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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 minorities and women. Predicted lifetime earnings effects of the Promise's credential gains, compared to the Promise's scholarship costs, represent an internal rate of return of 11.3 percent. Based on our results, simple and generous scholarships can significantly increase educational attainment and provide net economic benefits.

JEL Classification Codes: I21, I22, I24

Key Words: place-based scholarship, enrollment, college completion, natural experiment, difference-in-differences, education policy

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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, more than 30 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 state appropriations to public colleges having declined over the past 15 years (Baum and Ma 2014), 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 Promise effects, we estimate difference-in-differences models that compare outcomes of eligible

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

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 subsides, 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. Our core 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.

We examine both shorter-term outcomes, such as enrollment, as well as medium- and longer-term outcomes, such as credits attempted and credentials earned. We find substantively large and statistically significant effects of the Promise on many postsecondary outcomes, including the following three: 1) the probability of any college enrollment as well enrollment at a four-year college, 2) the cumulative number of credits attempted within two to four years after high school graduation, and 3) the probability of obtaining any postsecondary credential as well as the probability specifically of obtaining a bachelor's degree.

Due to the Promise, we estimate that the chance of students enrolling in any college within six months of high school graduation increases by 14 percent, and the chance of students enrolling in a four-year college is estimated to increase by 34 percent. Based on our estimated Promise effects, the cumulative number of credits attempted increases by 15 percent as of two

years after high school graduation, and these effects persist. At two years out, the effects imply an additional class attempted; at four years out, they imply an additional two to three classes attempted. As of six years after high school graduation, the Promise increases the percentage of students earning any postsecondary credential by 12 percentage points, from a pre-Promise baseline of 36 percent to 48 percent; this represents a proportional increase in credential attainment of one-third. About four-fifths of this boost in postsecondary credentials is due to more students earning a bachelor's degree. The credential attainment effects imply a large internal rate of return in increased earnings—over 11 percent—relative to the costs of the Promise's tuition subsidy.

These 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, our proxy for family income. Promise effects are at least as great and often greater for nonwhite students than for white students.

Importantly, these Promise estimates are conservative in two ways. First, by comparing Promise-eligible versus -ineligible students, these 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 more 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 might be attenuated if the Promise results in more marginal students being included among KPS graduates.

In the next section of this paper, 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) provide an excellent review, and for brevity's sake we only skim the surface here.

Most prior research has focused on merit scholarships, as they are more common and thus offer more plentiful identifying variation. These sorts of scholarships typically require a minimum high 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 (Pallais 2007). 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 (2012) find that state-based merit aid programs, as a whole, have generally not been successful in increasing college attainment. Nonetheless, Deming and Dynarski (2010) conclude that several merit-based programs have large enough effects to pass a social benefit-cost test: the increase in the present value of future earnings of affected students exceeds the scholarship's costs.

Less research has investigated broad-based scholarships with minimal academic requirements like the Kalamazoo Promise. 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, an important and 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.

The study of place-based scholarships, in contrast, has not yet reached early adolescence. Much of the existing research on place-based scholarships has focused on community or school district outcomes, such as housing prices and K–12 enrollment. 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 sub–state scale and being designed to reinvigorate the local community in addition to promoting higher education. Indeed, Bartik, Eberts, and Huang (2010) and Hershbein (2013) 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 (2014) 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.

While we might expect a place-based scholarship to attract families into a school district, a skeptic might argue that such a scholarship, with minimal academic requirements, will fail to encourage postsecondary success. The emerging evidence suggests, however, that the Kalamazoo Promise has improved high school outcomes and shaped postsecondary school choices. Andrews, DesJardins, and Ranchhod's (2010) study examines the 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. They find that the Promise increased score-sending to baccalaureate-granting 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.

In a paper using a similar identification strategy to the present study, Bartik and Lachowska (2013) compare high school achievement and behavior of Promise-eligible students versus ineligible students before and after the Promise announcement. They find that the Promise reduced disciplinary incidents, and, for African American students, increased GPAs. If the Promise alters students' high school success, 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 southwestern Michigan. Like many urban school districts, KPS is both poorer and more ethnically diverse than surrounding areas. In the year before the Promise's announcement, the district's poverty rate for school-aged youth was 28 percent, and African Americans and Hispanics made up 47 percent and 8 percent of district enrollment. For other school districts in the county, the poverty rate was only 8 percent, and African Americans and Hispanics were just 5 percent and 2 percent of enrollment.[2]

Figure 1 shows that prior to the Promise, enrollment in KPS had been declining. The Promise announcement in the fall of 2005 reversed that trend, and KPS enrollment has rebounded sharply. Bartik, Eberts, and Huang (2010) show that most of the subsequent increase stems from a reduced student exit rate after the Promise.

[2] 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.

Table 1 shows the number of KPS graduates from 2003 through 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

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.[3] 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 enhance Kalamazoo's economic development (Miller-Adams 2009), in part by attracting parents and businesses to the region as well as increasing the local supply and retention of college graduates.

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 2). 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.[4]

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

[4] 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).

The Promise pays up to 100 percent of tuition and fees at any public community college or university in Michigan.[5] The award is treated as first-dollar, meaning that it is applied *before* grant money from other sources.[6] The Promise benefit is graduated based on the length of continuous enrollment in the district's schools: the 100 percent subsidy is for students who have been in KPS since kindergarten; the rate is 95 percent for students enrolled since first through third grade; the rate is decreased by five percentage points for each subsequent grade through ninth (at a 65 percent scholarship), and no scholarship is available for students whose last continuous spell in KPS begins after the start of ninth grade.

Figure 3 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.[7] To maintain eligibility, enrolled students must keep a GPA of at least 2.0 per enrollment period. Students falling below this threshold can

[5] 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).

[6] 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.

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

regain eligibility by attending college for a semester without Promise support and raising their GPA above the cutoff.

Through the end of 2014, total scholarships paid by the Kalamazoo Promise reached $61 million, with an apparently 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.

DATA AND METHODOLOGY

Data

Our 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, and high school of graduation. The KPS records are more extensively discussed in Appendix A, titled "Data."

Most importantly for our identification strategy, the KPS records provide a history of student enrollment 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 continuous enrollment prior to the start of ninth grade—*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.[8] We are skeptical, however, that many students and their families would be overly sensitive to marginal changes in the percentage of tuition subsidized 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 from the Promise had the Promise been in effect, and zero if the student would have been ineligible. In the post-Promise period, we use administrative data from the Promise on eligibility.[9] 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.[10] 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 transfer behavior, and completions.[11]

[8] 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.

[9] For the post-Promise period, our calculations of Promise eligibility closely match Promise records. We use Promise records to minimize measurement error. Estimates using calculated eligibility for all students are not substantively different from the estimates presented in this paper.

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

[11] As documented by Dynarski, Hemelt, and Hyman (2013), 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.

Table 2A presents means for student characteristics and outcome variables, as determined by whether the student is observed in the pre-Promise or post-Promise period and whether the student is eligible or not for the Promise tuition subsidies.

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) $y_{it} = \alpha + \delta_1 \text{Elig}_{it} + \delta_2 \text{Post-Promise}_{it} + \delta_3 (\text{Post-Promise} \times \text{Elig})_{it} + \boldsymbol{\gamma_t} + \boldsymbol{x_{it}}\boldsymbol{\beta} + u_{it}$,

where *i* denotes the individual student and *t* denotes the academic year in which we observe the student. The outcome variable of interest is *y*; "Elig" equals one if, based on length of enrollment in KPS, the student is (would have been) eligible for a Promise tuition subsidy and zero otherwise; "Post-Promise" equals one if the student graduated after the Promise was in effect—the class of 2006 and later—and zero if before; and "Post-Promise × Elig" is an interaction between the two dummies. We also include a set of year dummies, $\boldsymbol{\gamma_t}$. The variable $\boldsymbol{x}$ is a vector of student-level observables, and *u* denotes student *i*'s unobservable traits. The coefficient of greatest interest is δ_3. This coefficient is a conservative estimate of Promise effects: differential outcomes for Promise-eligible students, compared both to ineligible students' outcomes and to pre-Promise outcomes.

The validity of a difference-in-differences strategy rests on two assumptions. The first assumption is that there is a common time trend in conditional outcomes between eligible and ineligible students before the Promise. In a hypothetical world without 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 "Post-Promise × Elig" interaction term would be zero. Unobservables would be uncorrelated with the interaction term after controlling for the ***x*** variables and the separate "Elig" and "Post-Promise" 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 group of ineligible students is systematically different before and after the Promise, 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 observable ***x*** variables, such as sex, race and ethnicity, participation in the federal subsidized lunch program, and high school of graduation. In addition to including these controls directly, we use them to construct inverse propensity score weights (IPW) that make post-Promise graduates resemble pre-Promise graduates in the *joint* distribution of observables (DiNardo, Fortin, and Lemieux 1996). By including these weights as well as the covariates directly, we implement what is sometimes called the augmented IPW (AIPW) estimator.[12]

To implement the reweighting, we first estimate via logit the propensity score, $p(x) = \Pr(\text{Pre-Promise} = 1|x)$, where "Pre-Promise" equals one if the student graduated before the

[12] In practice, our estimates are virtually identical if we use just the IPWs without the relevant covariates.

Promise, and is zero otherwise.[13] We assign a weight, $w(x) = \frac{\hat{p}(x)}{1-\hat{p}(x)}$, if pre-Promise = 0, and $w(x) = 1$ if pre-Promise = 1. We use the propensity score to reweight the students in the post-Promise period so that the distribution of covariates ***x*** of students in the post-Promise period resembles that of students in the pre-Promise period. We perform this reweighting procedure separately for eligible and ineligible students. These *w(x)* weights are then used to run weighted least squares difference-in-differences regressions. Table 2B shows how the distribution of covariates changes in the post-Promise period because of these weights.[14] The reweighting eliminates all significant differences in covariates between the pre-Promise and post-Promise periods.

It is still possible, however, that selection on unobservables remains, particularly later in the posttreatment period. After presenting the results, we will further examine threats to identification by looking at year-by-year trends in effects of Promise eligibility. We will also examine the effects of restricting the sample to exclude all students who entered KPS after the Promise announcement.

RESULTS FOR POSTSECONDARY OUTCOMES

We consider Promise effects on postsecondary outcomes in the short term, medium term, and long term.

[13]Our propensity score reweighting fully saturates the logit model with the various discrete variables controlling for gender, race, and free or reduced-price lunch status. In contrast, the control–variable approach includes each discrete variable without the interaction terms, as in previous studies.

[14] We have confirmed that there is common support among the discrete demographic cells used for reweighting, with the exception of a handful of observations that are omitted from the reweighted regressions. A histogram of the propensity scores is available upon request.

Short-Term Outcomes

Table 3 presents results for enrollment outcomes. The four panels examine enrollment at any postsecondary institution, or at a four-year school, within either 6 months or 12 months of high school graduation. For each outcome, the left column reports results for the specification with demographic controls, and the right column reports results for the specification with demographic controls and IPW reweighting. 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 tuition subsidies.

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 about 8 percentage points. The estimates are substantively quite similar regardless of whether IPW reweighting is used, although statistical significance weakens very slightly under reweighting. The percentage-point increase is large relative to the mean enrollment probability of 0.602 among eligibles in the pre-Promise period, representing an increase of 14 percent (0.084/0.602). However, when the enrollment horizon is extended to 12 months, the enrollment effects are smaller and not statistically significant. This finding suggests that the Promise may operate in part by accelerating the time to first enrollment.

Enrollment effects of the Promise are greater at four-year colleges. At either a 6-month or 12-month horizon, our estimates suggest that the Promise increases four-year college enrollment by 9 to 13 percentage points. These greater four-year enrollment effects compared to overall

enrollment effects suggest 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.[15] Because the base enrollment at four-year colleges is lower, the implied percentage effect is 23 to 34 percent (0.095/0.405 to 0.133/0.397). The similarity of the enrollment horizon results for four-year colleges suggests that the all-college enrollment horizon differences in enrollment timing are concentrated in the two-year sector, which is plausible.[16]

Previous research shows that aid can affect not just whether a student attends college, but which college she attends.[17] We explore this margin of response in Table 4, 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 huge amount: 18 percentage points. This represents an increase over the pre-Promise base of almost 40 percent (0.178/0.472 = 38 percent; 0.184/0.472 = 39 percent), 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 over 60 percent (0.175/0.277 base = 63 percent; 0.207/0.277 = 75 percent).

The third panel shows that the gains at Promise-eligible schools are partially due to losses at ineligible institutions. Such attendance declined by 10 percentage points, or almost 80 percent.

[15] These conclusions are only suggestive because the four-year enrollment effects are not statistically different from the all-college effects. If we examine the effects at two-year colleges directly, we find a small, negative point estimate not statistically different from zero, in line with the difference in the estimates between the all-college effects and the four-year college effects. The small point estimate for two-years likely masks offsetting effects, as some students upgrade to 4-years and others are induced to attend college at the extensive margin.

[16] In our data, about 97 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 80 percent of students who enroll in a two-year college within 12 months of high school graduation do so within the first 6 months.

[17] An especially salient example of how aid affects college choice is provided by Cohodes and Goodman (2014) and the references therein.

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 4, the drop in attendance at noneligible schools is driven by out-of-state schools, not private schools in Michigan. This substitution effect is consistent with the Promise donors' desire to encourage Michigan college attendance.[18]

Medium-Term Outcomes

These positive enrollment effects align with previous studies, but do these effects 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.[19] 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-attempted variable to be highly positively correlated with credits actually earned and with eventual degree attainment.

Compared to enrollment outcomes in Tables 3 and 4, 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.

[18]The large increases at eligible Michigan four-year institutions are driven by Western Michigan University, local to Kalamazoo, and Michigan State University, about 80 miles away.

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

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 between 13 percent and 15 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, increases in statistical significance. As of three years after high school, the Promise has led students to enroll in 4.6 to 5.7 additional credits (between 13 percent and 16 percent above baseline). As of four years after high school, the effect on credits attempted ranges from 6.9 to 9.3 (15 to 20 percent), depending on whether IPWs are used. Put differently, after the equivalent of eight semesters, treated students took an additional two to three classes, which is a sizable gain.

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. In particular, we find that among KPS graduates who enroll at all, 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 10–12 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) actually appear to take more classes, while other students (those who enroll at two-year colleges) appear to take fewer classes.[20]

Longer-Term Outcomes

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 and others none.

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.

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 5 percentage points (relative to a baseline of 14 to 18 percent) cannot be ruled out. But given that the median duration to a bachelor's degree is well over four years (Bound, Lovenheim, and Turner 2010; Cataldi et al. 2011), and that full funding from the Promise is

[20] 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 Kalamazoo Valley Community College.)

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.

Over a six-year horizon, point estimates are positive and large. The Promise effect on attainment of any credential as of six years is between 9 and 12 percentage points. The confidence intervals imply a wider range of possible effects. On the high side, we cannot rule out a 20-percentage-point increase. On the low side, the 9-percentage-point effect is statistically different from zero at a 6 percent significance level, and the 12-percentage-point effect is significantly different from zero at a 2 percent significance level. Still, these point estimates 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 25–34 percent (0.091 or 0.123 divided by 0.357).

For the attainment of a bachelor's degree (Panel D), we again find sizable point estimates, although statistical significance is slightly weaker. The 7–10 percentage-point increase translates to a percentage boost between 23 and 33 percent in the likelihood of earning a bachelor's degree (0.067 or 0.097 divided by 0.296). The bachelor's degree results suggest that most of the Promise effect on degree attainment comes from increasing bachelor's attainment.[21]

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 and ethnicity.

[21] 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.

Threats to Identification

Our identifying assumption is that the Promise-eligible groups and Promise-ineligible groups do not have trends in unobservable variables 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 migration (Hershbein 2013)—that led to relatively more favorable outcomes for Promise-eligible students vis-à-vis ineligible students.

One approach to addressing these identification threats is to 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).

Therefore, in Figure 4 we present fitted probabilities of enrollment in a four-year school within six months of high school graduation (Table 3, Panel C, column 1), allowing full interactions between class year and eligibility status. If these fitted probabilities 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 5 and 6 similarly present results allowing for the full interaction of Promise eligibility and cohort for the predicted number of credits attempted within two years of high school graduation (Figure 5) and for the fitted probability of attaining a bachelor's degree within six years of high school graduation. The patterns are in accord with Figure 4: there is a jump for 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.

A second approach to dealing with threats to identification, particularly dynamic selection effects, is to check whether the estimates are robust to excluding new entrants to 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 near the end of the sample horizon. Therefore, the reduction in the possibility of selection bias by restricting the sample to students who were in KPS prior to the Promise's announcement comes at the expense of estimate precision.

We have redone selected estimates using this smaller sample that includes only students who had entered KPS before the Promise was announced. The selected estimates were for the six-month enrollment outcome, the six-month enrollment in four-year college outcome, the six-year all-credential attainment estimates, and the six-year bachelor of arts/bachelor of science (BA/BS) credential attainment. In most cases, the resulting point estimates from excluding late-

entrant students are not close to being substantively or statistically different from the estimates in Tables 4 and 6 that include the late-entrant students.[22]

The one exception is for enrollment at a four-year college at six months after high school graduation. Without new entrants, the point estimate shrinks, and the difference in estimates is statistically significant at a 7 percent level.

This finding has at least three alternative interpretations. The first interpretation is that dropping new entrants reduces selection bias, and therefore we should view these estimates as more reliable, which suggests lower effects of the Promise on attendance at four-year colleges.

A second interpretation is that dropping new entrants may bias the results 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, who are more likely to go to a four-year college.[23] Because of excluding late entrants, the class of 2008 cannot include any student who entered KPS in eleventh or twelfth grade, 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 cannot control for fixed unobservables.

A third interpretation is that the effects of the Promise on four-year college attendance might be increasing over time. The estimates without new entrants necessarily exclude

[22] The *p*-values for tests of equality between the paired sets of estimates are: 0.33 for the non-IPW six-month any enrollment, 0.40 for the IPW six-month any enrollment, 0.33 for the non-IPW six-year any credential, 0.55 for the IPW six-year any credential, 0.59 for the non-IPW six-year BA/BS, and 0.99 for the IPW six-year BA/BS. Relative to estimates that include the late-entrant students, the point estimates increase slightly for the six-month any enrollment, decrease slightly for the six-year any credential, and are almost the same for the six-year BA/BS.

[23] 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).

information from later cohorts, as these cohorts lack ineligible students in the estimation sample. Bartik and Lachowska (2013) find evidence that the Promise had increasing effects over time on improving student behavior, and on increasing African American students' GPA. Estimates restricted to earlier cohorts might miss these increasing effects, and, because of the role of covariates, the individual-year estimates shown in Figures 4 through 6 might not fully capture them either.

Overall, we think these robustness checks support the validity of our identification assumptions.

HETEROGENEITY

An important question is the extent to which the 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 estimates imply that the Promise does *not* simply reflect an income transfer to inframarginal 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 7 reports selected results for how Promise effects vary with family income (proxied by free or reduced-price lunch status), race, and gender. For conciseness, we focus on Promise effects for three outcomes: four-year college attendance within six months, credits attempted at two years, and six-year attainment of a BA/BS.

As shown in the first panel, the point estimates for 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. 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 more than 50 percent (about twice the percentage increase for higher-income students), credits attempted within two years jumps 23 percent (17 percent for higher-income students), and bachelor's completion within six years rises (imprecisely) by 57 percent (22 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 2A). The estimates on four-year college enrollment are nearly identical across the racial groups, at 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 6.2 credits, or about two classes; remarkably, this represents a 34 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 three times as large as it is for white students, and, proportionally, approaches an effect size of 50 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 (45 percent), while men seem to experience zero benefit.

DISCUSSION AND CONCLUSION

This paper's estimates suggest that the Kalamazoo Promise has large effects on postsecondary outcomes. This conclusion seems reasonable in part because the estimates are consistent: they show sizable percentage effects on postsecondary attendance, credits, and attainment. The estimates also have reasonable patterns across outcomes: for example, the substitution of Promise-eligible colleges for ineligible colleges makes sense, as does the substitution of four-year colleges for two-year colleges. These findings echo the patterns in the earlier analysis of ACT score-sending of Andrews, DesJardins, and Ranchhod (2010).

The pattern of Promise effects across students is also quite similar to Angrist et al. (2015), who study the effect of college scholarships—covering full tuition and fees—that were conditionally 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.

We emphasize that the identification strategy we have used in this paper provides a conservative estimate of Promise effects. Our strategy, by its design, cannot capture Promise effects that are school-wide or community-wide. 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. All of these KPS and community efforts, which are more or less tied to the Promise as a unifying theme, may affect the college enrollment and success of all KPS students, both Promise-eligible and -ineligible. Peer effects from Promise-eligible students might also affect Promise-ineligible students. Our comparison of different time trends for Promise-eligible versus -ineligible students cannot capture these district-wide and community-wide effects. What we can capture are the specific effects of being eligible for the Promise's tuition subsidies, rather than the many broader Promise effects on school and community climate.

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 earnings. A back-of-the-envelope calculation drawing on our degree completion estimates (detailed in Appendix B) shows that the present value of increased career earnings exceeds the costs of Promise tuition subsidies at all real discount rates of up to 11.3 percent. 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, this conclusion could likely apply to other urban school districts considering setting up their own Promise-like scholarships—at least to the extent that they closely follow the Kalamazoo model in terms of universality and generosity.

On the other hand, the Promise effects have 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 36 percent to 48 percent. Presumably some of the remaining 52 percent might benefit from receipt of a postsecondary educational credential. As one might expect, "free" college is insufficient by itself to ensure higher skill levels through postsecondary education. Other policies prior to age 18 are likely needed to improve outcomes for more students. However, simple and generous scholarship programs have the potential of being a cost-effective component of the policy toolbox to increase the educational attainment of American students.

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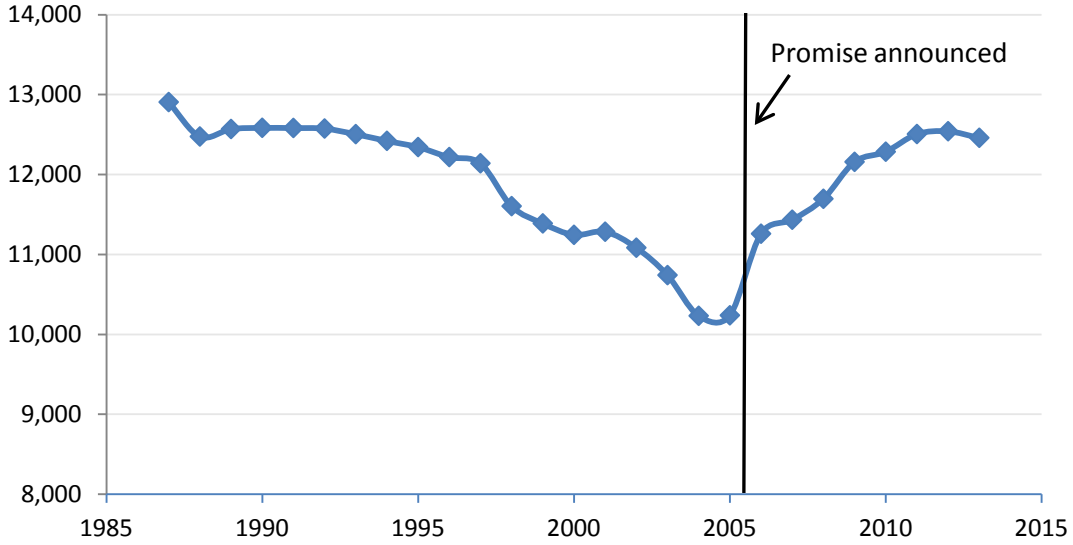
Figure 1 KPS Enrollment, 1987–2013



NOTE: Enrollment is as of the official fall count day.
SOURCE: Michigan Center for Educational Performance and Information (CEPI).

Figure 2 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)

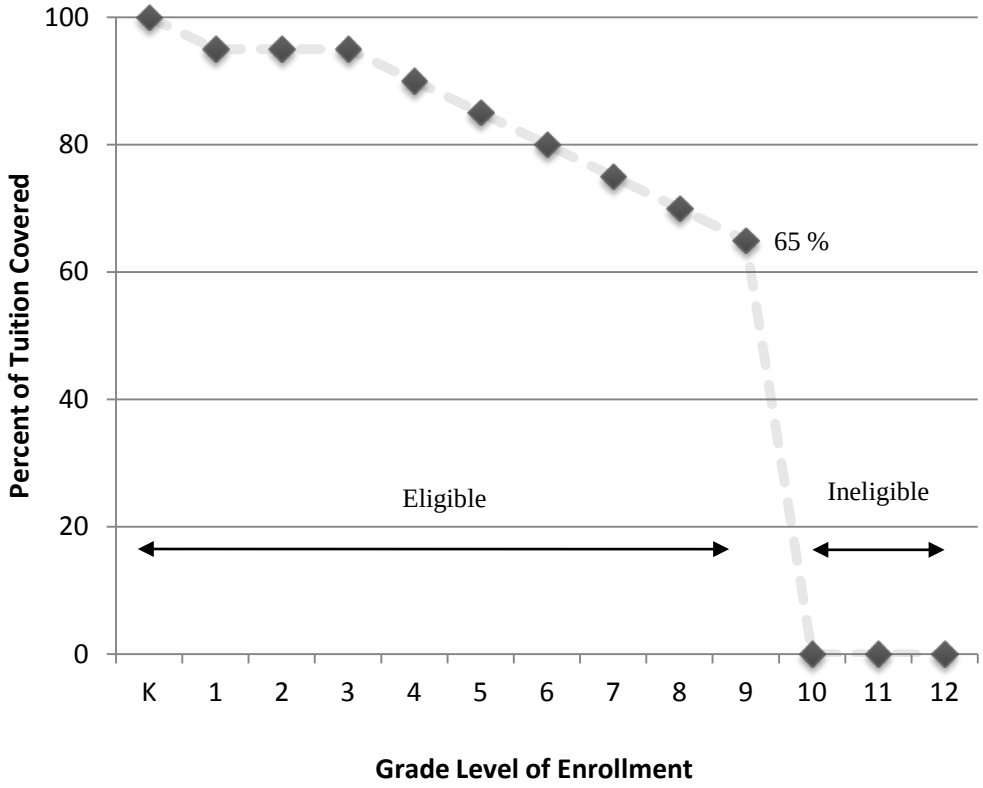
School 1: _____

School 2: _____

School 3: _____

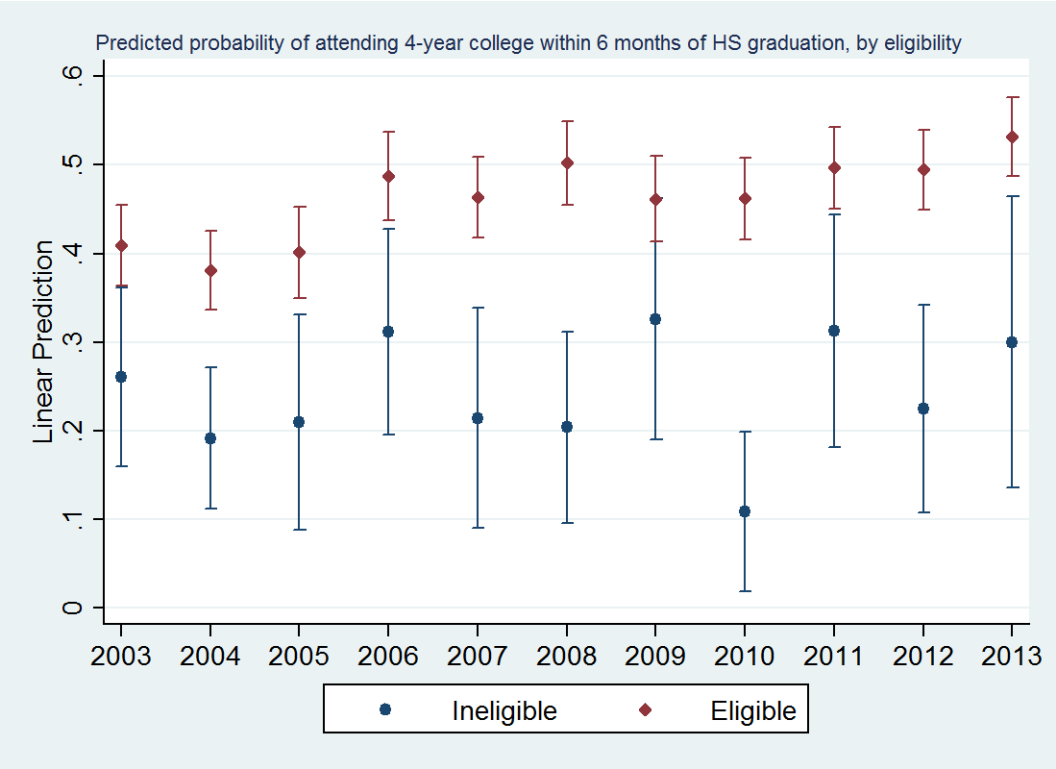
SOURCE: The Kalamazoo Promise.

Figure 3 Generosity of the Kalamazoo Promise Scholarship, by Grade of Initial Enrollment



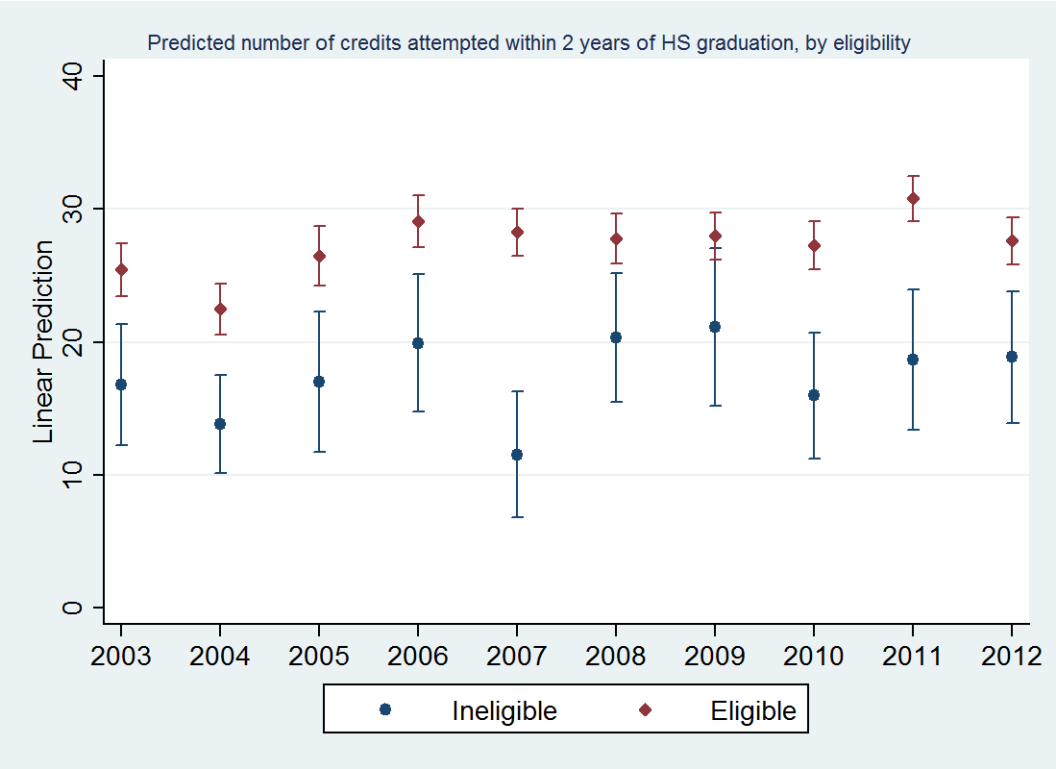
SOURCE: Eligibility rules from the Kalamazoo Promise.

Figure 4 Fitted Probabilities of Four-Year College Attendance, by Year and Promise Eligibility



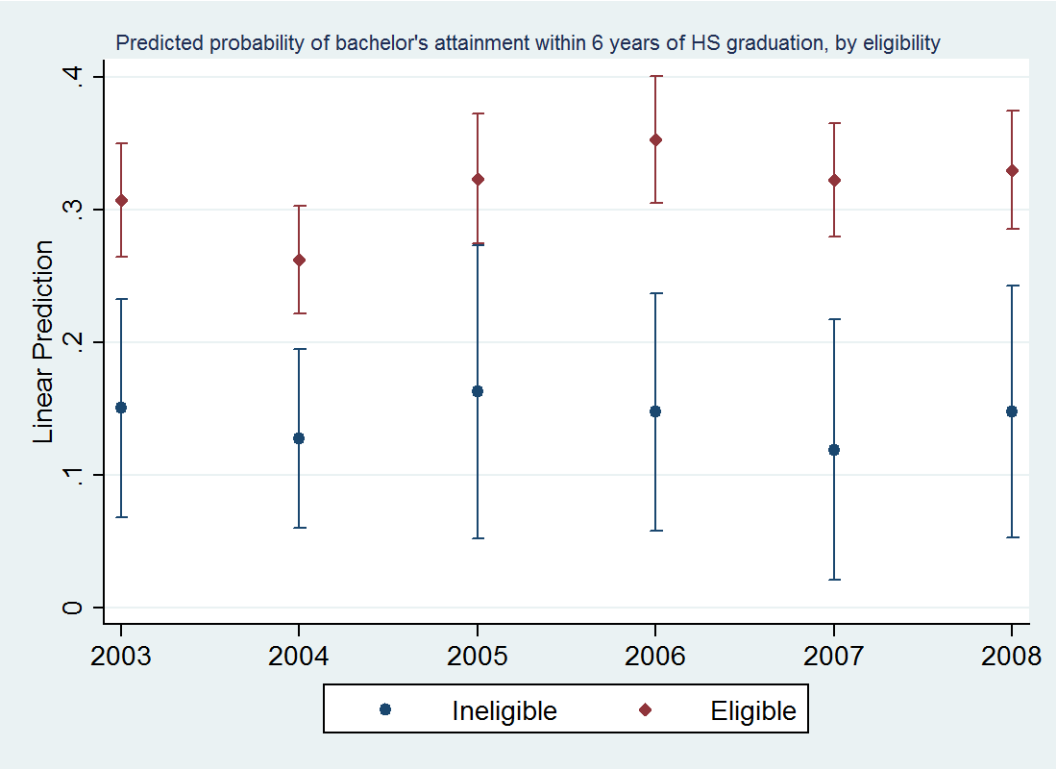
NOTE: The plotted values represent fitted probabilities of attending a four-year college within six months of high school graduation, by class year and Promise eligibility, taken from the underlying specification in column 1 of Table 3 but allowing the Promise effects to vary by year. The plots control for demographic characteristics; see the notes to Table 3. Point-wise 95-percent confidence intervals shown.

Figure 5 Predicted Number of Credits Attempted, by Year and Promise Eligibility



NOTE: The plotted values represent predicted number of credits attempted within two years of high school graduation, by class year and Promise eligibility, taken from the underlying specification in column 1 of Table 5 but allowing the Promise effects to vary by year. The plots control for demographic characteristics; see the notes to Table 5. Point-wise 95-percent confidence intervals shown.

Figure 6 Fitted Probabilities of BA/BS Attainment, by Year and Promise Eligibility



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, taken from the underlying specification in column 1 of Table 6 but allowing the Promise effects to vary by year. The plots control for demographic characteristics; see the notes to Table 6. Point-wise 95-percent confidence intervals shown.

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

Year	Graduates	Eligible	Ineligible
2003	525	0 (452)	525 (73)
2004	551	0 (457)	551 (94)
2005	392	0 (349)	392 (43)
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	4,829	586

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 2A Descriptive Statistics of Sample

Variable	All	Eligibles	Ineligibles	Before	After
Demographics					
Male	0.471	0.475	0.442	0.465	0.473
Black	0.411	0.399	0.507	0.367	0.427
Asian	0.026	0.024	0.046	0.023	0.027
Hispanic	0.070	0.069	0.078	0.055	0.076
White	0.485	0.500	0.358	0.550	0.460
Subsidized lunch	0.505	0.494	0.597	0.364	0.558
High school 1	0.513	0.515	0.497	0.475	0.527
High school 2	0.403	0.408	0.360	0.435	0.391
N	5,415	4,829	586	1,468	3,947

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 the cohorts 2006 through 2013.
SOURCE: Authors' calculations from KPS and Kalamazoo Promise administrative data.

Table 2B Descriptive Statistics, Reweighted Sample

	No reweighting				Reweighting			
Variable	Before	After	Diff	*p*-val	Before	After	Diff	*p*-val
Demographics								
Male	0.465	0.473	0.008	0.60	0.465	0.467	0.002	0.90
Black	0.367	0.427	0.060	0.00	0.367	0.369	0.002	0.88
Asian	0.023	0.027	0.004	0.40	0.023	0.021	−0.003	0.60
Hispanic	0.055	0.076	0.020	0.01	0.055	0.054	−0.001	0.89
White	0.550	0.460	−0.089	0.00	0.550	0.551	0.001	0.93
Subsid. lunch	0.370	0.558	0.188	0.00	0.370	0.371	0.001	0.95
High school 1	0.476	0.527	0.051	0.00	0.476	0.478	0.001	0.93
High school 2	0.435	0.391	−0.044	0.00	0.435	0.437	0.002	0.89
N	1,468	3,947			1,468	3,947		

NOTE: See Table 2A. See text for details of reweighting procedure.
SOURCE: Authors' calculations from KPS and Kalamazoo Promise administrative data.

Table 3 Promise Effects on Enrollment

	(1)	(2)
Panel A: Enrollment within 6 months		
(Mean of DV | after=0, elig.=1) = 0.602		
After × Eligible	0.084**	0.085*
	(0.042)	(0.047)
R^2	0.142	0.172
Panel B: Enrollment within 12 months		
(Mean of DV | after=0, elig.=1) = 0.662		
After × Eligible	0.053	0.054
	(0.042)	(0.046)
R^2	0.150	0.184
Panel C: Enrollment at 4-yr. within 6 months		
(Mean of DV | after=0, elig.=1) = 0.397		
After × Eligible	0.099**	0.133***
	(0.039)	(0.041)
R^2	0.183	0.189
Panel D: Enrollment at 4-yr. within 12 months		
(Mean of DV | after=0, elig.=1) = 0.405		
After × Eligible	0.095**	0.129***
	(0.040)	(0.043)
R^2	0.186	0.193
Use IPW?	N	Y

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, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation. The mean of the dependent variable is for eligible population in the pre-Promise period. Sample size is 5,415 with demographic controls, 5,377 with IPW reweighting; the smaller IPW sample reflects lack of matches in the fully saturated propensity score model.

Table 4 Promise Effects on Enrollment by Type of School (Promise vs. non-Promise school)

	(1)	(2)
Panel A: Enroll at a Promise school within 6 months (Mean of DV | after=0, elig.=1) = 0.472		
After × Eligible	0.184*** (0.043)	0.178*** (0.047)
R^2	0.133	0.150
Panel B: Enroll at a 4-yr. Promise school within 6 months (Mean of DV | after=0, elig.=1) = 0.277		
After × Eligible	0.175*** (0.036)	0.207*** (0.038)
R^2	0.159	0.167
Panel C: Enroll at a 4-yr. non-Promise school within 6 months (Mean of DV | after=0, elig.=1) = 0.130		
After × Eligible	−0.099*** (0.023)	−0.093*** (0.024)
R^2	0.039	0.039
Use IPW?	N	Y

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, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation. The mean of the dependent variable is for eligible population in the pre-Promise period. Sample sizes are the same as in Table 3 (5,415 with demographic controls, 5,377 with IPW reweighting, with the smaller IPW sample reflecting lack of matches). The sample is the total of students. The dependent variable in Table 4, Panel A is equal to 1 if a student enrolls at any Promise-eligible school and 0 otherwise. Analogous definitions hold for panels B and C.

Table 5 Promise Effects on Credits Attempted

	(1)	(2)
Panel A: Credits attempted at 2 years		
(Mean of DV | after=0, elig.=1) = 24.66		
After × Eligible	3.17*	3.59*
	(1.67)	(1.83)
R^2	0.200	0.211
N	4,902	4,870
Panel B: Credits attempted at 3 years		
(Mean of DV | after=0, elig.=1) = 35.59		
After × Eligible	4.56*	5.68**
	(2.51)	(2.69)
R^2	0.213	0.222
N	4,376	4,352
Panel C: Credits attempted at 4 years		
(Mean of DV | after=0, elig.=1) = 45.93		
After × Eligible	6.87**	9.27**
	(3.41)	(3.58)
R^2	0.208	0.214
N	3,869	3,851
Use IPW?	N	Y

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, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation. The mean of the dependent variable is for eligible population in the pre-Promise period. Smaller IPW sample reflects lack of matches in the fully saturated propensity score model.

Table 6 Promise Effects on Degree Attainment

	(1)	(2)
Panel A: Any credential at 4 years (Mean of DV | after=0, elig.=1) = 0.184		
After × Eligible	0.004	0.019
	(0.032)	(0.035)
R^2	0.085	0.075
Panel B: Any credential at 6 years (Mean of DV | after=0, elig.=1) = 0.357		
After × Eligible	0.091*	0.123**
	(0.047)	(0.050)
R^2	0.145	0.132
Panel C: BA/BS at 4 years (Mean of DV | after=0, elig.=1) = 0.141		
After × Eligible	0.004	0.016
	(0.024)	(0.028)
R^2	0.113	0.098
Panel D: BA/BS at 6 years (Mean of DV | after=0, elig.= 1) = 0.296		
After × Eligible	0.067*	0.097**
	(0.041)	(0.045)
R^2	0.178	0.159
Use IPW?	N	Y

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, and graduation year. Other controls are sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation. The mean of the dependent variable is for eligible population in the pre-Promise period. Sample sizes are 3,869 (3,851) at four years for non-IPW (IPW) sample, and 2,905 (2,896) at six years for non-IPW (IPW) sample. Smaller IPW sample reflects lack of matches in the fully saturated propensity score model.

Table 7 Promise Effects by Group

	6-month attendance at 4-year		2-year credits attempted		6-year BA/BS attainment	
Income groups	Non-low income	Low-income	Non-low income	Low-income	Non-low income	Low-income
After × Elig	0.137**	0.108**	4.92*	3.62*	0.089	0.059
	(0.066)	(0.048)	(2.68)	(2.19)	(0.076)	(0.043)
N	2,666	2,744	2,457	2,440	1,641	1,259
p-val of group diff		0.718		0.705		0.723
Mean DV	0.506	0.191	29.45	15.70	0.398	0.104
Race	White	Non-white	White	Non-white	White	Non-white
After × Elig	0.103	0.104**	−0.67	6.21**	0.026	0.073
	(0.067)	(0.048)	(2.77)	(2.12)	(0.069)	(0.048)
N	2,624	2,791	2,410	2,492	1,545	1,360
p-val of group diff		0.990		0.048		0.570
Mean DV	0.505	0.250	29.45	18.13	0.397	0.158
Gender	Male	Female	Male	Female	Male	Female
After × Elig	0.084	0.121**	1.31	5.08**	−0.003	0.133**
	(0.058)	(0.053)	(2.35)	(2.34)	(0.055)	(0.059)
N	2,551	2,864	2,293	2,609	1,388	1,517
p-val of group diff		0.637		0.254		0.088
Mean DV	0.396	0.398	25.38	24.02	0.294	0.297

NOTE: All estimates calculated without IPW weights; results using IPW are similar and available on request. 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 (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.

Appendix A

Data

This appendix gives more information on our data, estimation sample, and variables.

1. DESCRIPTION OF KPS ADMINISTRATIVE RECORDS AND VARIABLE DEFINITIONS

Our main data is derived from administrative records of KPS students from the graduating classes of 2003 through 2013. From these data, we define graduates based on entry and exit codes provided by KPS.

From the KPS records, we obtain information about high school of graduation and student demographics, including sex, race/ethnicity (Native American, Black, Asian, Hispanic, or White), and participation in the federal assisted lunch program (a binary yes/no variable). We also obtain data for high school of graduation and academic variables (cumulative GPA, highest math class taken, the number of AP classes taken, and whether the student attended the Kalamazoo Area Mathematics and Science Center [KAMSC]). Except for lunch status, we observe each of these variables for every high school graduate. We do not observe lunch status for the class of 2003; instead, we impute this variable as described in section 1.2, below.[24]

1.1 Definition of Kalamazoo Promise Eligibility

We use the KPS student entry and withdrawal records to construct our key explanatory variable, the indicator for whether a high school graduate is eligible for the Kalamazoo Promise. The KPS school tenure records go back to the 1996–1997 school year, which allows us to track

[24] As they are endogenous (Bartik and Lachowska 2013), the academic variables are used only for this lunch status imputation and not in the main analysis.

continuous enrollment histories for most students, although earlier cohorts have truncated histories. For example, for the high school class of 2003, we observe records back to sixth grade (if the student was on time), but not earlier.

For the graduates from the classes of 2003 through 2005—the pre-Promise period—we define an eligibility variable that equals one if the student is eligible for any tuition subsidy (65 percent or more) had the Kalamazoo Promise been in effect at that time, and zero if the student is not eligible for any subsidies. We define a student as eligible if he or she had been continuously enrolled in KPS from before the fall count date in ninth grade. If the student enrolled after the ninth-grade fall count date, we count that student as ineligible in accord with Promise rules. In the post-Promise period (2006–2013), we use administrative records directly from the Kalamazoo Promise. We made this decision in part because the student entry and withdrawal records available to us end in 2010–2011, which means we lack full records for 2012 and 2013 graduates on continuous enrollment. Comparing our calculated Promise eligibility to actual Promise eligibility in the overlapping period shows a close match (Appendix Table A1).

Appendix Table A1: Eligibility Comparison between Administrative Data and Assignment Algorithm

Class year	Eligible by administrative data	Eligible by algorithm	Exact match
2006	388	388	376
2007	462	455	449
2008	430	426	420
2009	420	413	408
2010	452	451	445
2011	459	463	449

1.2 Free or Reduced Lunch Program Participation in 2003

Because the KPS data do not record whether students participated in the federal assisted lunch program in 2003, we impute this variable using data from the other two pre-Promise years, 2004 and 2005. Using data for these two years, we predict the probability that a student is on the federal assisted lunch program by logistic regression of the lunch status dummy on a fully

saturated (i.e., all possible interactions) vector of controls: gender, race/ethnicity, high school indicator, and a KAMSC dummy. The regression also includes achievement variables: cumulative school-year-level GPA as a cubic spline, cumulative AP classes taken, and dummies for the highest math class taken.

We define a student as participating in the lunch program using the following two-step procedure. First, using a random-number generator, we create a variable that is uniformly distributed from 0 to 1. Second, we assign a student to the lunch program if the predicted value of a student's lunch-program participation probability exceeds his or her corresponding random-number value.

2. DESCRIPTION OF NSC DATA

The National Student Clearinghouse (NSC) is a nonprofit organization that tracks student enrollment at nearly all postsecondary institutions at which students can receive federal financial aid. Through the StudentTracker service, school districts can submit student names and birth dates, and NSC will match with their database and return postsecondary enrollment records. We obtained StudentTracker data from KPS covering the high school graduating classes of 2003 through 2013 and the enrollment periods of Fall 2003 through Spring 2014.

The data provide the college attended for each term's enrollment. They also record the intensity of enrollment (full-time, half-time, less than half-time, and whether the student withdrew). We also observe whether a credential was received, the type of credential, and the date of receipt. Together these data are used to construct our outcome variables.

We are able to match more than 97 percent of the KPS graduates in our data to NSC records (Appendix Table A2).[25] Of the unmatched, nearly half are from 2003: for this year, NSC reports no graduates from KPS's alternative high school. Our estimates control for high school of graduation, so this exclusion is not problematic.

Appendix Table A2: Final Match Rates

Class year	Final Matched Estimation Sample	All KPS grads
2003	525	585
2004	551	552
2005	392	393
2006	449	457
2007	504	522
2008	484	501
2009	466	467
2010	498	500
2011	507	522
2012	526	531
2013	513	519
Total	5,415	5,549

NOTE: All KPS grads refer to graduates earning a regular high school diploma.

Although the match rate is high, the NSC has shortcomings. As detailed by Dynarski, Hemelt, and Hyman (2013), NSC data do not cover all colleges, and some records are blocked because of student or school requests under the Family Educational Rights and Privacy Act (FERPA). They show that coverage ranged from about 83 percent of students in 2003 to 90 percent in 2011. For Michigan colleges, coverage was slightly lower than for the nation in 2003 and slightly larger in 2011. For-profit institutions have lower coverage than other institution types.

The most relevant coverage issue for this paper is for Kalamazoo Valley Community College (KVCC), the local public two-year school, which approximately one-third of KPS graduates attend. KVCC does not provide student records to NSC before 2005. As a substitute, we obtained equivalent data for KPS graduates for the Summer 2003 through Summer 2005

[25] Discrepancies between the two columns are primarily, but not exclusively, due to failed NSC matches. A very few additional graduates lacked core data, such as graduation date.

period directly from KVCC upon special request, in cooperation with KPS and the Kalamazoo Promise. There are other schools for which NSC coverage began during our sample period, but none are (or were) attended in large numbers by KPS graduates.[26]

2.1 Construction of Outcome Variables

The NSC data contain the dates enrollment begins and ends for each college attended. We combine these data with dates of high school graduation to determine whether and what type of postsecondary institution was attended within different time frames of high school graduation. We do not count college enrollment that began before high school graduation (i.e., dual enrollment).

The NSC data do not contain the number of credits attempted or earned, but they do contain a measure of enrollment intensity: full-time, half-time, less than half-time, or withdrawn. For institutions on a semester system, we assign 12 credits, 6 credits, 3 credits, and 0 credits attempted to each of the categories, respectively. For institutions on a trimester or quarter system, we assign credits per term that are proportionally adjusted to accord with the semester system over a standard academic year. We determine timing based on enrollment end dates and high school graduation dates, as above. The credit assignments are approximations, but as long as actual credits attempted to do not differentially vary by eligibility over time, our estimates should not be biased. The NSC data also provide the type and date of degrees or credentials earned, separately from enrollment.

[26] Within Michigan, coverage for Wayne State University (four-year public) and Washtenaw Community College (two-year public) began in 2004, Michigan Tech University (four-year public) began in 2008, Baker College of Flint and Davenport University (both four-year private nonprofit) began in 2009, and Everest Institute of Kalamazoo (four-year for-profit) began in 2011. Outside of Michigan, coverage for the University of Phoenix began in 2006.

Appendix B

Benefits and Costs of the Promise

In order to conduct a partial cost and benefit analysis, we need to translate our estimates of Promise effects on postsecondary outcomes into dollars. To do so, we predict the effects of educational credentials on future earnings.

This benefit calculation is partial in that it accounts only for the earnings benefits of individual students' Promise eligibility. This may understate total returns by not capturing the nonpecuniary returns to individuals (Oreopoulos and Salvanes 2011) or community-wide gains; for example, we do not account for the economic benefits of attracting parents and businesses to the community.

We compare these earnings benefits with the tuition subsidy costs of the Promise to the donors. A more complete cost analysis would also account for the transfer value of the Promise to students and their families and any psychic or private costs of increased college attendance.

Overall, our analysis probably understates benefits relative to costs. But these estimates may interest communities exploring their own Promise programs.

1. CALCULATING THE PRESENT VALUE OF A COLLEGE DEGREE

In order to compute the present value of a college degree, we simulate future earnings of KPS students. To do so, we use microdata from the 2012 American Community Survey (ACS) and apply the following procedure:

- First, using the 2012 ACS, we compute average unconditional earnings by age level from 25 to 79 (we start with age 25 to allow completion of educational attainment) and by three

educational attainment categories: 1) high school graduates with a high school diploma but without any postsecondary credential, 2) people with an associate's degree, and 3) people with at least a bachelor's degree.

- Second, we adjust for future earnings increases (i.e., since 2012) by assuming that future real wage growth is equal to 1.2 percent, in accordance with the assumptions of the trustees of the Social Security Administration (Office of the Chief Actuary 2012). At each age level, we adjust future earnings by the unisex probability of survival, conditional on being alive at 18 years of age.
- Third, we discount the mortality-adjusted future earnings back to age 18. The baseline real discount rate is 3 percent. We compute two net present values: 1) the net present value of an associate's degree relative to a high-school diploma (we call this net PV_{AA}) and 2) the net present value of at least a bachelor's degree relative to a high-school diploma (we call this net PV_{BA}). We also consider 5 percent as a real discount rate, and we consider the "rate of return" to the Promise's investment, which is the real discount rate that equates these future Promise earnings benefits to the Promise's tuition subsidy costs.
- Fourth, we use our preferred estimate of the causal effect of the Promise on two outcomes: 1) obtaining an associate's degree (a 1.6-percentage-point increase)[27]; and 2) obtaining a BA/BS degree (a 9.7-percentage-point increase; see Table 6, Panel D), and then we multiply net PV_{AA} and net PV_{BA} each by the relevant point estimate.

[27] Derived from Table 6, Panel B and Panel D, which estimate Promise effects on any credential and on a bachelor's degree, respectively. The difference is the Promise effect on earning a non-bachelor's credential. We multiply this difference by the observed share of our nonbaccalureate-degree-earning sample with an associate's degree. This product yields an estimate that the Promise increases associate degrees by 1.6 percentage points.

2. CALCULATING THE PRESENT VALUE OF TUITION SUBSIDY COSTS

We calculate the present value of the Promise tuition costs using available actual cost data from the Kalamazoo Promise for every eligible student in our analysis sample. The cost data are deflated to 2012-constant prices using the PCE deflator from the U.S. Bureau of Economic Analysis, and we have assumed that tuition payments are made three times a year (in June, September, and January). All of the cost data are discounted back to the June of a KPS student's senior high-school year.

The effects of the Promise on obtaining any college degree are estimated using the first three cohorts (cohorts that graduated in 2006, 2007, and 2008). Because of data limitations, our cost data are complete only for the 2006 and 2007 cohorts. We calculate present values of costs on a per-eligible student basis and see no significant changes between the 2006 and 2007 cohorts, and we would expect no large differences in per-student figures for the 2008 cohort.

3. A PARTIAL COST-BENEFIT ANALYSIS

Our partial cost-benefit analysis suggests that, when using a real discount rate of 3 percent, the ratio of benefits to costs is about 4.7. Using a 5 percent discount rate, we obtain that the ratio of benefits to costs is about 3.0. It is also interesting to calculate the Promise's internal rate of return—that is, the real discount rate at which benefits equal costs. Our calculations show that this rate equals 11.3 percent, which by any standard is a high real rate of return.

The estimates suggest that the Kalamazoo Promise has economic benefits that exceed program costs. A more complete analysis of the economic benefits of the Promise would most likely make this conclusion even stronger.