them as interesting and descriptive. Among KPS graduates who enroll at all, we find the Promise has essentially no effect on the number of credits attempted. This would seem to suggest that the

results in Table 5 are driven by the extensive margin, but this is not quite the case. If we look at those who enroll specifically at four-year colleges, Promise-eligible students do increase the number of credits they attempt, generally by about 15 percent. The Promise, then, does not just boost credits attempted by increasing college enrollment at the extensive margin; rather, some students (those who enroll at four-year colleges) appear to take more classes, while other students (those who enroll at two-year colleges) appear to take fewer classes.[28]

Credential Completion

We now turn to Promise effects on educational attainment or degree completion (Table 6). Degree completion is arguably the most important outcome, whether for researchers or policymakers. This outcome is also one over which the literature is most divided on the effects of financial aid, with some studies finding positive impacts (e.g., Scott-Clayton 2011) and others none (e.g., Cohodes and Goodman 2014 and Sjoquist and Winters 2015).

Our degree attainment estimates focus on two outcomes at two time horizons. The outcomes are 1) receipt of any credential, including a certificate, an associate's degree, or a bachelor's degree, and 2) receipt specifically of a bachelor's degree. The time horizons are four years and six years after high school graduation.

For the four-year time horizon, we have follow-up data for five post-Promise cohorts, through the class of 2010. For the six-year time horizon, we have data for only three post-Promise cohorts, through the class of 2008. The reduction in post-Promise cohorts reduces the precision of our estimates.

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[28] This is consistent, for example, with the marginal students induced by the Promise to attend a two-year college, who otherwise would not have enrolled at all, disproportionately enrolling part-time. (Recall that the Promise allows part-time enrollment at the local two-year school, Kalamazoo Valley Community College.)

For the four-year horizon, for either any credential (Panel A) or bachelor's degree (Panel C), point estimates are close to zero. Standard errors are large enough that positive or negative effects of about 5–6 percentage points (relative to a baseline of 19 percent in Panel A and of 14 percent in Panel C) cannot be ruled out. But given that the median duration to a bachelor's degree is well over four years (Bound, Lovenheim, and Turner 2012; Cataldi et al. 2011), and that full funding from the Promise is available by taking the equivalent of just 12 credits per semester, or a five-year pace for a bachelor's degree, four years may be too short a time horizon over which to expect an impact.[29]

Over a six-year horizon, point estimates are positive and large. The Promise effect on attainment of any credential as of six years is about 10 percentage points. The 95-percent confidence intervals imply a wide range of possible effects, ranging from 1.1 to 19 percentage points. Still, the point estimate of 10.2 percentage points would be judged by most researchers and policymakers to be large. Relative to a pre-Promise mean credential attainment of 36 percent among eligibles, the estimates represent an increase in credential attainment of 28 percent (=0.102/0.36).

For the attainment of a bachelor's degree (Panel D), we again find sizable point estimates, although statistical significance is slightly weaker. The 7.4 percentage-point increase translates to a percentage boost of 25 percent in the likelihood of earning a bachelor's degree (0.074 divided by 0.30). These latter results suggest that most of the Promise effect on degree attainment comes from increasing bachelor's attainment.[30]

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[29] We obtain almost identical results for the four-year outcomes if we restrict the estimates to the fewer cohorts used in the six-year results.

[30] One could speculate that the Promise induces some students to get bachelor's degrees who otherwise would get associate's degrees, which reduces the net effects on non–bachelor's degrees.

Table 6 Promise Effects on Degree Attainment

	(1)
Panel A: Any credential at 4 years	
(Mean of DV | after=0, elig.=1) = 0.186	
After × Eligible	0.008
	[0.031]
R^2	0.087
Panel B: Any credential at 6 years	
(Mean of DV | after=0, elig.=1) = 0.360	
After × Eligible	0.102**
	[0.046]
R^2	0.146
Panel C: BA/BS at 4 years	
(Mean of DV | after=0, elig.=1) = 0.143	
After × Eligible	0.001
	[0.023]
R^2	0.116
Panel D: BA/BS at 6 years	
(Mean of DV | after=0, elig.= 1) = 0.300	
After × Eligible	0.074*
	[0.040]
R^2	0.179

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for the eligible population in the pre-Promise period. Sample sizes are 3,869 at four years and 2,905 at six years.

SENSITIVITY ANALYSES AND ADDITIONAL ESTIMATES

In this section, we examine the internal validity of our empirical approach and results through several tests of our identification strategy. Finding that our results are quite robust, we also explore heterogeneous impacts of the Promise on different types of students, particularly with regard to students' socioeconomic status, ethnicity, and gender.

Our identifying assumption is that the Promise-eligible and -ineligible groups do not have trends in unobservables that cause a divergence in postsecondary outcomes. As discussed earlier, there are two possible threats to this identification. First, there is the possibility of differential pretrending: i.e., that outcomes between the two groups were diverging even before the Promise. Second, the Promise may have induced changes in the composition of the two groups—perhaps due to selective in- and out-migration—that led to relatively more favorable outcomes for Promise-eligible students vis-à-vis ineligible students. We demonstrate below that the results are robust to excluding students who entered the district after the Promise and to allowing the relationship between student demographics and outcomes to vary differentially before and after the Promise. Moreover, we use district-level data to show that a between-district analysis, resting on different identification, largely buttress the findings from the within-KPS analysis.

Examining the Parallel Trend Assumption

To address the parallel trend assumption, we examine the differences (conditional on covariates) in postsecondary outcomes for the two groups separately for each year. This strategy allows for an investigation of common pretrending before the Promise as well as the timing of the Promise "effect" in subsequent years. If our identification assumption is valid, we should expect to see an abrupt increase in the outcome among eligibles in the first year of the program, with no clear change among ineligibles. Of course, there is also the possibility of a trending effect that is still consistent with true effects of the Promise. For example, over time, Promise-eligible students may become better prepared academically to use the Promise (Bartik and Lachowska 2013).

Formally, we estimate regression models of the following form:

$$(2) \qquad y_{ist} = \alpha + \sum_j (\delta_{2,j} Year_j) + \sum_j [\delta_{3,j} (Year_j \times Elig_{ist})] + x_{ist}\beta + \gamma_{st} + u_{ist},$$

where *Year* is a vector of calendar year dummies $j = \{2003, \ldots, 2013\}$. We set year 2003 as the omitted category in the vector of year effects, but allow all years j to be interacted with the "Elig"-indicator. Hence, equation (2) allows us to interpret each coefficient δ_3 as the average difference in the outcomes of eligible students relative to the outcomes of ineligible students in year j. The other variables are defined as in Equation (1).

Figure 3 Fitted Probabilities of 4-Year College Attendance Within Six Months
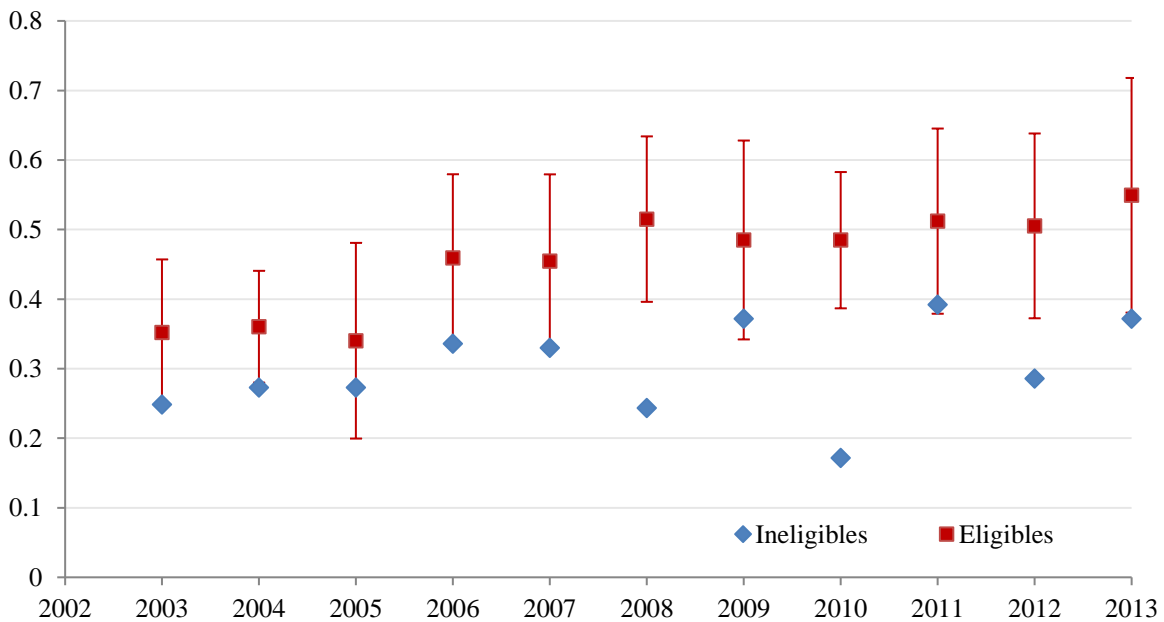
NOTE: The plotted values represent fitted probabilities of attending a four-year college within six months of high school graduation, by class year and Promise eligibility (column 1 of Table 3), but allowing the Promise effects to vary by year. The vertical black line indicates when the Promise began. The "Eligible" group includes those eligible for the Promise in the post-Promise period (class of 2006 and later) as well as those who would have been eligible for the Promise had it been around earlier (classes of 2003 through 2005); the "Ineligible" group is defined analogously. See the notes to Table 3. Point-wise 95-percent confidence intervals are shown for the difference between eligible and ineligibles.

In Figure 3 we present fitted probabilities of enrollment in a four-year school within six months of high school graduation (Table 3, Panel C, column 1).[31] If these fitted probabilities

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[31] The fitted values apply the coefficient estimates from the full interaction of eligibility and class year to the adjusted mean outcome of the omitted group (ineligibles in 2005), where the adjustment holds covariates at their

were diverging by eligibility in the pre-Promise period, we might be concerned that the Table 3 results were spurious. This is not the case: the fitted probabilities evolve similarly between the classes of 2003 and 2005. Reassuringly, there is a sharp spike in attendance among eligibles that begins for the class of 2006 and that remains elevated over the remaining horizon, perhaps even increasing slightly over time. The time path for ineligibles is noisy, owing to smaller sample sizes, but there is no sustained increase, and the probabilities oscillate from year to year. These patterns support our identifying assumptions.

Figures 4A, 4B, and 5 similarly present results allowing for the full interaction of Promise eligibility and cohort for the predicted number of credits attempted within two and four years of high school graduation (Figures 4A and 4B) and for the fitted probability of attaining a bachelor's degree within six years of high school graduation (Figure 5). The patterns are in accord with Figure 3: there is a jump among eligibles for the 2006 cohort, with the level remaining elevated over the rest of the cohort horizon; the levels for ineligibles fluctuate from year to year with no sustained increase.

The lack of any sustained post-Promise trend in academic effects makes it less likely that the Promise estimates are influenced by selective migration, which would be expected to lead to biases that grow over time.[32] The lack of a sustained post-Promise trend also suggests that—at least for these initial Promise-eligible cohorts—the Promise's effects have not grown dramatically over time due to students, parents, and the school district having more time to adjust behavior in response to the Promise.

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sample means over the whole analysis period. Appendix Table 2 shows the estimated δ_3 coefficients for each year j that Figures 3–5 are based on.

[32] This pattern is consistent with Hershbein's (2013) findings of minimal selective migration impacts following the Promise.

Figure 4A Predicted Number of Credits Attempted Within Two Years
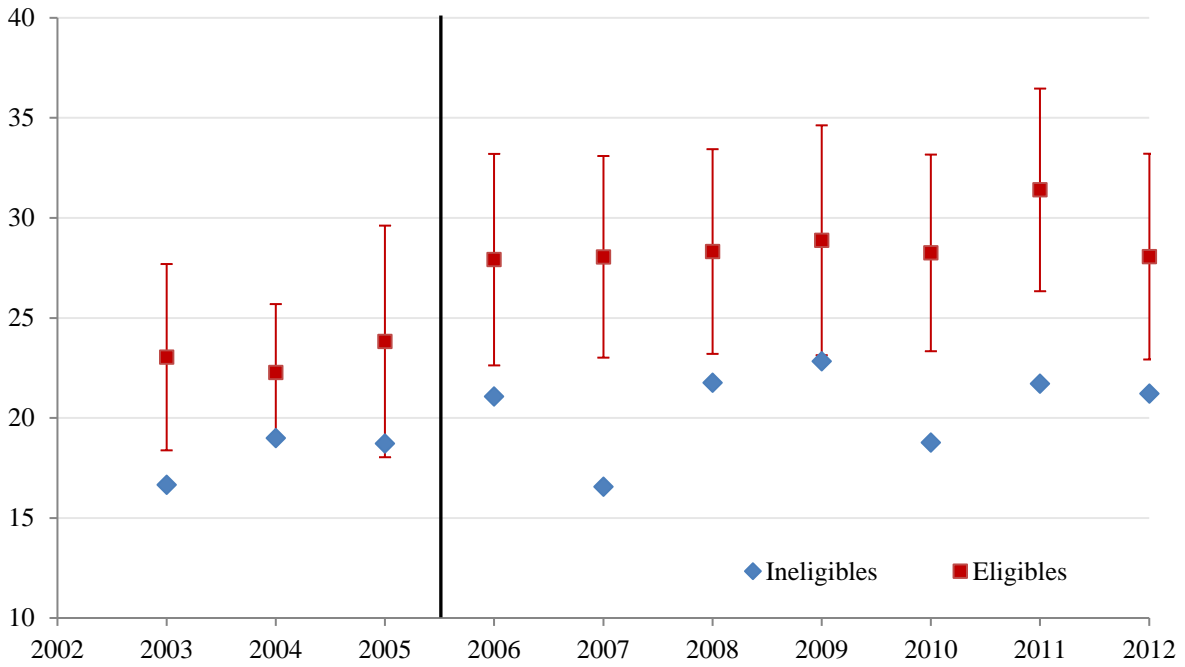
Figure 4B Predicted Number of Credits Attempted Within Four Years
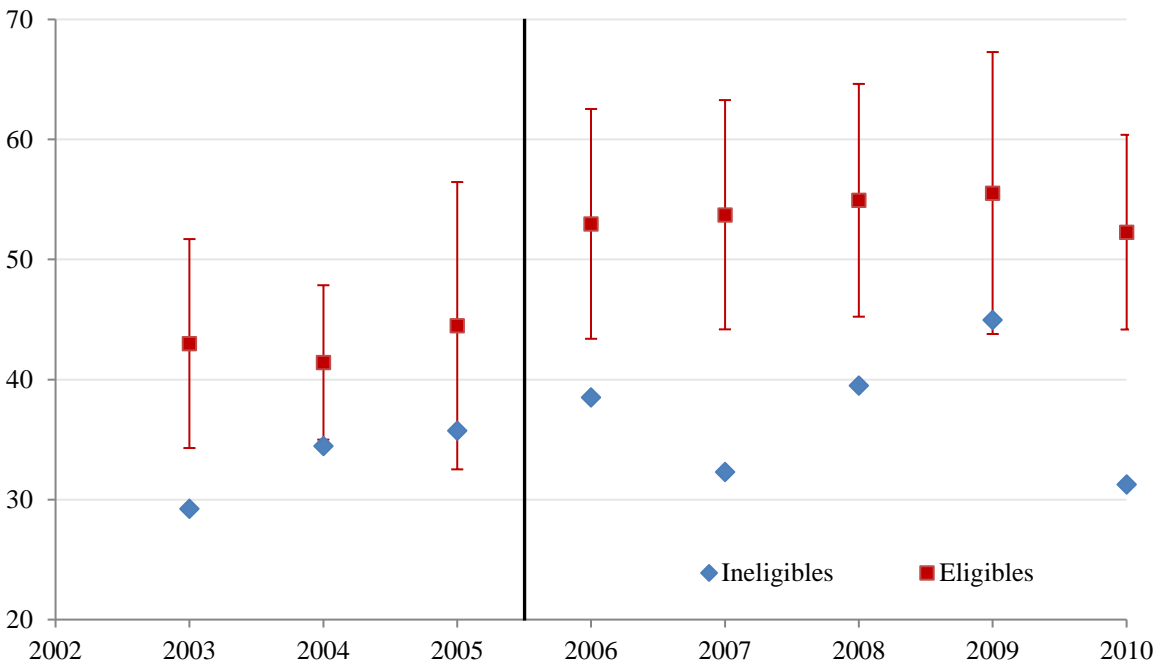
NOTE: The plotted values represent predicted number of credits attempted within two (Figure 4A) or four (Figure 4B) years of high school graduation, by class year and Promise eligibility (column 1 of Table 5), but allowing the Promise effects to vary by year. See the notes to Figure 3 and those to Table 5. Point-wise 95-percent confidence intervals are shown for the difference between eligible and ineligibles.

Figure 5 Fitted Probabilities of BA/BS Attainment Within Six Years
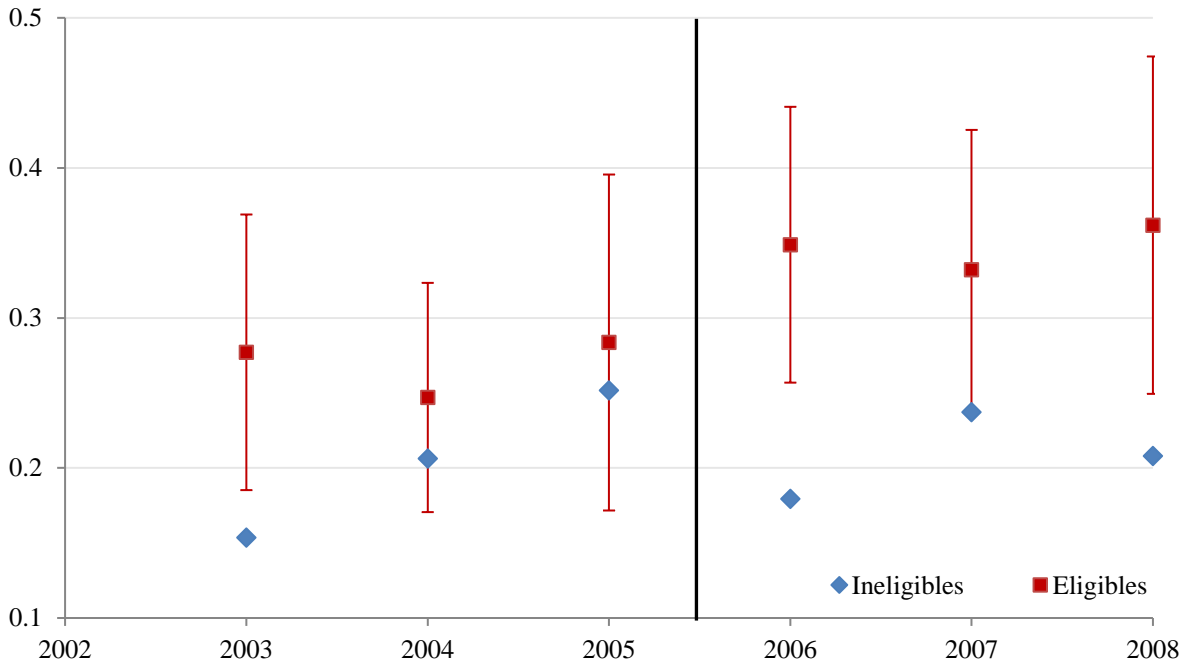
NOTE: The plotted values represent fitted probabilities of attaining a bachelor's degree within six years of high school graduation, by class year and Promise eligibility (column 1 of Table 6), but allowing the Promise effects to vary by year. See the notes to Figure 3 and those to Table 6. Point-wise 95-percent confidence intervals are shown for the difference between eligible and ineligibles.

Examining Selective In-Migration

To further address possible concerns of changing student composition, we check whether the estimates are robust to excluding students who enter KPS after the Promise. This strategy necessarily excludes an increasing number of students from the sample over time, particularly from the ineligible group, who are by definition later entrants. Therefore, lessening the risk of selection bias comes at the expense of estimate precision.

We have re-estimated selected outcomes—enrollment, credits attempted, and credential attainment—using this smaller sample that includes only students who were enrolled in KPS before the Promise was announced. The results, along with comparisons to the earlier estimates, are shown in Appendix Table 4. In most cases, the resulting point estimates from excluding late-

entrant students are not close to being substantively or statistically different from the baseline estimates in Tables 3 through 6.

The exception is for enrollment at a four-year college. When excluding new entrants, the point estimate shrinks, and the difference in estimates is statistically significant at the 5 percent level.

This finding has at least two alternative interpretations. The first interpretation is that dropping new entrants reduces selection bias, and therefore we should view these estimates of smaller Promise effects on four-year attendance as more reliable. The difficulty with this interpretation is that the completion estimates (including for bachelor's degrees) are robust to dropping late entrants, which would imply that the Promise increases postsecondary persistence without increasing 4-year attendance, which is inconsistent with previous research on college scholarships.

A second interpretation is that dropping new entrants may cause bias by making the post-Promise ineligible group less comparable to the pre-Promise ineligibles. Dropping new entrants seems to have its effect largely by reducing the ineligibles in 2008 to a much smaller group, whose students are more likely to go to a four-year college.[33] The exclusion of late entrants means that ineligibles from the class of 2008 contain no students who entered KPS in eleventh or twelfth grades, yet students who entered in these grades are still included among the *pre-Promise* ineligibles. If pre-Promise and post-Promise ineligibles are less comparable, then the ineligible dummy may not adequately control for fixed unobservables.

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[33] Dropping late entrants means that ineligibles for the class of 2007 must have entered KPS in tenth or eleventh grade and ineligibles in the class of 2008 must have entered KPS in tenth grade; ineligibles in later cohorts are excluded by construction. As a consequence, we lose 56 ineligible late-entrant students from the classes of 2008 or earlier, and 34 of these come from the class of 2008. The count of ineligibles in the class of 2008 falls from 54 (with four-year college enrollment of 0.185) to 20 students (with four-year college enrollment of 0.450).

Yet another way to control for the changing composition of our analysis sample is to estimate models where we allow the relationship between student characteristics *x* and outcomes to vary differentially before and after the Promise. These estimates, presented in Tables D1–D5 in Appendix D, are generally very similar to the baseline estimates that constrain the coefficients on student characteristics to be constant before and after the Promise.

Using an Alternative Eligibility Indicator

Our preferred determination of Promise eligibility uses administrative records from the Kalamazoo Promise for the classes of 2006 onward and an algorithm that assigns eligibility based on the residency and attendance rules of the Promise for the classes of 2005 and before. We believe this approach minimizes misclassification error.

However, we have also produced estimates using the eligibility assigned by the algorithm for the entire sample. Appendix A compares the eligibility assignment across the two approaches; in the Promise period when both are available, they match exactly for 95.5 percent of students. Of the cases where they disagree, the vast majority have the administrative data show eligibility when the algorithm does not. Apparently, some exceptions to the rules were made.

From conversations with the Promise administrators, these exceptions to the rules were apparently made to accommodate students who temporarily left KPS for behavioral or family issues, a rather disadvantaged group (see Appendix D). On the one hand, treating these students as eligible might bias estimates on attainment downward, as these needier students have low graduation probabilities on average. On the other hand, the motivation to seek eligibility on appeal suggests interest in at least attending college, and treating these students as eligible might lead to positive bias on enrollment outcomes.

In Appendix Table 5, we compare the estimates from our preferred specification (using the eligibility indicator from administrative records) with the estimates obtained using the algorithm throughout for assigning eligibility. By and large, the point estimates obtained using the latter are less precise and of smaller magnitude; however, for the key outcomes pertaining to credentials obtained after six years, we find that the estimates from the two eligibility assignment mechanisms are similar in magnitude and not statistically different from each other. This speaks to the robustness of our findings. By treating as ineligible (in the algorithm) students whom we know to be eligible (based on administrative data), we necessarily introduce misclassification error, and this will bias estimates toward zero. That our most important results survive this bias suggests the Promise effects are strong.[34] Appendix D presents the full set of estimates obtained using algorithm-based eligibility.

Another way to deal with the problem caused by the difference between the rules-based algorithmic eligibility assignment and actual Promise assignment is to use instrumental variables. In Appendix E, we present a full set of estimates in which we instrument actual (administrative) eligibility with eligibility based on the algorithm. In general, the estimates are quite similar. Where they differ, the instrumental estimates tend to be somewhat greater than baseline, and particularly so for six-year attainment outcomes. This pattern supports the notion that using administrative eligibility if anything biases us against finding Promise effects.

Between-District Analysis of Promise Effects

In this subsection, we estimate the effect of the Promise on enrollment and credentials using district-level data on high school graduates provided to us through the Michigan

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[34] In additional results available on request, we have also tried simply dropping from estimation the students for whom the eligibility assignment mechanisms disagree. This approach produces estimates nearly identical to our baseline estimates.

Consortium for Educational Research (MCER).[35] For each Michigan high school district, we observe graduation-cohort averages of college-going outcomes for the classes of 2003 through 2013. These data allow us to compare KPS to other districts in Michigan before and after the announcement of the Promise, an identification strategy that supplements our more-detailed within-KPS analysis. In practice, we estimate the regression equation:

$$(3) \quad y_{st} = \alpha + \delta_1 KPS_{st} + \delta_2 After_{st} + \delta_3 (After \times KPS)_{st} + \boldsymbol{x_{st}\beta} + \boldsymbol{\gamma_s} + \boldsymbol{\gamma_t} + \boldsymbol{\gamma_s} t + e_{st},$$

where s denotes the district and t denotes the academic year. The outcome variable of interest is y; "KPS" equals one for KPS and zero for the other control school districts; "After" equals one if the data are observed after 2005 and zero if before; and "After $\times$ KPS" is an interaction between the two indicators. The vector $\boldsymbol{x}$ includes time-varying district controls: the proportions of students to teachers, students eligible for subsidized lunch, black students, and Hispanic students. These controls are taken from the U.S. Department of Education's Common Core of Data and are district-wide, not just for high school graduates. (When a control is missing, we set its value to the cross-district mean that year and include a dummy to indicate the missing). $\boldsymbol{\gamma_s}$ denotes district fixed effects and $\boldsymbol{\gamma_t}$ denotes the year-of-graduation time effects. As parallel trending assumptions become more tenuous across a multitude of districts, our preferred specification also controls for district-specific time trends, $\boldsymbol{\gamma_s} t$ (in Table C2, in the appendix, we show results without the district-specific time trends). We weight observations by the size of the district's graduating class.

The coefficient of main interest is δ_3, the regression-adjusted difference in average college-going outcomes of students from KPS, compared both to other school districts and to

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[35] The MCER is a cooperative endeavor between the University of Michigan, Michigan State University, and the Michigan Department of Education to produce a harmonized version of the state's administrative student-level, longitudinal education data. Through special agreement, we received aggregated data from an earlier period than is generally available to researchers.

pre-Promise outcomes. Our preferred set of control school districts are the members of the Michigan Middle Cities Education Association (MCEA), a consortium of 31 middle-sized Michigan urban school districts (http://www.middlecities.org) that we deem likely to face similar challenges as KPS.[36] Table C1, in the appendix, presents graduate-weighted summary statistics for KPS, the MCEA districts, and all 511 districts in Michigan.

Table 7 presents estimates of δ_3 for our main outcomes of enrollment and credential completion (credits attempted are not available in the data). Note that because we have a single "treated" district (KPS), clustering the standard errors by district will not yield correct standard errors (Conley and Taber 2011). While for context we present heteroskedasticity-robust standard errors (which are indeed more conservative than when clustering by district), we employ the permutation-inference approach recommended by MacKinnon and Webb (2016) to calculate the p-value. This approach essentially assigns a placebo treatment to every possible control, calculates the t-statistic in each case, and compares the t-statistic from the estimate on the actual treatment to the distribution of placebo t-statistics.

Turning to the results, Table 7 shows that the percentage changes implied by the point estimates are remarkably similar to those obtained using the within-KPS analysis. Specifically, column 1 shows that the Promise increased enrollment in a four-year school within six months of graduation by about 7 percentage points, which is a 27 percent increase (=0.071/0.259). This is quite close to the 23 percent increase implied by Table 3, Panel C.

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[36] The MCEA districts include: Albion, Battle Creek, Bay City, Beecher, Benton Harbor, Dearborn, Ferndale, Flint, Garden City, Grand Rapids, Hazel Park, Highland Park, Jackson, Kalamazoo, Lansing, Monroe, Mt. Clemens, Mt. Pleasant, Muskegon, Muskegon Heights, Niles, Pontiac, Port Huron, Romulus, Saginaw, Southfield, Waterford, Wayne-Westland, Westwood, Willow Run, and Ypsilanti.

Table 7 Promise Effects on Enrollment and Completion using Between-District Analysis

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
					Credential at 6 years:	
	Enrollment within 6 months at:					
VARIABLES	4-yr school	4-yr.Promise school	4-yr. non-Promise school	Flagship school	Any credential	BA/BS
After × KPS	0.071**	0.110**	-0.039*	0.085**	0.061	0.039
Robust standard error	[0.024]	[0.019]	[0.013]	[0.026]	[0.044]	[0.040]
Permutation inference *p*-value	0.032	0.032	0.065	0.032	0.258	0.258
Observations	332	332	332	332	181	181
Adjusted R-squared	0.834	0.830	0.721	0.743	0.906	0.897
Mean DV	0.259	0.181	0.0781	0.0414	0.238	0.179

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. p-value is obtained using a placebo-regression permutation inference described in the text. Regressions include district-by-year proportions of students to teachers, students eligible for subsidized lunch, African-American students, and Hispanic students. For each observable, we also include the proportions of missings. The regressions control for district fixed effects, year-of-graduation time effects, and district-specific linear time trends. Observations are weighted by the number of district graduates. The mean of the dependent variable is for the control districts in the pre-Promise period. The control districts consist of the Michigan Middle Cities Education Association (MCEA) districts described in the text.

Column 2 shows that the likelihood of enrolling in a four-year Promise-eligible school increased by 61 percent (=0.110/0.181), while column 3 shows that the likelihood of enrolling in a four-year non-Promise school decreased by 49 percent (=0.039/0.079), which are only slightly smaller than the implied relative changes from Table 4a. Finally, column 4 shows that the Promise had a large effect on increasing enrollment in Michigan flagship schools.

Turning to the completion results, the last two columns show that the probability of obtaining any credential after six years increases by about 26 percent (=0.061/0.238), and the probability of obtaining a bachelor's degree after by about 22 percent (=0.039/0.179). These two changes are also remarkably close to those reported in Table 6, panels B and D (28 percent and 25 percent, respectively). However, unlike in Table 6, the completion point estimates in Table 7 are not statistically different from zero at conventional thresholds. Nevertheless, these point

estimates are clearly economically meaningful and strikingly similar to the estimates obtained using variation within KPS. The similarity of the between-district and within-KPS analyses corroborates the causal impact of the Promise.[37]

HETEROGENEITY

An important question is the extent to which Promise effects vary across demographic groups. The simplicity of the Kalamazoo Promise means that the scholarship is not necessarily targeted at those who need it most (the financially constrained) or those who would be expected to benefit most (the academically capable). Although the aggregate credential completion estimates imply that the Promise does *not* simply reflect an income transfer to supramarginal students, it is entirely possible that the gains are concentrated among relatively more-advantaged groups, which could limit its potential (and that of other place-based scholarships) to promote social mobility.

Table 8 reports selected results (using our within-KPS data) for how Promise effects vary with family income (proxied by free or reduced-price lunch status), race, and gender. For conciseness, we focus on three outcomes: four-year college attendance within six months, credits attempted at two years, and six-year attainment of a bachelor's degree.

As shown in the first panel, the estimated Promise effects are both substantively and statistically similar for lower-income students and their higher-income peers. Notably, both groups experience sizable gains in enrollment and credits attempted, and the magnitudes for baccalaureate completion are considerable even if the estimates are imprecise.

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[37] In Appendix C, we also present results using nearly all other school districts in Michigan as the control set, as well as results using the Abadie, Diamond, and Hainmueller (2010) synthetic control method approach.

Table 8 Promise Effects by Group

Income groups	6-month attendance at 4-year		Credits attempted after 2 years		6-year BA/BS attainment	
	Non-low income	Low-income	Non-low income	Low-income	Non-low income	Low-income
After × Elig	0.145**	0.093**	5.80**	2.90	0.115	0.051
	[0.066]	[0.046]	[2.70]	[2.12]	[0.076]	[0.041]
N	2,666	2,744	2,457	2,440	1,641	1,259
p-val of group diff		0.524		0.399		0.466
Mean DV	0.509	0.196	29.64	16.1	0.399	0.108
Race	White	Non-white	White	Non-white	White	Non-white
After × Elig	0.109	0.088*	−0.01	5.40***	0.030	0.074
	[0.067]	[0.046]	[2.75]	[2.07]	[0.069]	[0.047]
N	2,624	2,791	2,410	2,492	1,545	1,360
p-val of group diff		0.791		0.113		0.599
Mean DV	0.506	0.257	29.51	18.68	0.398	0.161
Gender	Male	Female	Male	Female	Male	Female
After × Elig	0.080	0.116**	1.47	5.16**	0.018	0.133**
	[0.057]	[0.052]	[2.35]	[2.29]	[0.054]	[0.058]
N	2,551	2,864	2,293	2,609	1,388	1,517
p-val of group diff		0.639		0.257		0.144
Mean DV	0.400	0.405	25.65	24.43	0.295	0.304

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicates *p* less than 0.01, 0.05, or 0.10. Timing is since high school graduation. All regressions include dummies for after the Promise's introduction, individual (pseudo-) eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year (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.

Although point estimates are slightly higher for students who were not eligible for free or reduced-price lunches, the differences are small and not statistically significant. Moreover, because the baseline postsecondary outcomes are much lower for the low-income groups, the effect of the Promise in proportional terms is much higher for these students. In particular, their attendance at four-year colleges increases by about 50 percent (about twice the percentage increase for higher-income students), credits attempted within two years jumps (imprecisely) 18

percent (20 percent for higher-income students), and bachelor's completion within six years rises (imprecisely) by 47 percent (29 percent for higher-income students). These results suggest that the Promise does not benefit only students from relatively well-to-do families but reaches broadly across the economic spectrum.

The second panel shows differential results for white students and minority students (who are overwhelmingly black or Hispanic; see Table 2). The estimates on four-year college enrollment are nearly identical across the racial groups, at 9–10 percentage points, but only the effect for nonwhite students is statistically significant, and because of the lower baseline for these students, the effect is twice as large in proportional terms relative to white students. For credits attempted, the effect for white students is a relatively imprecise zero, but for nonwhite students it is a statistically significant 5.4 credits, or almost two classes; remarkably, this represents a 29 percent increase from baseline. For baccalaureate completion, neither estimate approaches statistical significance, owing to the reduced sample size, but the point estimate for nonwhite students is roughly 2.5 times as large as it is for white students, and, proportionally, approaches an effect of 46 percent. In accord with the first panel, the Promise boosts postsecondary outcomes among racial minorities (who typically are economically disadvantaged) at least as much as it does for white students.

Comparing males and females in the third panel, it is clear that Promise effects are considerably larger for women than for men. Unlike the previous two panels, baseline means vary relatively little across sexes, but the point estimates are consistently large and statistically significant for women and small or close to zero and statistically insignificant for men. (Because of the reduced sample sizes, these differences are seldom statistically different, however.) The gap between men and women is particularly stark for baccalaureate completion: the Promise

boosts women's attainment by 13.3 percentage points (44 percent), while men seem to experience zero benefit.[38]

To investigate whether these within-KPS results are also present in the between-district analysis, we have estimated versions of model (3) for subgroups defined by income (again proxied by free or reduced-prize lunch status), race (white vs nonwhite), and gender. Table 9 presents results for enrollment at a four-year college within six months of high school graduation and for obtaining a bachelor's degree within six years of graduation. As in Table 8, estimates are similar for low-income and non-low-income students, although the former group has a larger percentage increase due to a lower baseline. While the point estimates are economically large, they are noisy, coming from a sample of a few hundred observations, some of which may be based on relatively few students in an income group.

Turning to the results by race, the results differ more from those in Table 8. Although both whites and minority students experience an increase in enrollment from a similar baseline, the increase for the latter is much smaller and is imprecisely estimated. Whereas the likelihood of a bachelor's degree increases by 40 percent (=0.082/0.203) for white students, the point estimate is small, imprecise, and negative for students of color. It is not clear why these between-district results by race differ from the within-KPS ones, but it is possible that white students in KPS were on a different trajectory than their counterparts in the comparison districts vis-à-vis the analogous trajectory for students of color.

The last panel of Table 9 shows results by gender. We again observe a large increase in the likelihood of enrollment for both males and females, although the relative effect for men is larger here than in Table 8. Once again, however, only women experience a large (0.0904/0.201

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[38] This result could potentially be explained by the recent findings of Autor et al. (2015), who offer evidence that family disadvantage has a more harmful effect on the academic outcomes of boys than girls.

= 45 percent) and statistically significant increase in the likelihood of obtaining a bachelor's

degree. For males, the point estimate is small and negative.

Table 9 Between-District Estimates of Promise Effects by Group

Income groups	6-month enrollment at 4-year		6-year BA/BS attainment	
	Non-low income	Low-income	Non-low income	Low-income
After × KPS	0.0823*	0.0877	0.0450	0.0360
	[0.0340]	[0.0493]	[0.0469]	[0.0649]
Permutation p-value	0.0323	0.1290	0.3226	0.3871
N	327	330	177	179
Mean DV	0.299	0.196	0.213	0.105
Race	White	Non-white	White	Non-white
After × KPS	0.129*	0.0226	0.0820	-0.0278
	[0.0516]	[0.0391]	[0.0402]	[0.0948]
Permutation p-value	0.0968	0.3871	0.1613	0.6774
N	317	332	174	181
Mean DV	0.274	0.242	0.203	0.140
Gender	Male	Female	Male	Female
After × KPS	0.0903**	0.0559*	-0.0137	0.0904
	[0.0236]	[0.0354]	[0.0463]	[0.0390]
Permutation p-value	0.0323	0.0645	0.6774	0.1613
N	332	332	181	181
Mean DV	0.234	0.283	0.155	0.201

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate p < 0.01, 0.05, or 0.10. p-value is obtained using a placebo-regression permutation inference described in the text. Regressions include district-by-year proportions of students to teachers, students eligible for subsidized lunch, African-American students, and Hispanic students. For each observable, we also include the proportions of missings. The regressions control for district fixed effects, year-of-graduation time effects, and district-specific linear time trends. Observations are weighted by the number of district graduates. The mean of the dependent variable is for the control districts in the pre-Promise period. The control districts consist of the Michigan Middle Cities Education Association (MCEA) districts described in the text.

To summarize, the heterogeneity analyses based on both the within-KPS sample and the

between-district sample indicate that Promise effects were not restricted to higher-income

students but benefitted low-income and non-low-income students similarly. Moreover, both sets

of analyses imply that women benefitted more from the Promise than men. However, our results for different racial groups are more sensitive to these different methodological approaches.

DISCUSSION AND CONCLUSION

In this paper we show that the Kalamazoo Promise, a universal, place-based college scholarship, has large effects on postsecondary outcomes. Our estimates consistently demonstrate sizable percentage effects on postsecondary attendance, credits, and attainment, and these estimated effects are robust to different identification strategies, specifications, and assumptions of student composition. The estimates accord with both theory—for example, substitution of enrollment from Promise-ineligible to Promise-eligible colleges—and previous suggestive evidence from ACT score-sending (Andrews, DesJardins, and Ranchhod 2010).

Indeed, the pattern of Promise effects across students is quite similar to that in Angrist et al. (2015), who study the effect of full college scholarships that were randomly assigned to academically talented high school seniors in Nebraska. They find the strongest effects on enrollment and second-year persistence for disadvantaged groups, such as racial minorities.

In contrast, the pattern of Promise effects is different from that of Cohodes and Goodman (2014), who study the effects of the Adams scholarship, a tuition-waiver program available to students attending public colleges in Massachusetts. Cohodes and Goodman find that the scholarship increased enrollment but, by shifting the college choice to lower quality publics in Massachusetts, resulted in lower college graduation rates. The contrast between our findings and those of Cohodes and Goodman illustrates clearly that the local context of college scholarships matters for postsecondary success. Given the public versus private college options available to Michigan students, the Kalamazoo Promise incentivized students to trade up, rather than down,

the college quality spectrum. With the exception of the Northeast, the Michigan context is more relevant to the nation than the Massachusetts context.

We also find that the Promise had a stronger impact on women than on men. We speculate that this finding could be explained by the results in Autor et al. (2015), who document that family disadvantage has a disproportionately stronger effect on the educational outcomes of boys than of girls. It is important, however, to keep in mind that our primary identification strategy for identifying Promise effects focuses on cohorts who found out that they would be Promise-eligible relatively late. If boys tend to mature later than girls, then these cohorts of boys might not have been affected by the Promise the same way as the girls were. If this is the explanation, then our results for men might not generalize to later time periods or other settings.

We emphasize that the primary identification strategy we have used in this paper provides a conservative estimate of Promise effects. By its design, it cannot capture Promise effects that are school-wide or community-wide.[39] For example, the Promise has led to intensive effects, by both KPS school officials and many in the Kalamazoo community, to encourage a more "college-going culture" among students and their parents and guardians. Billboards, mailings, class meetings, and school-wide and community-wide meetings inform parents and students of college's nature and benefits and the application process. Counseling, tutoring, and support services encouraging students to stay in school and succeed have been initiated. More Advanced Placement courses have been offered. These KPS and community efforts may affect the college enrollment and success of all KPS students, both Promise-eligible and -ineligible.

Nonetheless, and despite these limitations, the Promise effects are large, and they speak to the potential of place-based scholarship programs to be a cost-effective way of increasing

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[39] The supplementary between-district analysis can partially capture district-wide effects, but relies on a somewhat different counterfactual.

earnings. A back-of-the-envelope calculation drawing on our degree completion estimates (based on Bartik, Hershbein, and Lachowska 2016) shows that the present value of increased career earnings exceeds the costs of Promise tuition subsidies at all real discount rates up to 11.3 percent.[40] At a real discount rate of 3 percent (5 percent), the implied Promise earnings effects have a present value that is 4.7 (3.0) times the present value of Promise subsidy costs. Since we believe the external validity of our results to be high, at least in similar contexts, this conclusion could likely apply to other urban school districts considering setting up their own Promise-like scholarships—to the extent that they closely follow the Kalamazoo model in terms of universality and generosity.

On the other hand, a Promise-like scholarship has the potential for solving only a portion of America's skills challenge. The Promise increases postsecondary credential attainment at six years after high school graduation from about 36 percent to about 46 percent. Presumably some of the remaining 54 percent might benefit from receipt of a postsecondary educational credential. As one might expect, "free" college is insufficient by itself to ensure successful postsecondary education. However, our results indicate that a simple, universal, and generous scholarship program can significantly increase educational attainment of American students. In addition, our results indicate that a simple universal scholarship can help low-income as well as non-low-income students, and therefore have broad benefits.

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[40] In comparison, Zimmerman (2014) estimates that the internal rate of return of admitting academically marginal students to four-year colleges is between 6 and 14 percent.

Appendix

Appendix Table 1 Promise effect on enrollment at colleges categorized by 2004 Barron's selectivity categories

Dependent variable	Most selective	≥ Highly selective	≥ Very selective	≥ Selective
After × Eligible	0.001	0.004	0.053**	0.121***
	[0.007]	[0.017]	[0.023]	[0.038]
Observations	5,415	5,415	5,415	5,415
Mean of DV	0.006	0.078	0.141	0.387

NOTE: Standard errors robust to heteroskedasticity are in brackets. *** , **, and * indicates p less than 0.01, 0.05, or 0.10. The category of college is based on the first college attended within the first 12 months after high school graduation. "Most selective" denotes the most competitive colleges. "Highly selective" includes University of Michigan-Ann Arbor and Kalamazoo College; "very selective" includes Michigan State University; "selective" includes Western Michigan University. All regressions include dummies for after the Promise's introduction, individual (pseudo-) eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and, and high school of graduation-by-graduation year.

Appendix Table 2 Promise effects by year for selected outcomes

Dependent variable	(1) Enrollment at 4-yr. within 6 months	(2) Credits attempted at 2 years	(3) Credits attempted at 4 years	(4) BA/BS at 6 years
Regressor				
2003 × Eligible	0.103*	6.381***	13.744***	0.124***
	[0.054]	[2.376]	[4.441]	[0.047]
2004 × Eligible	0.087**	3.269*	6.962**	0.041
	[0.041]	[1.746]	[3.282]	[0.039]
2005 × Eligible	0.067	5.096*	8.748	0.032
	[0.072]	[2.953]	[6.106]	[0.057]
2006 × Eligible	0.123**	6.835**	14.452***	0.169***
	[0.061]	[2.697]	[4.884]	[0.047]
2007 × Eligible	0.125**	11.488***	21.431***	0.095**
	[0.064]	[2.571]	[4.873]	[0.048]
2008 × Eligible	0.272***	6.560**	15.430***	0.154***
	[0.061]	[2.610]	[4.947]	[0.057]
2009 × Eligible	0.113	6.043**	10.574*	
	[0.073]	[2.931]	[5.992]	
2010 × Eligible	0.313***	9.474***	21.009***	
	[0.050]	[2.508]	[4.138]	
2011 × Eligible	0.120*	9.685***		
	[0.068]	[2.584]		
2012 × Eligible	0.220***	6.842***		
	[0.068]	[2.624]		
2013 × Eligible	0.177**			
	[0.086]			
Observations	5,415	4,902	3,869	2,905

NOTE: The coefficient estimates represent the average difference in the outcomes of eligible students relative to the outcomes of ineligible students in each given year. Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicates $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions also include dummies for each graduation year, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year.

Appendix Table 3 Promise Effect with Clustered Standard Errors

Dependent variable	Enrollment				Credits attempted at			Credentials			
	Within 6 months	Within 12 months	At 4-yr. within 6 months	At 4-yr. within 12 months	2 years	3 years	4 years	Any at 4 years	Any at 6 years	BA/BS at 4 years	BA/BS at 6 years
After × Elig	0.083	0.059	0.094	0.089	3.24	4.31	6.56	0.008	0.102	0.001	0.074
	(0.042)	(0.041)	(0.038)	(0.039)	(1.65)	(2.47)	(3.36)	(0.031)	(0.046)	(0.023)	(0.040)
	[0.030]	[0.027]	[0.038]	[0.038]	[1.32]	[2.21]	[3.02]	[0.036]	[0.049]	[0.024]	[0.036]
Number of clusters	33	33	33	33	30	27	24	24	18	24	18

NOTE: The table compares the baseline standard errors estimates (in parentheses) to standard errors clustered by high school × cohort (in brackets). All regressions include dummies for after the Promise's introduction, individual (pseudo-) eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year.

Appendix Table 4 Robustness to Excluding Late Entrants

Dependent variable	Enrollment				Credits attempted at			Credentials			
	Within 6 months	Within 12 months	At 4-yr. within 6 months	At 4-yr. within 12 months	2 years	3 years	4 years	Any at 4 years	Any at 6 years	BA/BS at 4 years	BA/BS at 6 years
Panel A: Baseline											
After × Elig	0.083**	0.059	0.094**	0.089**	3.24**	4.31*	6.56*	0.008	0.102**	0.001	0.074*
	[0.042]	[0.041]	[0.041]	[0.039]	[1.64]	[2.47]	[3.36]	[0.031]	[0.046]	[0.023]	[0.040]
Panel B: No late entrants											
After × Elig	0.091	0.060	0.016	−0.008	1.95	1.99	4.05	0.023	0.078	0.006	0.064
	[0.058]	[0.056]	[0.055]	[0.055]	[2.35]	[3.39]	[4.44]	[0.040]	[0.054]	[0.031]	[0.046]
Panel B: p-value of test of difference between coefficients in panel A and panel B											
p-val of diff	0.842	0.975	0.043	0.013	0.425	0.301	0.358	0.540	0.321	0.808	0.619

NOTE: The table compares the baseline estimates (Panel A) to estimates of samples restricted to students enrolled in KPS at the time of Promise announcement and to the classes of 2003 through 2008 (Panel B). (All ineligible students from the classes of 2009 or later either had interrupted KPS enrollment or entered the district after the Promise was announced). Standard errors are in parentheses. ***, **, and * indicates *p* less than 0.01, 0.05, or 0.10. All regressions include dummies for after the Promise's introduction, individual (pseudo-) eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year.

Appendix Table 5 Effects of the Promise estimated using alternative eligibility indicator

Dependent variable	Enrollment				Credits attempted at			Credentials			
	Within 6 months	Within 12 months	At 4-yr. within 6 months	At 4-yr. within 12 months	2 years	3 years	4 years	Any at 4 years	Any at 6 years	BA/BS at 4 years	BA/BS at 6 years
Panel A: Baseline estimates of effect of Promise obtained using administrative and predicted (algorithm) eligibility											
After × Eligible	0.083**	0.059	0.094**	0.089**	3.24**	4.31*	6.56*	0.008	0.102**	0.001	0.074*
	[0.042]	[0.041]	[0.041]	[0.039]	[1.64]	[2.47]	[3.36]	[0.031]	[0.046]	[0.023]	[0.040]
Panel B: Estimates of effect of Promise obtained using predictive (algorithm) eligibility only											
After × Eligible (predicted)	0.049	0.016	0.049	0.042	0.97	1.33	2.49	−0.028	0.082*	−0.020	0.056
	[0.039]	[0.039]	[0.037]	[0.037]	[1.56]	[2.32]	[3.17]	[0.030]	[0.043]	[0.022]	[0.037]
Panel C: p-value of test of difference between coefficients in panel A and panel B											
p-value	0.028	0.002	0.002	0.001	0.000	0.001	0.002	0.006	0.291	0.018	0.258
Observations	5,415	5,415	5,415	5,415	4,902	4,376	3,869	3,869	2,905	3,869	2,905

NOTE: Standard errors are in parentheses. ***, **, and * indicate p < 0.01, 0.05, or 0.10. Each cell is a separate regression. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. Panel A uses observed Promise eligibility in the post-Promise period and predicted (pseudo-) eligibility in the pre-Promise period. Panel B uses predicted (pseudo-) eligibility in both the pre- and post-Promise period; as noted in the text, this approach assigns some students to ineligibility even though they were in fact eligible according to administrative records.

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