The Short-Term Effects of the Kalamazoo Promise Scholarship on Student Outcomes

Timothy J. Bartik and Marta Lachowska

April 2012

Abstract: In order to study whether college scholarships can be an effective tool in raising students’ performance in secondary school, we use one aspect of the Kalamazoo Promise that resembles a quasi-experiment. The surprise announcement of the scholarship created a large change in expected college tuition costs that varied across different groups of students based on past enrollment decisions. This variation is arguably exogenous to unobserved student characteristics. We estimate the effects of this change by a set of “difference-in-differences” regressions where we compare the change in student outcomes in secondary school across time for different student “length of enrollment” groups. We find positive effects of the Kalamazoo Promise on Promise-eligible students large enough to be deemed important—about a 9 percent increase in the probability of earning any credits and one less suspension day per year. We also find large increases in GPA among African American students.

Keywords: academic output, educational incentives, universal scholarship, natural experiment

JEL Codes: I21, I22

1 We thank Susan Houseman, Lars Lefgren, and the participants of PromiseNet and AEFP for valuable suggestions. We have benefited from our discussions with Michael Rice. We thank Ben Jones for his editorial assistance. Wei-Jang Huang has provided outstanding research assistance. All errors are our own.

2 Upjohn Institute. E-mail: bartik@upjohn.org

3 Upjohn Institute and Stockholm University. E-mail: lachowska@upjohn.org
1 Introduction

The Kalamazoo Promise provides an unusual model for revitalizing an urban school district and its community. Announced on November 10, 2005, the Kalamazoo Promise provides large college scholarship benefits to graduates of Kalamazoo Public Schools (KPS), a midsized school district (numbering a little over 10,000 students) with a racially and economically diverse student population. Anonymous donors promised to pay up to 100 percent of college tuition for any Kalamazoo Public School graduate attending a public college or university in Michigan. Tuition subsidies start at 65 percent of college tuition for students enrolling in KPS from ninth grade, and gradually increase to 100 percent for students attending since kindergarten. The scholarship does not require any minimum GPA or other academic achievement in high school, or any demonstration of student financial need. Students must simply get into a college and maintain a 2.0 GPA in college. In sum, the Kalamazoo Promise is unusual among scholarship programs in its universality and generosity.

The Kalamazoo Promise (for short, the Promise) has attracted much attention and many imitators. In 2008, the Economist ran a piece on the scholarship and the community (“A Promising Future,” Economist 2008). In part because of the Promise, in 2010, President Obama gave his first commencement address (and became the first American president to speak at a high-school graduation ceremony) to the graduating class of Kalamazoo Central High School. At least 24 areas around the country have started or are trying to start Promise-style programs, sometimes with private funding and sometimes with public funding.4

The tuition subsidies of the Promise provide incentives for higher academic output. Students who otherwise might choose to attend the state university located in Kalamazoo, Western Michigan University (WMU), may use the tuition subsidy to attend higher-ranked state universities such as Michigan State University or the University of Michigan. Students who otherwise might have chosen to attend the local community college might, with the help of the subsidy, choose WMU.5 Students, who, without the Promise, might not have attended any college, may use the subsidy to

---

4 See http://www.upjohn.org/promise/promisescholarships.html for a list of such programs.
5 For empirical evidence of such a shift in the choice set of colleges following the Promise, see Andrews, DesJardins, and Ranchhod (2010).
go to a community college. Succeeding at these higher academic aspirations requires that students have better academic performance and behavior, so that they can be admitted to and graduate from more demanding postsecondary institutions.

The aim of our paper is to study the effects of the Kalamazoo Promise on student achievement and behavior. To this end, we use one aspect of the Kalamazoo Promise that bears resemblance to a “quasi-experiment.” The surprise November 2005 announcement of the Kalamazoo Promise created a large change in expected college tuition costs that varied across different groups of students based on prior enrollment decisions. Specifically, the morning after the Promise was announced, some students enrolled in Kalamazoo Public Schools found themselves to be eligible for a 100 percent tuition subsidy, others for a smaller tuition subsidy, while still others could expect to receive no scholarship. The tuition subsidy amount depended upon how long the student had been enrolled in KPS. That enrollment decision, however, had been made without knowledge of the Promise. This variation across student groups in the surprise change in college tuition costs is arguably exogenous to unobserved student characteristics. Therefore, it is plausible to argue that effects on student achievement and behavior that are statistically linked to such exogenous tuition changes can be interpreted as causal effects of the program. We estimate this effect by estimating a “difference-in-differences” regression where we compare the change in student outcomes across time for different “length of enrollment” groups. This “difference over time,” before and after the Promise, in the achievement and behavior of the different student groups addresses potential concerns of unobserved differences between students who started their enrollment in KPS at different grades.

The quasi-experimental investigation is needed because there is theoretical uncertainty about the magnitude, and even the sign, of the Promise’s effects on student behavior and achievement. On the one hand, the Promise relaxes credit constraints of attending any public college in Michigan and lowers the relative cost of selective state universities, which may spur the students to exert additional effort. On the other hand, one could hypothesize that the Promise may diminish the value of merit-based scholarship, leading the students to exert less effort than in the absence of the Promise. Furthermore, for students from disadvantaged families, the benefit of the scholarship might be relatively small, as they are eligible for need-based financial aid (although
many may not be fully aware of this eligibility). Finally, a question remaining is whether high school students understand the mapping between education inputs and outputs well enough to respond to incentives induced by a college scholarship (for a discussion of financial incentives in education, see Fryer, 2011). Perhaps the benefits of the Promise are too delayed and too uncertain for the Promise to have an effect. In sum, whether the Promise has an impact on student high school outcomes, and how big the impacts are, is essentially an empirical question.

Our analysis finds that the Kalamazoo Promise does have statistically and substantively significant effects in improving student achievement levels and behavior. Specifically, we observe a decrease in the number of days spent in suspension by over 100 percent. We interpret our findings as indirectly supportive of the conclusions drawn by Fryer (2011). When confronted with incentives generated by the Promise, students are more likely to react along a margin they perceive that they can control, such as improving their behavior. For the overall sample, we do not find effects on the GPA. For African American students we observe a dramatic increase in GPA, ranging from about 0.17 standard deviations to about 0.60 standard deviations. For these students, whose baseline achievement and behavior indicators are lagging those of white students, the decrease in the number of days spent in suspension appears to spill over into a higher GPA. We speculate whether this could be due to the number of days spent in suspension exceeding beyond a “tipping point” beyond which GPA increases by the virtue of students being present in the classroom for some critical number of days. Finally, our estimated effects are only apparent when the analysis controls for student “fixed effects”—that is, when it actually considers differences in behavior of the same student before and after the Promise announcement.

The remainder of the paper is organized as follows. In section 2, we discuss related previous research literature. This includes research on the effects of student financial incentives on student achievement and behavior and research on effects of the Kalamazoo Promise. Section 3 provides further background information on the Kalamazoo School District and the Kalamazoo Promise. Section 4 describes the data we use, as well as the econometric models we use to analyze the data. Section 5 describes the results. The final section concludes.
2 Related Literature

Relevant research to this paper includes studies of how financial incentives affect student achievement and behavior, and how the Kalamazoo Promise has affected students, the school district, and the Kalamazoo area.

Several recent studies have examined how student achievement and behavior are affected by financial incentives. Kremer, Miguel, and Thornton (2009) study the effects of a merit-based randomized scholarship program for girls in primary schools in Kenya. The evaluation of the scholarship, which was implemented as a randomized experiment at the school level, provided the winning sixth-grade girls with an award for the next two academic years. The authors find that the scholarships increased performance by 0.09–0.14 standard deviations. Importantly, the authors find positive externalities for boys, who were not entitled to the scholarship, and for girls in the bottom quartile of pretest scores (i.e., students that had a relatively low probability of winning the scholarship).

Dhiraj Sharma (2010) studies the impact of a randomized cash rewards program among Nepalese eighth-graders. Similar to Kremer, Miguel, and Thornton (2009), the randomization was conducted across schools. The cash rewards were based on total scores on exams. Sharma finds that the financial impact of these incentives equaled about a 0.09 standard deviation gain in aggregate scores. He also finds that this gain was not uniform across different subjects. He interprets this finding as being in line with the hidden effort model involving multitasking, where one would expect an agent to increase marginal effort towards activity most likely observed by the principal.

A related strand of research studying the effects of financial rewards on academic performance looks at vouchers. Angrist et al. (2002) and Angrist, Bettinger, and Kremer (2006) study the randomly distributed vouchers in Colombia that partially covered the cost of private secondary school for students who maintained satisfactory academic progress. The authors find that, three years after the lotteries, the winners of the vouchers were about 10 percent more likely to have finished eighth grade and scored about 0.2 standard deviations higher on achievement tests.
Several studies examine how incentives affect student performance in developed economies. Angrist and Lavy (2009) look at the effects of a cash rewards experiment on graduating (obtaining a Bagrut) from Israeli high schools. The authors find strong effects among high-ability women. Angrist, Lang, and Oreopoulos (2009) study the effects of merit-based scholarships on students who are solid performers, but not top-ranked, among entering first-year undergraduates at a large Canadian university. They, too, find strong effects for women, but none for men. In a similar study, Leuven, Oosterbeek, and van der Klaauw (2010) conducted a randomized experiment among first-year undergraduate students at the University of Amsterdam. The experiment entailed a cash reward for those students who completed all of their first-year requirements by the start of the next academic year. They find that rewards matter only for high-ability groups; the average causal effects are small and statistically insignificant.

The aforementioned studies deal with incentives related to academic output—performance on tests, grades, or fulfilling certain requirements. Standard agency theory suggests that in order for a principal (such as a school or a teacher) to incentivize the agent (the student) to exert effort—which is only in part observed by the principal—the optimal contract should be conditional on output. But this result does not hold if students do not understand the mapping between educational inputs and outputs. This issue has been studied in a series of experiments aiming to understand what incentives work best in urban schools (Fryer 2011). Using data from randomized experiments conducted in school districts in New York City, Dallas, Chicago, and Washington, D.C., Fryer concludes that incentives tied to output (e.g., being paid to do well on a test) are not as effective as those tied to inputs (e.g., being paid to read a book).\(^6\)

In the Promise’s case, the program provides complex and contradictory incentives for changes in student behavior and achievement, which may in turn be only partly understood by students. Because Promise-eligible students can more easily afford more selective colleges and universities, such as the University of Michigan, the program provides some incentive for students to change behavior and achievement, particularly high school behavior and

---

\(^6\) Fryer is, however cautious in interpreting his findings as a panacea and points out the need to understand the relationship between inputs in the education production function. If there are important complementarities between various inputs, then conditioning rewards on one input may prove ineffective.
achievement, in order to be able to be admitted to and succeed at these more selective post-secondary institutions. However, as Fryer’s work underlines, students may not fully understand what behavior and academic achievement is needed for these more selective institutions, or may not fully understand how to change their behavior and achievement. One unintended effect of the scholarship could be that it may lower the value of merit-based scholarships, thus leading the students to exert less effort than otherwise. However, many students may not fully understand what is required for merit-based scholarships, or the Promise’s effects on such scholarships. For low-income students, the Promise may in many cases simply replace institutional or government need-based aid, which would limit the effects of the Promise. However, low-income students may not fully understand the need-based aid system and what their eligibility is for such aid. In addition, from the perspective of the students, the tuition subsidies of the Promise might be too delayed and too uncertain for the Promise to have any effect. Finally, if high school students do not understand the mapping between educational inputs and outputs, then the benefits of a college scholarship might appear too abstract to alter any behavior.

Whether the Promise has changed student behavior has not yet been studied in-depth. However, various other aspects of the Kalamazoo Promise program have been studied in a number of recent papers and books (see, e.g., Miller-Adams [2009]). Bartik, Eberts, and Huang (2010) study changes from before to after the Promise in enrollment and students’ test scores. The authors find a dramatic post-Promise increase in enrollment. Furthermore, after decades of shrinking enrollment among white students, the Promise has led to a stabilization of KPS’s racial makeup. These enrollment effects are due to a one-year increase in the entry rate to KPS, in the year after the Promise, accompanied by a permanent decrease in the exit rate, with these patterns occurring for all ethnic groups. These entry rate and exit rate effects are consistent with the Promise making KPS significantly more attractive to students and their families. Bartik, Eberts, and Huang also find some evidence that since the Promise, KPS test scores have increased somewhat faster than in similar Michigan school districts.

---

7 The anonymous donors have stated their intention for the program to continue indefinitely and have guaranteed that if the program is ever ended, all students enrolled in KPS at that time would receive the scholarship. We cannot however, rule out that students are still uncertain about receiving the Promise.
These results are further corroborated by Miller (2010) who also looks at the reaction of the real estate market. One purpose of the Promise is to promote the Kalamazoo area’s economic development. These economic development effects would occur by Kalamazoo attracting parents and businesses. Miller (2010) addresses these issues by studying whether the effects of the scholarship have been capitalized by the real estate market. Using a difference-in-differences design, Miller (2010) does not find positive effects of the Promise on house prices. However, Miller finds that the Promise has had positive effects on student culture, for example by, improving school safety.

Andrews, DesJardins, and Ranchhod (2010) use a difference-in-differences method to study the effects of the Kalamazoo Promise on college choice. Using proprietary data from the ACT Student Profile Questionnaire, they estimate the effect of the Promise on the test takers’ intended college choice set. Using other public high schools in the state of Michigan as a control group, the authors find large effects of the Promise on college choice, especially for students who are economically disadvantaged. The Promise increases student interest in all Michigan public colleges and universities, with particularly strong effects on student interest in the flagship schools—the University of Michigan and Michigan State University. Therefore, this study provides some evidence that the Promise increases student interest in more selective universities, which will require higher student achievement during high school.

3 Background Information on the Kalamazoo Public School System and the Kalamazoo Promise

Kalamazoo Public Schools is a midsized, predominantly urban school system. As shown by Figures 1 and 2, before the Kalamazoo Promise, enrollment had been declining for many years. This partially reflects relatively modest economic growth in Michigan and Kalamazoo. In addition, it reflects Kalamazoo’s status as a district centered in a core city that has more intense social and economic problems than its surrounding metropolitan areas. For example, family poverty rates as of the 2000 census were 13.6 percent in the city of Kalamazoo and 6.5 percent in all of Kalamazoo County.
Kalamazoo Public Schools is a mix of an urban school system with some surrounding suburban areas. Because of school district consolidation in the 1950s, the Kalamazoo school district also includes a considerable portion of suburban and rural population and land, mostly in Kalamazoo Township and Oshtemo Township. Figure 3 shows how the district (shaded region) compares to the city (in the center of the map).

Even before the Promise, the Kalamazoo school district had a considerable portion of poor students and the students were from a wide variety of ethnic groups. Figure 4 shows trends in the number of black, Hispanic, and non-Hispanic white students in the district. As can be seen in the figure, although KPS retained a considerable percentage of white students and students who did not qualify for free or reduced price lunches, the percentage of students in these two groups was clearly falling.

Since the advent of the Kalamazoo Promise, enrollment in KPS is on the rise (see Figure 2). Furthermore, enrollment seems to be up proportionately for all ethnic groups, so the ethnic percentages have stabilized (Figure 4). As mentioned, the analysis by Bartik, Eberts, and Huang shows that these trends are the result of KPS having a one-year temporary jump in entry rates and a permanently lower exit rate. These patterns are consistent with a Promise effect.

**The Kalamazoo Promise**

What is the purpose of the Kalamazoo Promise? The anonymous donors, according to information provided by the school district, believe that the purpose of the Promise is to promote economic and community development, in part by attracting parents and businesses to the Kalamazoo area. The Promise is also intended to boost educational achievement and attainment. Finally, the Promise could reasonably be interpreted as being intended to help increase confidence in Kalamazoo Public Schools.
The Kalamazoo Promise is available to all students who graduate from Kalamazoo Public Schools (KPS), reside in the district, and have been KPS students four years or more. The scholarship covers up to 100 percent of all tuition and mandatory fees for up to four years and must be used within 10 years of high school graduation. The benefit is graduated based on the length of attendance in the KPS system. Figure 5 traces the relation between grade level enrollment in KPS and the expected fraction of tuition and fees covered if the student graduates from KPS.

Between grades 3–9, there is a 5 percent decrement in the generosity of the scholarship for each additional year of postponing enrollment in KPS. The biggest discrete drop-off in generosity occurs between enrolling in ninth grade (65 percent) and tenth grade or later (0 percent). A student entering KPS in grade 10 or afterward is ineligible for Promise tuition benefits.

One requirement of the scholarship is that enrollment and residency must be continuous. For example, suppose a student started in KPS in kindergarten. If that student stays in KPS until graduation, she is eligible for a 100 percent Promise tuition subsidy. But if that student instead switches to another district in fifth grade and later reenrolls in KPS in ninth grade, she will only be eligible for a 65 percent Promise tuition subsidy. The student’s enrollment in KPS from kindergarten to fourth grade is made irrelevant to her Promise benefit by her departure; only the last grade at which continuous enrollment began is relevant in determining the tuition subsidy.

Other than date of continuous enrollment, no other aspect of a student’s K–12 experience or family background directly affects eligibility. Students do not have to demonstrate financial need. Students do not need to maintain any minimum GPA in high school, or take any particular mix of courses. Students of course do need to be admitted to a college to receive Promise benefits, and so expected college entrance requirements may alter student behavior if the Promise alters college aspirations.

---

8 This information comes from the Kalamazoo Promise Web page [http://kalamazoopromise.com/uploaded/Promise%20Senior%20Information%20Brochure.pdf](http://kalamazoopromise.com/uploaded/Promise%20Senior%20Information%20Brochure.pdf).

10
The scholarship applies to students who are admitted to and enrolled at any public university or community college in the state of Michigan. The students must be full-time, meaning that they are taking 12 credit hours per semester at a minimum. Students must maintain a 2.0 GPA in college. Students who fall below a 2.0 GPA can become eligible again for the Promise if they continue attending college on their own dime (or their family’s) and then succeed in increasing their cumulative GPA above the 2.0 college GPA requirement.

Students are eligible for Promise benefits for up to 130 credits of undergraduate college or university education. As was stated above, this eligibility extends for up to 10 years after high school graduation. The Promise’s benefits can be applied to certificate programs at community colleges, not just programs leading to associate’s degrees or bachelor’s degrees. However, eligibility does not include private apprenticeship programs outside of public community colleges.

In order to gain an appreciation of the value of the Kalamazoo Promise, we calculate a “back-of-the-envelope” estimate of the discounted present value of the scholarship. In order to do this we need to make certain assumptions about the set of colleges that the graduates of KPS attend. Using information about enrollment decisions of the first cohort of Kalamazoo Promise recipients, we divide this sample into community colleges and 4-year universities.

About 45 percent of new enrollees in 2006 attended a community college (almost all of them attended the local Kalamazoo Valley Community College). The remainder, 181 students, enrolled in public universities, of which the majority enrolled at Western Michigan University (101 students), followed by Michigan State University (37), and University of Michigan (17). For the sake of simplicity we assume that these college going probabilities remain constant over time and across different tuition subsidy groups. In Table 1, we calculate a present value of the Promise for different subsidy groups. Our calculations indicate that for someone eligible for a

---

9 In fact, the Kalamazoo Promise has altered these probabilities. Our aim with this calculation is for illustrative purposes only. In column (2) of Table 1, we lower the probability of going to a community college to 0.30. Since the likelihood of attending a 4-year college correlates with family background, such weights might better reflect preferences of high-income families. This reweighting increases the present value of the scholarship, holding other parameters constant. For an in-depth study on how the Kalamazoo Promise altered the college choice set across time and different income groups, see Andrews, DesJardins, and Ranchhod (2010).
100 percent tuition subsidy, the present value is about $27,413, while for someone who is eligible for a 65 percent subsidy; the present value is about $17,818.

**Take-up of the Kalamazoo Promise and Variation in Eligibility**

The Kalamazoo Promise has been widely used among KPS graduates. As Table 2 shows, in the various graduation years, 80 to 90 percent of KPS graduates have been eligible for at least some Promise benefits. Of those eligible, between 82 and 85 percent have at some point used Promise benefits.

There is wide variation in degree of Promise eligibility among high school students in KPS. As shown in Table 3, among KPS graduates, of the varying possible tuition subsidy groups, the largest group is those eligible for a 100 percent tuition subsidy (attended KPS since kindergarten). However, there are also large numbers ineligible for a subsidy (last entered KPS after ninth grade), eligible for a 65 percent tuition subsidy (entered KPS at ninth grade), and eligible for a 95 percent subsidy (entered KPS at first, second, or third grade).

Anecdotally, we know that many of those who enter KPS at ninth grade have previously been students who attended private or charter schools from kindergarten through eighth grade. Many private and charter schools in the Kalamazoo area do not include high school, perhaps in part because of the larger costs per student that are characteristic of high school. We therefore might expect some differences in academic performance between students entering at ninth grade and students entering at other grade levels.

**4 Data and Methods**

**Data**

Our data come from KPS administrative records. In our analysis, we focus on students in grades 9–12. We chose this focus for several reasons. First, this allows the analysis to include some students who end up being ineligible for the Promise because they entered after ninth grade.
Obviously, for earlier grades, all students in those grades when the Promise announcement was made will be potentially eligible for at least a 65 percent tuition subsidy. Second, for high school students as opposed to younger students, the tuition subsidy benefits of the Kalamazoo Promise are closer in time. Third, high school students, compared to students in earlier grades, are more likely to believe that their achievement and behavior in school will affect admission prospects at more selective colleges.

The structure of the data set is shown in Figure 6. Our regression sample consists of ninth through twelfth graders from the school years 2003–04 to 2007–08. This regression sample is shown within the stair step black lines. Our “window of observation” thus consists of two pre-Promise years, the year the Promise scholarship was announced, and two post-Promise school years. Because our enrollment data only go back to 1997–98, we cannot track information for everyone in our regression sample for earlier than sixth grade.10

For those school years, we have data on student characteristics, grade-point averages, types of courses taken, and disciplinary actions. The disciplinary data consist of information on days of suspension and detention. Our data set is an unbalanced panel—students are in the panel for various lengths of time, depending upon what grade they started in and how long they stayed in KPS.

For each student in each of those years, we can track enrollment history starting in the school year 1997–98. This information allows us to calculate what the student’s eligibility for the Promise would have been if the Promise had been in effect for that year and the student had continued attending KPS until graduation. We call this the student’s “virtual Promise benefit.”

For each student in high school in any school year from 2003–04 until 2007–08, we calculate such a “virtual Promise benefit.” To do this, we hypothesize that for every time period, students

10 Under the Promise, continuous enrollment starting in sixth grade makes a student eligible for an 80 percent tuition subsidy. To be consistent for all cohorts in our sample, we classified all students whose “virtual Promise benefit” is 80 percent or above as one sub-category. We did not find significantly different patterns by conducting such disaggregated analysis.
are forward-looking and adjust their performance (or behavior) as a function of the expected generosity of the Promise, given that they maintain a continuous enrollment in KPS, graduate, and enter a public college or university in Michigan. Our interest lies in estimating how the variation in this perceived future tuition subsidy at the time of observation affects achievement and behavior. Throughout our analysis, we therefore focus on these virtual Promise benefits (as opposed to the levels of tuition subsidy at the time of graduation), since they capture a shock to the expectations of the student following the announcement of the scholarship.

For school years 2003–04 and 2004–05, these virtual Promise benefits are virtual in the sense that the student was unaware of them, as the Promise was not announced until November 10, 2005. Therefore, we would assume that any effect of this simulated Promise benefit in those years reflects effects that are associated with what grade the student entered KPS, rather than the effect of a Promise benefit of which the student had no knowledge.

Including 2003–04 as an additional control year allows us to see the extent to which results jump around during the pre-Promise years. If there are big changes across these two pre-Promise years in student achievement and behavior for students with different enrollment histories in KPS, and hence different virtual Promise eligibility, then this raises some doubts about whether any differences between pre and post-Promise years are actually due to the Promise.

On November 10, 2005, students became aware of the potential Promise benefits that would accrue to them given their enrollment in KPS to date. This allows some effect of Promise benefits on student achievement and behavior after that date. However, it would be reasonable to assume that there might be some lag in students’ fully understanding the incentives of the Promise, or being in a position to act on those incentives. By November 2005, students had already made certain decisions about that academic year, such as what courses to enroll in for the fall of 2005. The school year 2006–07 is a full post-Promise year. This year allows the time from the fall of 2005 to the fall of 2006 for students to more fully understand what the Promise might mean for their future. Including the 2007–08 school year adds a second full post-Promise year to help confirm effects estimated for the 2006–07 year. If estimated positive effects of the Promise
do not persist or strengthen for the second post-Promise year, this raises some doubts about whether these effects are truly due to the Promise.

Therefore, we choose to have two full years pre-Promise and two full years post-Promise, in order to increase our knowledge about whether any estimated Promise effects are due to the Promise rather than to idiosyncratic effects of a particular school year. On the other hand, we choose to limit ourselves to just these five school years, for two reasons. First, limiting ourselves to these five years also limits the extent to which other changes in KPS’s policies and practices might differentially affect different “length of enrollment” groups, who will differ in general in both observed and unobserved characteristics. Second, as it turns out in our analysis, in controlling for student fixed effects we must restrict our attention to years close to the Promise to have students whose high school careers comprise the years both before and after the advent of the Promise.

Restricting ourselves to these five years helps our research analysis from a pure social science perspective, in that it isolates the effects of the Promise itself, without the accompanying effects of subsequent policies instituted by either the Kalamazoo school district or the Kalamazoo community. However, from a broader perspective, some of the more interesting questions involve these subsequent policies. Do Promise-type programs help lead to a broad set of holistic policies by school districts and communities, which transform the cultural attitudes of a community towards higher education and educational opportunities? Although such questions are fascinating and important, it is difficult and perhaps impossible to identify a convincing comparison group for evaluating the overall effects of the Kalamazoo Promise plus subsequent school district and community policies. It is also difficult to identify a convincing comparison group to determine whether the Promise actually led to subsequent policy changes. We focus in this paper on what we can evaluate with a convincing comparison group, which is the immediate effects of the Promise itself on student behavior and achievement. It is quite plausible that the effects of Promise-style programs would be much larger if we considered their possible synergistic effects with complementary school district and community policies to promote educational attainment. Analysis if natural experiments most often omits general equilibrium effects.
Methods

Estimating effects of the Kalamazoo Promise programs is challenging. A key problem is the difficulty in identifying suitable comparison groups. For example, consider comparing a district with a Promise-style program with a district that lacks such a program. The achievement/behavior levels and trends in the Promise district, compared to the non-Promise district, could be due to the Promise. Alternatively, these differences could be due to other differences in the two school districts’ policies, which could lead to differences between the two districts in time trends. One promising approach would be to use panel data for both the Promise and the non-Promise district and conduct a difference-in-difference analysis. At this moment, we do not have access to microdata from other non-Promise districts. This restricts our analysis to being conducted within the KPS.

One possible comparison group within KPS can be derived by using variation in eligibility for the Promise stemming from differences in pre-Promise enrollment decisions. As the announcement of the Promise in November 2005 was a surprise, families did not choose the timing of enrollment of their children in KPS in anticipation of the possibility of coverage by a universal scholarship program. Thus, some children enrolled in KPS found themselves to be eligible for 100 percent tuition, some for 65 percent tuition, while others could expect to receive no tuition subsidy at all—in the first two cases, of course, it being conditional that they remain in KPS until graduation. This unanticipated and exogenous variation in the eligibility for a college scholarship provides a unique opportunity to causally infer the impact of the Promise on student outcomes and behavior.

This differential change in tuition scholarships can be seen as a natural experiment: an exogenous change in policy assigns certain individuals to a “treatment,” whereas other individuals are not affected (the “controls”). However, because it is a natural experiment, this differential change in tuition scholarship is not randomly assigned, but rather depends on pre-Promise enrollment decisions.
Because students enrolled in different grades will differ in their behavioral and achievement levels, it would be misleading to use a simple comparison of achievement levels between eligible and ineligible students to estimate the Promise’s effects. There is no reason to think that students eligible for no Promise benefits because they enrolled in tenth grade or later will on average be similar in academic achievement or behavior to students eligible for various Promise tuition subsidies. By simultaneously focusing on not only changes over time but also differences across eligibility groups, we end up with comparisons that more plausibly represent Promise effects. In order to address concerns of bias due to systematic pre-Promise differences between groups of students that enrolled in KPS in ninth grade as opposed to, say, kindergarten, we conduct our long-term analysis in regression-adjusted difference-in-differences framework where, for the pre-Promise school years, we calculate the virtual benefit as if the Promise were in effect during those years. Hence, we treat the ineligible students as our control group.

This approach has, however, an important caveat, namely that it is probable that the group of students entitled to zero benefits from the Promise is also affected by the change in KPS from, for example, peer effects. If even this “zero eligibility” group has been affected by the Promise and the direction of this effect is the same as for students eligible for the Promise, the estimated effect will understate the true treatment effect.

Equation (1), below, provides a formal framework for evaluating the effects of interventions on student achievement. First, consider the following regression:

\[ y_{it} = \alpha + \varphi I\{Benefit > 0\}_i + \delta_t T_t + \gamma T_t \times I\{Benefit > 0\}_i + x_{it}\beta + u_{it} \]  

Equation (1)
announcement of the Promise (2004–05) as our omitted reference categories. The rationale is that Promise eligibility versus no Promise eligibility may be more salient for most students and their families, and that we might get somewhat more precise and informative estimates by simply looking at the average effects of any Promise eligibility. The coefficients on the $T \times I\{\text{Benefit} > 0\}$ interactions are the differences between the pre- and post-Promise outcomes for the two groups determined by the generosity of the Promise formula.

The identifying assumption of model (1) is that the $T \times I\{\text{Benefit} > 0\}$ interaction terms are orthogonal to the error term $u_{it}$. We believe this untestable assumption to be plausible, as the fraction of the tuition covered by the Promise is a function of past decisions that were not made in anticipation of the announcement of the scholarship. If this assumption holds, the assignment into various levels of generosity of the Promise makes the estimates of $\gamma$’s the effects of the scholarship on outcomes.\footnote{In this specification we cannot assume the $u_{it}$ to be independent over time; hence we compute individual cluster-robust standard errors.}

Although the eligibility for the different levels of the KPS dummies ought to be orthogonal to student characteristics at the time of the announcement, in order to increase the precision of the estimate we also include observable characteristics of the students, denoted by the vector $x$, such as gender, race, grade level, and free and reduced price lunch status.

As with virtually any educational policy analysis, it is impossible in principle to exclude student effects on student educational achievement and behavior. Prior research suggests that such student effects may be large. However, here the relevant issue is whether we need to control for student effects—e.g., to condition on these effects and thereby treat them as fixed, in order to get unbiased estimates of Promise effects. We will need to control for student effects as fixed effects if such student fixed effects are correlated with the $T \times I\{\text{Benefit} > 0\}$ interaction terms. Hence, in our analysis we also replace $\alpha$ in Equation (1) with $\alpha_i$.

When will student fixed effects be correlated with these year dummy × Promise eligibility interaction terms? This correlation will occur if there is differential migration of different
Promise eligibility groups into or out of the KPS school district after the Promise. For example, we could imagine that some families with “better students”—in part “better” for reasons that are unobserved—may be less likely to move students with zero eligibility out of KPS because of the Promise. This might occur if such students also have younger siblings who are eligible for the Promise.

In principle, once a student is enrolled in KPS, what that student can expect in terms of the generosity of the scholarship can never increase—it remains at a constant level or it can decrease, if the student drops out and reenrolls. From our conversations with the administrative staff of the Kalamazoo Promise, we became aware of certain deviations from this principle. For example, if a student starts in tenth grade and goes back to ninth, the practice adopted by the Promise is to count the student as if she had enrolled in the district in ninth grade. This creates certain incentives for doing poorly in tenth grade, in the hope of being given a chance to enroll in ninth grade and thus have the opportunity of being awarded 65 percent of tuition. It is not clear, however, that the students and their families are aware of this practice.

After our conversations with the administrators of the Kalamazoo Promise, we made the following assumptions for nontypical cases. If a student enrolled sometime in the middle of the school year, we assume that their expected Promise eligibility is based on continuous enrollment starting the following fall semester. The student has until the state school fall census date (October 1) to enroll in KPS.

As mentioned previously, we calculate a virtual benefit for each student for each year. We model it as a function of enrollment in KPS. In the case where there is within-person variation in the benefit level, we assign the benefit level equal to the chronologically first observed level. The concern with within-student variation in the virtual benefit level in the years following the announcement of the Promise is that decisions affecting enrollment are done with the potential knowledge of the formula determining the generosity of the program and hence possibly endogenous. As a robustness check, we later restrict our analysis to only include students whose benefit level has remained time-invariant in the years following the Promise.
As described previously, our dataset is an unbalanced panel, where we observe new students entering as well as established students leaving the school district. There is little concern that, before the announcement of the Promise, this in- and out-migration would be systematic with respect to anticipation of a universal scholarship. However, in the post-Promise years, students have an incentive to enroll in KPS. As this post-Promise sorting is endogenous, we exclude all the new students who enrolled in ninth grade after November 10, 2005 (as these students are entitled to 65 percent of tuition covered if they stay enrolled). We allow for new entrants in grades 10-12, as they are entitled to zero coverage and have no financial incentives to enroll in KPS because of the Promise. Nevertheless, in order to be prudent about maintaining the exogenous nature of how the Promise assigns the different levels of generosity, we conduct a robustness check by excluding these observations. This turns out not to matter much for our main results, though for some results it leads the point estimate to lose some of its precision.

**Descriptive Statistics**

Table 4 presents descriptive statistics for the sample. In the table we pooled together the years into a “before” (that is, 2003–04 and 2004–05) and an “after” (2005–06 until 2007–08) period, which separated whether the student is eligible for any or no future tuition subsidy (“Benefit > 0” and “No benefit”). We report the sample means, the standard deviations (although not for proportions), and the number of observations (that is, the number of student-year cells).

As can be seen from the demographic data, the student population of KPS over this time is certainly diverse. Many disadvantaged students are included, as well as many racial minorities, but there are also many white students and nondisadvantaged students. We notice several differences between the groups eligible for some future tuition subsidy and those groups ineligible. Before the announcement of the Promise, the recent enrollees—entitled to no future tuition subsidy—were more likely to be African Americans and beneficiaries of free or reduced-price lunch.

As our dependent variable, we use several different metrics. For each student and year, we compute grade-point average (GPA). If the grade was a letter, we assign A to equal 4, B to equal
3, C to equal 2, D to equal 1, and F to equal zero. We obtain the incidence of suspension and the number of days within the school year spent in suspension. We also collect data on the incidence of “in-school suspension” (that is, detention) and, we look at the effects of the Promise on credits earned.12

In KPS, students also participate in a credit recovery program, which allows them to accumulate more credits than the normal eight per school year. (There are normally eight credits per school year. KPS during this time was on a block schedule, in which students normally took four courses, at one credit per course, each semester. Each course was counted as if it were equivalent to a full year of a course under the previous six-period day.) We top-code the maximum number of credits earned at 12. This procedure affects 59 observations.

Student achievement is relatively low using our measures: GPAs are low, and we record a low mean number of credits obtained per year (out of 8 possible). There is certainly plenty of scope for a Kalamazoo Promise program to improve student achievement in high school.

Using GPA as a measure of achievement raises some concerns about whether changes in teacher behavior might drive any results – have teachers, for example, become more lenient following the announcement of the Promise? As long as any such changes in the school environment are uniformly affecting students, their impact is controlled for by the time fixed effects T in equation (1). A potential pitfall for our empirical strategy would be if, post-Promise, teachers systematically graded a given student (as we control for fixed effects) who is eligible differently than another student who is not eligible. We deem this rather unlikely. However, in such case, one would like to conduct the analysis using more standardized measures of high school achievement, such as the MEAP test.13 Unfortunatley, the MEAPs are only comparable since the fall of 2005, just about the time when the Kalamazoo Promise was announced. Thus, we cannot control for pre-Promise trends, which turns out to be important.

12 We have also looked at the impact on the number of attempted advanced-placement (AP) credits. AP classes are intended to offer material at a similar level to undergraduate courses in college. KPS displays relatively low levels of enrollment in AP courses and of students obtaining AP credits. Our analysis indicates that changes in AP enrollment and in the attempted earning of AP credits is driven by group × year trends.
13 MEAP is an acronym for Michigan Educational Assessment Program and is a standardized test used by the state of Michigan for No Child Left Behind Accountability.
There also is considerable scope for student behavior to improve. A large proportion of students were suspended or detained each year, and the figure for mean days of detention and suspension (which includes those students who had zero days for the year) is large enough that we certainly could imagine some significant reduction in these indicators of poor behavior.

Table 4 indicates that some ninth graders are not eligible for the Promise. In those cases, the student had enrolled past the state school fall census date, October 1, and according to our conversations with the administrators of the scholarship, the enrollment of such a student counts as if the student had enrolled in tenth grade. Finally, we see a decline in the fraction of students eligible for 65 percent or more of the future tuition subsidy. This happens because we drop all of the new students entering ninth grade after November 10, 2005.

5 Results

Main Results

Tables 5 and 6 show the main results. Table 5 shows results for academic achievement dependent variables, and Table 6 shows results for behavioral dependent variables. The tables show results for the various Promise eligibility categories and year dummies and for their interactions, included in each regression. The immediate pre-Promise year of 2004–05 and the zero benefit category are the omitted dummies.

We do not report coefficients on other controls, which are included in these specifications. These other variables include controls for grade level. We also include controls for gender and for race/ethnic group (white, black, Hispanic) and a dummy, which indicates whether there is variation in the benefit across time after school year 2005–06 within students and any new enrollees post-2005–06. Obviously, these controls are eliminated when we include student fixed effects. The fixed effect regressions do control for free/reduced-price lunch status, which is a time-varying variable.
Our focus is on the estimated effects of the Promise benefit categories interacted with the dummy for the year 2005–06 (the year of the announcement) and for 2006–07 and 2007–08, the post-Promise years. These interacted effects are relative to the effect for the zero-benefit category in the school year 2004–05. For the fixed-effect regressions, these estimated effects also control for the student’s performance or behavior in other years. In other words, we are looking at whether students in the various Promise benefit categories differentially changed in the years following the announcement relative to their own history, and then comparing these findings to what happened to students in the zero-benefit category.

As the tables show, in the regressions without fixed effects, Promise eligibility frequently has the unexpected sign, and it is sometimes statistically significant and negative. For example, without student fixed effects, students entitled to any Promise tuition subsidy are estimated to have a statistically significantly reduced GPA.

In contrast, results are more often of the expected sign and statistically significant when we control for student fixed effects. In particular, we find evidence that Promise eligibility had positive effects on GPA, although this effect is not precisely estimated.

A similar pattern emerges in columns (3) through (6) of Tables 5 and 6: simple OLS suggests a decrease (although not statistically significant), whereas controlling for individual student fixed effects reverses the sign on the coefficient.

The bottom rows of Table 5 and Table 6 provide another way of ascertaining the size of the estimated effects of the Kalamazoo Promise benefits on student achievement and behavior in the years following the Promise. As is often done in educational research, we compute the “effect size” of this policy for the dependent variables that are continuous. This simply rescales the estimated effects by the standard deviation of these variables across individual students in some control group, which in this case is taken to be the standard deviation across individual students in the pre-Promise year of 2004–05. For the GPA, “Credits earned,” and “Days suspended” the
estimated effect sizes are about 0.1σ–0.16σ in absolute magnitude, which represent effect sizes that considered typical of many educational interventions.\textsuperscript{14}

The average number of days of out-of-school suspension declined for Promise beneficiaries in 2006–07, compared to nonbeneficiaries, by a little over one day per school year. This is averaged across all students, including the approximately 80 percent of all students who received no out-of-school suspensions, and is large compared to average number of days suspended over all students of about two days. We see that this effect is even more pronounced in the school year 2007–08, with a decline of about two days.

As Tables 5 and 6 show, results differ considerably when controlling for individual student fixed effects. This implies that individual student fixed effects and their trends over time must be correlated with the interactions between year dummies and benefit categories. In the appendix, we present some figures showing trends in average fixed effects over time for different benefit categories. These figures show that average fixed effects do have different trends over time for different benefit levels. Specifically, for GPA and credits earned, average student fixed effects tend to increase over time for the zero-benefit category, whereas average student fixed effects tend to stay the same or modestly decline for the students eligible. For days suspended, average fixed effects increase over time for both groups, but by less for the zero-benefit category. These differential time trends are consistent with the absence of controls for fixed effects leading to the “wrong” sign for Promise benefits in the post-Promise year. Because fixed effects are the same for all students who remain in the sample over time, these trends reflect differences in the students moving into or out of KPS during that period. It seems that for the zero benefit group, this out-migration and in-migration has tended to lead to higher student fixed effects of the students that remain, whereas this is not as true for the students in the positive benefit categories. The causes of this differential migration form an interesting topic that we hope to explore in future research.

\textsuperscript{14} Bloom, Hill, and Lipsey (2008) discuss magnitude of effect sizes across different grades. It is known that learning gains are typically greatest between kindergarten and first grade, ranging sometimes in effect sizes larger than one standard deviation. The learning gains in later grades are typically much smaller. This in turn implies that an effect size of an intervention of 0.1σ in high school is a more pronounced impact than a 0.1σ in kindergarten.
Multiyear difference-in-difference analysis can be represented in a graph and enable detection of existing preintervention group × time trends. The idea is that if our estimation procedure is sound, we would not see any significant effects for Promise-eligible groups versus non-Promise-eligible groups in the years preceding the announcement of the Promise. This is a type of falsification test for our model.

The various panels of Figures 7 and 8 plot the difference-in-differences point estimates from the fixed-effects regressions, along with 90-percent confidence intervals, across the pre-Promise and post-Promise years. Recall that 2005–06 was only a partial Promise year, as the Promise was announced in November of 2005. We might expect effects in this first Promise year to be smaller, as it may take some time for students, parents, and teachers to make much of a substantial adjustment to the incentives provided by the Promise. In general, the effects are statistically insignificant for 2005–06.

Panel A of Figure 7 shows plots the difference-in-differences point estimates for GPA from column 1 in Table 5. It is clear from the plot that the estimate seems driven by a preexisting trend. In addition, the post-Promise point estimate is estimated imprecisely. Therefore, it is hard to argue that there is any convincing evidence of a causal effect of the Promise on GPA. As a robustness check we have also grouped students based on whether they are eligible for 65 percent tuition subsidy versus if they are eligible for 80 percent or more. Our rationale for this exercise has been that for both these groups of eligible students teachers might be even less certain about eligibility levels. The findings for GPA are very similar to the trend displayed in Panel A of Figure 7.15

We observe a similar trend for enrollment in AP courses and the number of attempted AP credits. Attempted AP credits are significantly lower for Promise-eligible students in the 2003–04 pre-Promise year compared to the 2004–05 year. On the one hand, this suggests that if we compared post-Promise AP credits for Promise-eligible students in 2006–07 with the 2003–04 year, rather than the 2004–05 year, the Promise effect might appear more significant. On the other hand, these results suggest that even in the pre-Promise period, there might have been some differential trends in AP enrollment among different Promise eligibility groups. For example, perhaps there were attempts to expand AP enrollment that particularly affected long-time KPS students versus newcomers. This therefore raises some doubts about the post-Promise results for this variable. However, for the other variables, there are no significant differences between Promise-eligible groups vs. ineligible groups across the two pre-Promise years.
Turning to Panels B and C, which plot the effect on credits earned and whether the student earned any credits (i.e., the point estimates from columns 8 and 10 in Table 5), we observe that following 2005–06, any preexisting group × year trend appears to have been reversed. The point estimate in the school year 2007–08 suggests that the probability of earning any credits is about 8.8 percent higher for students eligible for some future tuition subsidy. This latter point estimate is significant at the 5 percent level.

Figure 8 plots the point estimates from Table 6. The results are clear: there are no statistically significant differences in the pre-Promise effects. In addition, the point estimates in 2003–04 in Panel A through Panel D are approximately zero. Following the Promise, days spent in suspension decrease during the school year 2005–06 and continue to decrease.

Table 4 shows that the distribution of days spent in suspension and detention is quite skewed—as most students are not suspended or detained, there is a large cluster of zeros. In order to investigate whether the effect on total days suspended or in detention is driven by the extensive margin, we also plot the point estimates of the effect of the Promise on the probability of being suspended or assigned detention. The point estimate on the probability of being suspended is imprecise but also suggests a decrease; see Panel B of Figure 8. This suggests that the overall effect on days suspended is at least in part due to effects on the likelihood of being suspended.16

For detention, the pattern is different; the probability of being assigned detention at school appears not to have been affected. Hence, it is likely that the overall effect on days spent in detention is likely to be driven by the intensive margin.

Robustness checks

16 We do not model the analogous effect along the intensive margin due to the usual issues with regressions conditioning on the positive value of the outcome variable (see Angrist and Pischke [2010] Chapter 3). In order to get an idea how much of this effect is due to the intensive margin, we conduct the following back-of-the-envelope calculation. When differentiating the equation \( E(y|x) = E(y|x, y>0)Pr(y>0|x) \) with respect to \( x \), we obtain that the overall average effect of a variable \( x \) on \( y \) is a weighted average of the intensive and extensive margins: \( \frac{\partial E(y|x)}{\partial x} = \frac{\partial E(y|x, y>0)}{\partial x}Pr(y > 0|x) + \frac{\partial E(y|x, y > 0)}{\partial x}E(y|x, y > 0) \). Plugging in sample means and regression effects from Table 6 columns 2 and 4 for the school year 2007-08, we can back out the conditional effect on suspension equal to a reduction in less than 6 days in suspension.
Figure 9 shows some robustness checks: it shows the point estimates for probability of earning any credits and days spent in suspension for a reduced sample. We focus on these two outcome variables, as a) we deem them not to display pre-Promise trends, and b) the post-Promise point estimates were significantly different from zero, at least at the 10 percent significance level.

This restricted sample drops all the students who entered tenth through twelfth grade in KPS after the Promise was announced in November 2005. (We already excluded ninth graders who came after the Promise, as these ninth graders would be eligible for Promise benefits, which might differentially affect in-migration. However, we previously included tenth through twelfth graders who came after the Promise announcement, as they are ineligible for Promise benefits.) This reduced sample also excludes those who had a change in their benefit (dropped out and reenrolled, for example) in 2005–06 or later. In sum, 1,037 observations are dropped.

Who are the students in this zero eligibility group? These students consist of:

1. Students who enrolled in KPS in 2005 in their 9th-grade year after the state fall count date or did not stay throughout the whole school year. Thus, the first year “countable” towards the Promise for them was when they were 10th graders and that makes them ineligible.

2. Students who enrolled as 10th graders in 2005 – these students will not get any benefit even if they came before Nov. 10 because they were not in KPS as 9th graders.

The main effects of moving to this reduced sample are twofold. First, the estimated effects of the Promise on the dummy for credits earned lose some precision; it is now only statistically different from zero at a 16.4 percent level. Second, the effect is still positive and of important size: a 9-percentage-point increase in the probability of earning credits.

The lower panel for Figure 9 shows the effect on days spent in suspension. This effect is still statistically different from zero, though the point estimates are a bit smaller in absolute magnitude: in 2007–08, the decrease in days spent in suspension is 1.55.
We also considered specifications in which we dropped all newly enrolled 9th graders for all years. We wanted to make sure that our baseline results were not driven by our decision to only drop newly enrolled 9th graders after the Promise announcement. We found that dropping all newly enrolled 9th graders for all years did not significantly change any of our results.

**Analysis by subsamples**

Previous research studying effects of educational interventions often finds heterogeneous responses for boys and girls and by race/ethnicity. The economically and racially diverse nature of the KPS allows us to analyze student outcomes by race. Specifically, in Figure 10 we focus on African American students and impose the same sample restrictions as used in the robustness analysis in Figure 9. This subsample consists of 6,385 observations—5,808 eligible for any tuition subsidy and 577 observations not eligible for anything.

The results for African American students are striking. For black students, unlike for the entire sample, there do not appear to be as clear group × pre-Promise trends in GPA. Panel A suggests that following the Promise, GPA has increased and continued to improve for these Promise-eligible black students. There does not appear to be a clear pre-Promise effect in the school year 2003–04. The results are also very big in magnitude; for example, in the school year 2007–08 there was an increase of 0.70 in GPA. The GPA effects traced in Figure 10 translate to a 0.174σ increase in the school year 2005–06, followed by 0.280σ increase in 2006–07, and an enormous 0.63σ increase in 2007–08. One might wonder why these difference-in-difference point estimates keep trending up following the Promise as opposed to observing a one-time increase in GPA, which remains at a steady level past that. We would expect to find such a continuing increase if following the Promise there are synergies cross mapping into higher performance.

Panels C and D show the impact on days in suspension and detention. On average, the point estimate for black students implies a decrease of two days in suspension in the first full post-

---

17 We also conducted separate regressions for boys but did not find the response different from the rest of the sample.
Promise year and a 3-day decrease in 2007–08. Note that the effect on the number of days in detention is not precisely estimated.

For African American students there is a contemporaneous change in the effect the Promise has on days spent in suspension and GPA that we do not observe for the overall sample. On average, these students have a lower GPA and more days spent in suspension than their white counterparts do. We can only speculate whether this decrease in the days spent in suspension might have shifted past some “tipping point” beyond which more presence in the classroom leads to higher grades, while leaving the white students unaffected.

Discussion

Overall, we believe the results suggest that the Kalamazoo Promise did have some differential effects on student achievement and behavior even in the first full post-Promise year, which is 2006–07. These differential effects on Promise-eligible students are most convincing for increasing the probability of earning any credits and for reducing out-of-school suspensions—and, mainly for the African American students, for an increase in GPA. There is less convincing evidence that the Promise may have increased GPA in the full sample.

Our results relate directly to the body of work trying to understand the incentives in urban education. In his work on incentivizing students in urban schools, Fryer (2011) concludes that, in general, paying for inputs tends to give better results than conditioning rewards on student output. These findings are consistent with students not fully understanding the education production mapping between inputs and achievement. Specifically, Fryer found that rewarding works best when the students perceive that they can exert control over the input.

Our findings indirectly support Fryer’s notion. It is possible that students simply do not know what inputs map directly into a higher GPA, but that they understand that the opportunities given by the Promise are dependent on displaying better behavior in school. Thus, the relevant margin along which the students react could be that of altering their behavior so that fewer days are spent in out-of-school suspension.
If this hypothesis is correct, our findings suggest that Promise-style policies, and other policies focused on making higher education more affordable, may be usefully supplemented by helping students better understand how their behavior affects their future. Subsidies for higher education may make a greater difference to student achievement and behavior if students understand better the link between their behavior and work habits and their GPA, and the link between their GPA and the future rewards offered by the Promise.

### 6 Conclusion

This paper uses the large change in expected college tuition costs induced by the surprise announcement of the Kalamazoo Promise’s tuition subsidies to estimate the Promise’s causal effects on student achievement and behavior. The structure of the Kalamazoo Promise benefit formula creates a quasi-experiment for evaluating the impact of the scholarship on Promise-eligible students. We find positive effects for credits earned and a decrease in days spent in suspension.

Our results suggest that universal scholarships can be effective in incentivizing students to exert effort, by improving their behavior at school. Our results lead us to speculate about ways of strengthening the effects of Promise-type tuition scholarships and other policies to make postsecondary education more affordable. If students in urban school districts do not completely understand their education production function, the incentives provided by a universal scholarship such as the Kalamazoo Promise might lead them to react by improving their behavior but not necessarily by taking actions, such as doing more homework, that will directly lead to a higher GPA. One possible future role for school policies could be to help students better understand the link between their student work effort and achievement and future returns to education.

As mentioned before, our paper focuses on short-run effects in the Kalamazoo Promise. Promise-caused trends may have increased further in subsequent years. Certainly, school
administrators and the Kalamazoo community have been trying to help more students access the Promise. We hope in future work to analyze these subsequent effects.
References

*Southern Economic Journal*, 63(1): 36-50


Results

Figure 1: KPS Enrollment, by Year

Note: Data come from KPS, and are based on state count. Numbers include special ed., alternative ed., and students at Kalamazoo Area Math and Science Center. These are total student counts, not full-time equivalent counts. Numbers exclude out-of-district Education for Employment (vocational education) students, adult ed., CTEP (pre-school) and Head Start.

Figure 2: New Student Entrants to KPS in Fall of Recent School Years, Grades 1 to 12

Note: Data come from micro student data from KPS. A new entrant is a student enrolled in KPS as of the fall count day of a particular school year who was not enrolled as of the fall count day of the previous school year.
Figure 3: Map of Kalamazoo Public School District, Kalamazoo City, and Surrounding Townships

Figure 4: Number of KPS Students in Various Ethnic Groups, 1987 to 2009

Notes: These data were provided by KPS. The reflect raw counts of students, not FTE counts. These numbers include special ed, alternative ed, and students at the Kalamazoo Area Math and Science Center, a selective half-day program for high school students that serves a countywide student population, but is administered by KPS. These KAMSC numbers include students whose "home school" is not a KPS school. The numbers exclude out-of-district Education for Employment (Kalamazoo County's vocational education program) students. The inclusion of all KAMSC students probably increases the percentage of white and Asian American students but is probably roughly a constant factor over time. KPS classifies ethnic groups in mutually exclusive categories, so that Hispanic is used as a separate ethnic category. Therefore, in terms of official government definitions, the categories for all the other groups are explicitly "black non-Hispanic," "white non-Hispanic," etc. For details, see Bartik, Eberts, and Huang (2010).
**Figure 5:** Generosity of the Kalamazoo Promise Scholarship by Grade of Enrollment

![Graph showing the generosity of the Kalamazoo Promise Scholarship by grade of enrollment.](image)

**Table 1:** Present Value of the Kalamazoo Promise for Graduates of KPS

<table>
<thead>
<tr>
<th>Tuition Subsidy Group</th>
<th>Present Value (1)</th>
<th>Present Value (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0%</td>
<td>$0.0</td>
<td>$0.0</td>
</tr>
<tr>
<td>65%</td>
<td>$17,818.2</td>
<td>$21,839.0</td>
</tr>
<tr>
<td>70%</td>
<td>$19,188.9</td>
<td>$23,518.9</td>
</tr>
<tr>
<td>75%</td>
<td>$20,559.5</td>
<td>$25,198.9</td>
</tr>
<tr>
<td>80%</td>
<td>$21,930.1</td>
<td>$26,878.8</td>
</tr>
<tr>
<td>85%</td>
<td>$23,300.8</td>
<td>$28,558.7</td>
</tr>
<tr>
<td>90%</td>
<td>$24,671.4</td>
<td>$30,396.6</td>
</tr>
<tr>
<td>95%</td>
<td>$26,042.0</td>
<td>$31,918.6</td>
</tr>
<tr>
<td>100%</td>
<td>$27,412.7</td>
<td>$33,598.5</td>
</tr>
</tbody>
</table>

Note: we assume a 4.7 percent discount rate (we use this number from a study of parents’ discount rate for investing in children’s health – a proxy for quality; see Agee and Crocker, 1996), a 7 percent annual increase in tuition costs for 4-year universities and a 4 percent increase for community colleges. In column (1), we fix the probability of going to a community college at 0.45 and to a 4-year university at 0.55. We base these percentages on enrollment numbers in 2006-07 of the first cohort of Kalamazoo Promise recipients. In column (2), we change the probability of going to a community college to 0.3 and to a 4-year university to 0.7. We assume the tuition cost of community colleges equal to $2,385 per year (15 credits).\(^\text{18}\) Within the universe of 4-year universities, we assume that 13 percent attend University of Michigan at an annual cost of $13,437, 21 percent attend Michigan State University at $12,769, and 66 percent attend Western Michigan University at $10,140.\(^\text{19}\)

---

\(^{18}\) Based on the 2011-2012 tuition costs for KVCC: [http://www.michiganccc.net/data/​​tuition](http://www.michiganccc.net/data/tuition)

\(^{19}\) Based on Michigan State Notes: [http://www.senate.michigan.gov/sfa/Publications/Notes/2011Notes/NotesSum11bb2.pdf](http://www.senate.michigan.gov/sfa/Publications/Notes/2011Notes/NotesSum11bb2.pdf)
### Table 2: Trends in Kalamazoo Promise Scholarship Use

<table>
<thead>
<tr>
<th></th>
<th>2006</th>
<th>2007</th>
<th>2008</th>
<th>2009</th>
</tr>
</thead>
<tbody>
<tr>
<td>KPS graduates</td>
<td>518</td>
<td>579</td>
<td>550</td>
<td>535</td>
</tr>
<tr>
<td>Eligible for Promise</td>
<td>410</td>
<td>502</td>
<td>476</td>
<td>474</td>
</tr>
<tr>
<td>% of graduates eligible</td>
<td>79</td>
<td>87</td>
<td>87</td>
<td>89</td>
</tr>
<tr>
<td>Have used Promise</td>
<td>347</td>
<td>419</td>
<td>406</td>
<td>389</td>
</tr>
<tr>
<td>% eligible who have used Promise at any time</td>
<td>85</td>
<td>83</td>
<td>85</td>
<td>82</td>
</tr>
</tbody>
</table>

Source: Kalamazoo Promise.

### Table 3: Promise Eligibility Summary

<table>
<thead>
<tr>
<th>Class</th>
<th>0%</th>
<th>65%</th>
<th>70%</th>
<th>75%</th>
<th>80%</th>
<th>85%</th>
<th>90%</th>
<th>95%</th>
<th>100%</th>
<th>Grand Total</th>
<th>% Eligible</th>
<th>100%</th>
</tr>
</thead>
<tbody>
<tr>
<td>2006</td>
<td>108</td>
<td>45</td>
<td>25</td>
<td>17</td>
<td>18</td>
<td>16</td>
<td>9</td>
<td>40</td>
<td>238</td>
<td>518</td>
<td>79%</td>
<td>46%</td>
</tr>
<tr>
<td>2007</td>
<td>77</td>
<td>57</td>
<td>39</td>
<td>30</td>
<td>24</td>
<td>21</td>
<td>16</td>
<td>38</td>
<td>277</td>
<td>579</td>
<td>87%</td>
<td>48%</td>
</tr>
<tr>
<td>2008</td>
<td>74</td>
<td>50</td>
<td>15</td>
<td>19</td>
<td>16</td>
<td>8</td>
<td>23</td>
<td>48</td>
<td>297</td>
<td>550</td>
<td>87%</td>
<td>54%</td>
</tr>
<tr>
<td>2009</td>
<td>61</td>
<td>43</td>
<td>15</td>
<td>24</td>
<td>17</td>
<td>24</td>
<td>23</td>
<td>60</td>
<td>268</td>
<td>535</td>
<td>89%</td>
<td>50%</td>
</tr>
<tr>
<td>2010</td>
<td>75</td>
<td>74</td>
<td>7</td>
<td>23</td>
<td>22</td>
<td>17</td>
<td>24</td>
<td>59</td>
<td>248</td>
<td>549</td>
<td>86%</td>
<td>45%</td>
</tr>
<tr>
<td>Grand Total</td>
<td>395</td>
<td>263</td>
<td>102</td>
<td>113</td>
<td>97</td>
<td>86</td>
<td>95</td>
<td>245</td>
<td>1328</td>
<td>2731</td>
<td>86%</td>
<td>49%</td>
</tr>
</tbody>
</table>

Source: Kalamazoo Promise
**Figure 6: Structure of the Data Set**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9</td>
<td>10</td>
<td>11</td>
<td>12</td>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>7</td>
<td>8</td>
<td>9</td>
<td>10</td>
<td>11</td>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8</td>
<td>9</td>
<td>10</td>
<td>11</td>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>9</td>
<td>10</td>
<td>11</td>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10</td>
<td>11</td>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>11</td>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

High-lighted green and beige are students that could be eligible for the Promise:
- School years in which demographics and outcome variables are available
- School years in which enrollment history data is available
- Regression sample
Table 4: Summary Statistics. Means (standard deviations in parenthesis), before and after the Promise, by eligibility for the Promise (no benefit versus 65 percent or more)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No Benefit</td>
<td>Benefit &gt; 0</td>
</tr>
<tr>
<td>Demographic characteristics</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.50</td>
<td>0.48</td>
</tr>
<tr>
<td>Free/reduced price lunch</td>
<td>0.60</td>
<td>0.49</td>
</tr>
<tr>
<td>White</td>
<td>0.36</td>
<td>0.45</td>
</tr>
<tr>
<td>Black</td>
<td>0.51</td>
<td>0.46</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.10</td>
<td>0.07</td>
</tr>
<tr>
<td>Outcome variables</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Suspended (0/1)</td>
<td>0.20</td>
<td>0.22</td>
</tr>
<tr>
<td>Days suspended</td>
<td>1.12</td>
<td>1.73</td>
</tr>
<tr>
<td></td>
<td>(3.39)</td>
<td>(9.50)</td>
</tr>
<tr>
<td>In detention (0/1)</td>
<td>0.07</td>
<td>0.09</td>
</tr>
<tr>
<td>Credits earned (0/1)</td>
<td>0.87</td>
<td>0.96</td>
</tr>
<tr>
<td>Credits earned</td>
<td>4.62</td>
<td>6.12</td>
</tr>
<tr>
<td></td>
<td>(3.23)</td>
<td>(2.63)</td>
</tr>
<tr>
<td>GPA</td>
<td>1.57</td>
<td>2.15</td>
</tr>
<tr>
<td></td>
<td>(1.22)</td>
<td>(1.21)</td>
</tr>
<tr>
<td>Enrolled in AP class (0/1)</td>
<td>0.03</td>
<td>0.07</td>
</tr>
<tr>
<td>Grade</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Grade 9</td>
<td>0.19</td>
<td>0.40</td>
</tr>
<tr>
<td>Grade 10</td>
<td>0.30</td>
<td>0.25</td>
</tr>
<tr>
<td>Grade 11</td>
<td>0.25</td>
<td>0.19</td>
</tr>
<tr>
<td>Grade 12</td>
<td>0.26</td>
<td>0.17</td>
</tr>
<tr>
<td>Benefit</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Benefit = 0</td>
<td>1.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Benefit = 65</td>
<td>0.00</td>
<td>0.15</td>
</tr>
<tr>
<td>Benefit = 70</td>
<td>0.00</td>
<td>0.06</td>
</tr>
<tr>
<td>Benefit = 75</td>
<td>0.00</td>
<td>0.06</td>
</tr>
<tr>
<td>Benefit = 80+</td>
<td>0.00</td>
<td>0.73</td>
</tr>
</tbody>
</table>

Number of observations: 786, 5,226, 724, 7,693

Note: Most students taking an AP course sign up for 2 credits during a school year, for both the fall and spring, out of 8 possible credits. “Days suspended” is days of out-of-school suspension during the school year. GPA average is computed on the 4-point scale (A=4.0, B=3.0, C=2.0, D=1.0, F=0). The number of observations is the number of student-year cells used in computing the above statistics. Source: KPS.
### Table 5: Estimated effect of the Kalamazoo Promise on academic achievement

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) OLS GPA</th>
<th>(2) FE GPA</th>
<th>(3) OLS Credits Earned</th>
<th>(4) FE Credits Earned</th>
<th>(5) OLS Credits Earned (0/1)</th>
<th>(6) FE Credits Earned (0/1)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction terms: $\gamma_{tb}$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2003-04 × Benefit &gt; 0</td>
<td>0.0779</td>
<td>-0.0675</td>
<td>0.812***</td>
<td>0.172</td>
<td>0.0492**</td>
<td>-0.00978</td>
</tr>
<tr>
<td></td>
<td>(0.964)</td>
<td>(-0.913)</td>
<td>(3.707)</td>
<td>(0.681)</td>
<td>(2.099)</td>
<td>(-0.331)</td>
</tr>
<tr>
<td>2004-05 × Benefit &gt; 0</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>2005-06 × Benefit &gt; 0</td>
<td>0.0450</td>
<td>0.0584</td>
<td>-0.239</td>
<td>-0.284</td>
<td>0.00243</td>
<td>-0.00987</td>
</tr>
<tr>
<td></td>
<td>(0.496)</td>
<td>(0.750)</td>
<td>(-0.955)</td>
<td>(-1.083)</td>
<td>(0.0998)</td>
<td>(-0.436)</td>
</tr>
<tr>
<td>2006-07 × Benefit &gt; 0</td>
<td>-0.159</td>
<td>0.133</td>
<td>-0.437</td>
<td>-0.0830</td>
<td>-0.00278</td>
<td>0.0331</td>
</tr>
<tr>
<td></td>
<td>(-1.428)</td>
<td>(1.315)</td>
<td>(-1.418)</td>
<td>(-1.083)</td>
<td>(-0.8080)</td>
<td>(0.949)</td>
</tr>
<tr>
<td>2007-08 × Benefit &gt; 0</td>
<td>-0.330**</td>
<td>0.205</td>
<td>-0.466</td>
<td>0.587</td>
<td>-0.000759</td>
<td>0.0879*</td>
</tr>
<tr>
<td></td>
<td>(-2.526)</td>
<td>(1.274)</td>
<td>(-1.278)</td>
<td>(1.293)</td>
<td>(-0.0197)</td>
<td>(1.819)</td>
</tr>
<tr>
<td>Constant</td>
<td>2.042***</td>
<td>2.075***</td>
<td>4.637***</td>
<td>5.514***</td>
<td>0.860***</td>
<td>0.879***</td>
</tr>
<tr>
<td></td>
<td>(18.87)</td>
<td>(102.7)</td>
<td>(20.36)</td>
<td>(83.44)</td>
<td>(40.18)</td>
<td>(111.8)</td>
</tr>
<tr>
<td>Observations (NT)</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
</tr>
<tr>
<td>Observations (N)</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.298</td>
<td>0.019</td>
<td>0.196</td>
<td>0.044</td>
<td>0.059</td>
<td>0.077</td>
</tr>
<tr>
<td>Effect size 2005-06</td>
<td>0.0354</td>
<td>0.0459</td>
<td>-0.0861</td>
<td>-0.103</td>
<td>0.0103</td>
<td>-0.0418</td>
</tr>
<tr>
<td>Effect size 2006-07</td>
<td>-0.125</td>
<td>0.104</td>
<td>-0.158</td>
<td>-0.0300</td>
<td>-0.0118</td>
<td>0.140</td>
</tr>
<tr>
<td>Effect size 2007-08</td>
<td>-0.259</td>
<td>0.161</td>
<td>-0.168</td>
<td>0.212</td>
<td>-0.00322</td>
<td>0.373</td>
</tr>
</tbody>
</table>

Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Note: Regressions include the following controls: female, free or reduced price lunch, white, black, Hispanic, grade level (9-12), indicator for whether the student is new enrollee, an indicator for whether the student has had a change in the eligibility level over time, and a full set of interactions between school years (2003-04, 2005-06, 2006-07, and 2007-08) and Promise eligibility dummy (Benefit > 0) For the regressors of interest, the benchmark category is the school year 2004-05 and eligibility level equal to zero. Hence, for the positive eligibility level, the estimate is the difference in the outcome variable over time (from 2004-05 to 2007-08) relative to the same change in the zero eligibility group (control). The effect size is calculated by dividing the coefficient from the each regression by the standard deviation of dependent variable in the control year (school year 2004-05). Universe: Students enrolled in KPS in grades 9-12 during school years 2003-2004 trough 2007-2008 subject to sample restrictions, see the text for details. Source: KPS
Table 6: Estimated effect of the Kalamazoo Promise on student behavior

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) OLS Suspended (0/1)</th>
<th>(2) FE Suspended (0/1)</th>
<th>(3) OLS Suspended Days</th>
<th>(4) FE Suspended Days</th>
<th>(5) OLS In detention (0/1)</th>
<th>(6) FE In detention (0/1)</th>
<th>(7) OLS Days in detention</th>
<th>(8) FE Days in detention</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction terms: $\gamma_{tb}$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2003-04 × Benefit &gt; 0</td>
<td>0.00894</td>
<td>0.00950</td>
<td>-0.134</td>
<td>-0.0117</td>
<td>0.0259</td>
<td>-0.00258</td>
<td>0.0613</td>
<td>0.0825</td>
</tr>
<tr>
<td></td>
<td>(0.313)</td>
<td>(0.226)</td>
<td>(-0.396)</td>
<td>(-0.0277)</td>
<td>(1.342)</td>
<td>(-0.0917)</td>
<td>(1.026)</td>
<td>(0.873)</td>
</tr>
<tr>
<td>2004-05 × Benefit &gt; 0</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>2005-06 × Benefit &gt; 0</td>
<td>0.00786</td>
<td>-0.00969</td>
<td>0.369</td>
<td>-0.357</td>
<td>-0.00712</td>
<td>-0.0157</td>
<td>0.0466</td>
<td>-0.0411</td>
</tr>
<tr>
<td></td>
<td>(0.260)</td>
<td>(-0.253)</td>
<td>(0.914)</td>
<td>(-0.796)</td>
<td>(-0.298)</td>
<td>(-0.521)</td>
<td>(0.593)</td>
<td>(-0.370)</td>
</tr>
<tr>
<td>2006-07 × Benefit &gt; 0</td>
<td>0.0249</td>
<td>-0.0215</td>
<td>-0.115</td>
<td>-1.296**</td>
<td>0.0542**</td>
<td>0.00936</td>
<td>0.0933</td>
<td>-0.0671</td>
</tr>
<tr>
<td></td>
<td>(0.693)</td>
<td>(-0.427)</td>
<td>(-2.325)</td>
<td>(-2.396)</td>
<td>(2.147)</td>
<td>(0.252)</td>
<td>(1.218)</td>
<td>(-0.588)</td>
</tr>
<tr>
<td>2007-08 × Benefit &gt; 0</td>
<td>0.0378</td>
<td>-0.0579</td>
<td>-0.502</td>
<td>-1.796***</td>
<td>-0.00185</td>
<td>-0.0207</td>
<td>-0.0687</td>
<td>-0.179</td>
</tr>
<tr>
<td></td>
<td>(1.034)</td>
<td>(-0.924)</td>
<td>(-1.036)</td>
<td>(-2.833)</td>
<td>(-0.0675)</td>
<td>(-0.468)</td>
<td>(-0.938)</td>
<td>(-1.379)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.207***</td>
<td>0.210***</td>
<td>1.772***</td>
<td>1.521***</td>
<td>0.122***</td>
<td>0.106***</td>
<td>0.245***</td>
<td>0.197***</td>
</tr>
<tr>
<td>Observations (NT)</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
<td>14,429</td>
</tr>
<tr>
<td>Observations (N)</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
<td>6,618</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.149</td>
<td>0.056</td>
<td>0.047</td>
<td>0.023</td>
<td>0.090</td>
<td>0.042</td>
<td>0.056</td>
<td>0.037</td>
</tr>
<tr>
<td>Effect size 2005-06</td>
<td>0.0187</td>
<td>-0.0230</td>
<td>0.0310</td>
<td>-0.0301</td>
<td>-0.0231</td>
<td>-0.0509</td>
<td>0.0426</td>
<td>-0.0376</td>
</tr>
<tr>
<td>Effect size 2006-07</td>
<td>0.0593</td>
<td>-0.0510</td>
<td>-0.00965</td>
<td>-0.109</td>
<td>0.176</td>
<td>0.0304</td>
<td>0.0853</td>
<td>-0.0613</td>
</tr>
<tr>
<td>Effect size 2007-08</td>
<td>0.0899</td>
<td>-0.138</td>
<td>-0.0423</td>
<td>-0.151</td>
<td>-0.00602</td>
<td>-0.0671</td>
<td>-0.0628</td>
<td>-0.164</td>
</tr>
</tbody>
</table>

Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Note: Same as in Table 4. Universe: Students enrolled in KPS in grades 9-12 during school years 2003-2004 through 2007-2008 subject to sample restrictions, see the text for details. Source: KPS
**Figure 7:** Estimated effect (fixed effects) of the Kalamazoo Promise (KP) on academic achievement

Panel A

**Estimated effect of KP on GPA**

Panel B

**Estimated effect of KP on credits earned**

Panel C

**Estimated effect of KP on credits earned dummy**

Note: Note The Kalamazoo Promise was announced on November 10, 2005 (school year 2005-06). Panels A-C use the same specification as fixed effects regressions in Table 4. Dots around estimates indicate statistical significance at 10 percent level, p<0.10.
Figure 8: Estimated effect (fixed effects) of the Kalamazoo Promise (KP) on student behavior

Panel A

Panel B

Panel C

Panel D

Note: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005-06). Panels A-D use the same specification as fixed effects regressions in Table 5. Dots around estimates indicate statistical significance at 10 percent level, p<0.10.
Figure 9: Estimated effect (fixed effects) of the Kalamazoo Promise (KP) on outcomes. Robustness checks for selected results.

Panel A

![Estimated effect of KP on credits earned dummy](image)

Note: dots indicate statistical significance at 10 percent level, p<0.10

Panel B

![Estimated effect of KP on days in suspension](image)

Note: dots indicate statistical significance at 10 percent level, p<0.10

Note: Note The Kalamazoo Promise was announced on November 10, 2005 (school year 2005-06). In both specifications we drop the all the new enrollees since (and including 2005-06) and all of those who changes their eligibility level after and including 2005-06. This sample consists of 13,392 observations.
**Figure 10:** Estimated effect (fixed effects) of the Kalamazoo Promise (KP) on outcomes. Selected results for the subsample of African-American students only.

Panel A  Panel B

Panel C  Panel D

Note: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005-06). This specification includes only African American students. Additionally, we drop the all the new enrollees since 2005-06 (including that year) and all of those who changes their eligibility level after and including 2005-06. This sample consists of 6,385 observations.
Appendix

**Figure A1:** Trends in demographics -- averages with 95 percent confidence intervals. Computed on the same sample as in Table 3 (14,429 observations).

Note: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005-06).
Figure A2: Estimated effect (fixed effects) of the Kalamazoo Promise on student outcomes
Results omitted results from Table 4.

Panel A

Panel B

Panel C

Note: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005-06).
Figure A3: Trends in selected average estimated student fixed effects.\textsuperscript{20}

Panel A: GPA

Panel B: Credits earned

\textsuperscript{20}Note: the individual student fixed effects were obtained residually for each regression using the \texttt{areg} command in Stata and averaged across the school years.
Panel C: Credits earned (0/1)

Panel D: Days suspended
Panel E: Days in detention

Average Fixed Effects by Benefit and School Years

![Graph showing average fixed effects by benefit and school years.](image)

Dependent variable: Days in detention