The Short-Term Effects of the Kalamazoo Promise Scholarship on Student Outcomes[1]

Timothy J. Bartik[2] and Marta Lachowska[3]

December 7, 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 also control for student fixed effects. 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 the following persons and groups for their comments and suggestions: Susan Dynarski, Douglas Harris, Susan Houseman, Caroline Hoxby, Brian Jacob, Lars Lefgren, the participants at various conferences where we have presented this paper (PromiseNet, AEFP, MEA, SOLE, APPAM, and the NBER Economics of Education Program Meeting), two anonymous referees, and Solomon Polachek. We have benefited from our discussions with Michael Rice. We thank Ben Jones and Allison Colosky for their 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.

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 12,000 students) with a racially and economically diverse student population. Anonymous donors promised to pay up to 100 percent of college tuition for any KPS 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 on, and gradually increase to 100 percent for students attending since kindergarten. The scholarship does not require any minimum high school grade point average (GPA) or financial need. Students must simply get into college and maintain a 2.0 college GPA. In sum, the Kalamazoo Promise is unusual among scholarship programs in its universality and generosity.

The Promise, as it is called, has attracted much attention and many imitators. In 2008, the *Economist* ran a piece on the scholarship, "Rescuing Kalamazoo: A Promising Future" (*Economist* 2008). In part because of the Promise, in 2010, President Obama gave the commencement address 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, with private or 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

[4] See http://www.upjohn.org/promise/promisescholarships.html for a list of such programs (accessed August 17, 2012).

otherwise would have attended the local community college may use the subsidy to attend WMU.[5] Students who without the Promise might not have attended college may use the subsidy to go to a community college. Admission to and graduation from more demanding colleges requires students to have better academic performance. Despite the Promise's incentives for better academic performance, the magnitude of student responses to such incentives is doubtful, for several reasons. Many students may view the Promise's benefits as too uncertain and too delayed. Even if students want to respond to the Promise by improving their academic performance, students may not know how academic performance can best be improved. Therefore, there is a need for rigorous research to determine the magnitude of positive benefits of Promise-style programs, or indeed, whether there are discernible benefits.[6]

Our paper estimates the effects of the Kalamazoo Promise on student achievement and behavior. We use one aspect of the Promise that bears resemblance to a "quasi-experiment." The surprise 2005 announcement of the Kalamazoo Promise created a large change in college tuition costs that varied across different groups of students based on prior enrollment decisions. The morning after the Promise was announced, some KPS students 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 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

[5] For empirical evidence of such a shift in the choice set of colleges in the years following the adoption of the Promise, see Andrews, DesJardins, and Ranchhod (2010).

[6] As we will briefly mention below, a skeptic could even argue that the Promise might reduce incentives for academic performance for some students by reducing the need to depend on merit scholarships for college tuition. We disagree with this skeptical argument, because we doubt whether most students understand how their behavior and decisions translate into eligibility for merit scholarships.

changes in student achievement and behavior that are statistically linked to such exogenous tuition changes can be interpreted as program effects. 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 procedure controls for unobserved differences between students who started their enrollment in KPS at different grades. We also control for student fixed effects. As we will explain, this accounting for student fixed effects controls for changes in group composition that are due to differential out-migration of different groups from the district after the Promise.

Our analysis finds that the Kalamazoo Promise has statistically and substantively significant effects on improving student achievement levels and behavior. For the overall sample, we estimate a decrease in the number of days spent in suspension by one or two days per school year, which is large compared to sample means and standard deviations. For the overall sample, we do not find effects on high school GPA. We speculate that when confronted with incentives generated by the Promise, students are more likely to react along a margin that they perceive that they can control, such as improving their behavior. On the other hand, for African American students, we estimate a dramatic increase in GPA, ranging from about 0.17 of a standard deviation to about 0.60. For these students, whose baseline achievement and behavior indicators lag behind those of white students, the decrease in the number of days spent in suspension appears to spill over into a higher GPA. We speculate that this could be due to the number of days in suspension exceeding a "tipping point" beyond which GPA increases by virtue of students being present in the classroom for some critical number of days. Finally, the estimated positive effects of the Promise 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. The following section discusses related previous research literature. The next section provides further background information on the KPS district and the Kalamazoo Promise. We then describe the data we use, our econometric models, and our results. The final section offers conclusions.

Related Literature

Research relevant to this paper includes studies of how college costs or other financial incentives affect student achievement and behavior. Other relevant research focuses on how the Promise has affected students, the school district, and the Kalamazoo area.

The Kalamazoo Promise offers a generous college tuition subsidy with minimal requirements. As described in the previous section, the Kalamazoo Promise may relax the financial constraints of going to a more selective college. This creates incentives for greater academic effort. However, the Promise's incentives may be somewhat similar to the incentives created by many states' merit aid programs for college. As described by Dynarski (2004), these state merit aid programs, which have become increasingly prevalent, often have modest high school achievement requirements.[7] The Promise is at an extreme in terms of its broad eligibility. However, qualifying for many of these state merit aid programs is not unduly difficult. It is unclear to what degree these differences between the Promise and state merit aid programs will matter for student behavior and achievement. Therefore, it is of interest to see how these

[7] Dynarski (2004) compares the eligibility requirements of several state merit aid scholarships: the majority of such programs require a high school GPA of between 2.5 and 3.0; some additionally require ACT or SAT scores of a certain level, or demonstration of financial need, or both. Similar to the Kalamazoo Promise, most of these scholarships require maintaining a certain minimum GPA while in college.

somewhat different college tuition subsidies affect high school behavior and achievement. These previous studies provide a context for considering our Promise estimates.

Among state merit aid programs, Georgia HOPE (Helping Outstanding Pupils Educationally) is the largest program and has been much studied. Georgia HOPE increases college enrollment and shifts college choices toward eligible in-state colleges (see, for example, Dynarski [2002, 2004] and Cornwell, Mustard, and Sridhar [2006]). Of more relevance for the current paper, Georgia HOPE increases academic performance in high school (Henry and Rubenstein 2002), especially among African American students. However, Georgia HOPE may also have some unintended consequences. For example, Georgia HOPE has led to decreased course loads in college and increased course withdrawals (see Cornwell**,** Lee, and Mustard [2005]), which may reflect the program's requirement that recipients maintain a minimum college GPA.

Researchers have also studied other state merit-based scholarships. For example, Kane finds increased college enrollment resulting from both the D.C. Tuition Assistance Grant program (Kane 2006) and California's Cal Grant program (Kane 2003). Pallais (2007) finds that the Tennessee Education Lottery Scholarship (TELS) affected students' college choices. Of particular relevance for our paper, Pallais also finds that TELS improves student achievement in high school. Scott-Clayton (2010) studies the effects of West Virginia's merit aid program (called the West Virginia Promise) and finds an increase in the effort students put forth in college as well as a higher likelihood of completing a bachelor's degree within four years. Interestingly, the particular design of the West Virginia Promise prompts Scott-Clayton to conclude that the observed impacts are not solely due to a reduced cost-of-college effect, but also to an incentive effect.

Some recent studies have looked at 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 and find that the scholarships substantially increased performance. Another important study finding is that this scholarship has positive spillover benefits for nonscholarship students. For example, the program is estimated to increase academic achievement for boys, who are ineligible for this scholarship, and for girls with low odds of being scholarship winners.[8] As we will explain later, the possibility of spillover effects means we must be careful in interpreting our estimates as representing the total effects of the Kalamazoo Promise.

Other research has examined the effects of financial incentives in developed economies. This research often finds effects mainly for women or high-ability groups. Angrist and Lavy (2009) look at the effects of a cash rewards experiment on teenagers' graduating from Israeli high schools and find strong effects among high-ability women. Angrist, Lang, and Oreopoulos (2009) study the effects of merit-based scholarships on first-year undergraduates at a large Canadian university. They too find strong effects for women. In a similar study, Leuven, Oosterbeek, and van der Klaauw (2010) conduct a randomized experiment among first-year undergraduates in the Netherlands. The experiment provides 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.

[8] Dhiraj Sharma (2010) studies the impact of a randomized cash rewards program among Nepalese eighth-graders and finds that the financial impact of these incentives equaled about a 0.09 standard deviation gain in aggregate scores. A related strand of research 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 more likely to have finished eighth grade and to have scored higher on achievement tests.

The aforementioned studies deal with incentives related to academic output—performance on tests, grades, or fulfilling certain requirements. Standard agency theory suggests that if we want to incentivize a student to exert effort, and we do not observe the resultant effort perfectly, the optimal contract should be conditional on output. Jackson (2010) studies a program in Texas that paid students cash for attaining certain grades on Advanced Placement tests in high school. Using differences in the timing of the adoption of this program, Jackson finds effects on measures of achievement, as predicted by contract theory. However, the prediction of a simple agency model fails if students do not understand the mapping between educational inputs and outputs. Fryer (2011) has studied this issue in experiments on what incentives work best in urban schools. Based on randomized experiments 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).[9]

In the case of the Promise, as mentioned above, the program provides some incentive for students to improve high school behavior and achievement in order to be admitted to and succeed at more selective postsecondary institutions. However, as Fryer's (2011) work underlines, students may not fully understand what behavior needs to change or how to alter it. In addition, from the perspective of the students, the tuition subsidies of the Promise might be too delayed and too uncertain.[10] For example, Levitt et al. (2012) argue that financial incentives are less potent if they are handed out with a delay.

[9] Fryer (2011) 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.
[10] The anonymous donors have stated their intention for the program to continue indefinitely and have guaranteed that if the program ever ends, all students enrolled in KPS at that time would receive the scholarship. However, we cannot rule out that students are still uncertain about receiving the Promise.

A skeptic could also argue that the Promise may reduce the incentive for students to work hard to obtain merit-based scholarships. A theoretical argument could also be made that the Promise does not provide additional incentives for low-income students to work hard, because the Promise may simply replace need-based aid. However, we doubt whether these potential negative incentive effects of the Promise are large for most students. Most students do not have a good understanding of how our current system of need-based and merit-based scholarships will affect their college costs.[11] The Promise makes a much more simple "promise" of tuition assistance. This simple "promise" is more likely to be understood by students than the current scholarship system. However, it remains an open question whether even this simple tuition assistance offer is sufficient to make large changes in behavior and academic achievement.

Although there have been no in-depth studies of whether the Promise has changed student behavior, other aspects of the Kalamazoo Promise program have been analyzed (see, for example, Miller-Adams [2009]). Bartik, Eberts, and Huang (2010) 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. Bartik, Eberts, and Huang also find evidence that since

[11] Such informational asymmetries in the context of federal need-based scholarships have been studied by Avery and Kane (2004), who find that qualified students often believed that they were not qualified for aid, and who report that students from disadvantaged families were deterred by the complexity of applying for financial aid. Complexities associated with applying for federal Pell Grants have been the focus of work by Dynarski and Scott-Clayton (2006) and of the experiment conducted by Bettinger et al. (2011). Both papers advocate simplification of the Free Application for Federal Student Aid (FAFSA) application procedure.

the establishment of the Promise, KPS test scores have increased somewhat faster than in similar Michigan school districts.

These results are further corroborated by Miller (2010), who also looks at 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 housing prices, but does find 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, admission to which will require higher student achievement during high school. This paper also suggests that the response might be concentrated among marginal students.

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

Kalamazoo Public Schools is a midsized, predominantly urban school system. As Figures 1 and 2 show, 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 (although also including some nearby suburban and rural areas) that has more 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.

Even before the Promise, the Kalamazoo school district had many low-income students and many students from diverse ethnic backgrounds. Figure 3 shows trends in the number of black, Hispanic, and non-Hispanic white students in the district. As can be seen in the figure, in the years before the Promise, although KPS retained a considerable percentage of white students (as well as students who did not qualify for free and reduced price lunches, not shown), the percentage of such students was clearly falling. Since the advent of the Kalamazoo Promise, enrollment in KPS has been on the rise. Furthermore, enrollment seems to be up proportionately for all ethnic groups, so the ethnic percentages have stabilized. These patterns are consistent with a Promise effect.

The Kalamazoo Promise

According to information provided by the school district, the anonymous donors believe that the Promise's purposes are threefold: 1) to promote local economic and community development, in part by attracting parents and businesses to the Kalamazoo area; 2) to boost educational achievement and attainment; and 3) to help increase confidence in KPS.

The Kalamazoo Promise is available to all students who graduate from KPS, reside in the district, and have been KPS students for four years or longer.[12] 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

---

[12] This information comes from the Kalamazoo Promise Web site: http://kalamazoopromise.com/uploaded/Promise%20Senior%20Information%20Brochure.pdf (accessed August 17, 2012).

high school graduation. The benefit is graduated, based on the length of attendance in the KPS system. Figure 4 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 and 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.

The 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. 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.

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, maintain any minimum GPA in high school, or take any particular mix of courses. However, students obviously need to be admitted to a college to receive Promise benefits.

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 (taking 12 credit hours per semester at a minimum) and 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 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 an associate or bachelor's degree.

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. Our calculations use information about the enrollment decisions of the first cohort of Kalamazoo Promise recipients. About 45 percent of new enrollees in 2006 attended a community college (almost all of them attended the local Kalamazoo Valley Community College, KVCC). 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 the University of Michigan (17). We assume that these college-going probabilities remain constant over time and across different tuition subsidy groups.[13] In Table 1, we calculate a present value of the Promise for different subsidy groups. Our calculations indicate that for someone eligible for a 100 percent tuition subsidy, the present value averages $27,413, while for someone who is eligible for a 65 percent subsidy, the present value averages $17,818. We also computed the present value of the 100 percent tuition subsidy version of the Promise at the most expensive college, the University of Michigan, and the present value of one of the cheaper options, KVCC. The present value of four years' tuition at the University of Michigan equals $55,545, whereas the cost of KVCC for two years is $4,731.

[13] 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, as mentioned in the table note. Since the likelihood of attending a four-year college correlates with family background, such weights might better reflect the preferences of high-income families. This reweighting increases the present value of the scholarship, holding other parameters constant. For an in-depth study of how the Kalamazoo Promise altered the college choice set across time and different income groups, see Andrews, DesJardins, and Ranchhod (2010).

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, 80–90 percent of KPS graduates have been eligible for at least some Promise benefits. Of those eligible, between 82 and 85 percent at some point have used Promise benefits.

There is wide variation in the Promise subsidy across KPS students. As shown in Table 3, among KPS graduates, the largest group is made up of 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).[14]

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, it allows the analysis to include some students who end up being ineligible for the Promise because they entered after ninth grade. Obviously, all students in earlier grades are 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 are more likely

[14] 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 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.

than students in earlier grades to believe that their achievement and behavior in school will affect their admission prospects at more selective colleges.

Our regression sample consists of ninth- through twelfth-graders from the school years 2003–2004 to 2007–2008. 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 go back to 1997–1998, we consistently track enrollment histories for everyone since sixth grade. 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. We have data on student characteristics, grade point averages, and disciplinary actions. The disciplinary data consist of information on days of suspension and detention.

We use our data to calculate for each student what his or her Promise subsidy would have been had the Promise been in effect for that year and had the student continued attending KPS until graduation. We call this the student's "virtual Promise benefit." Our hypothesis is that for every time period, students are forward-looking and adjust their 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 2003–2004 and 2004–2005, these "virtual Promise benefits" are virtual in the sense that the student was unaware of them, as the Promise was not announced until November 2005. Therefore, we would assume that any effect of this simulated Promise benefit in those years reflects effects that are associated with the grade level in which the student entered KPS, rather than the effect of a Promise benefit of which the student had no knowledge.

Including 2003–2004 as an additional control year allows us to see whether there are different trends for Promise-eligible versus ineligible groups during the pre-Promise years. If there are differences in pre-Promise trends between these groups, then there is reason to question whether any post-Promise differences between these groups are actually caused by 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 time before students fully understood and acted on the incentives of the Promise. 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 2006–2007 is a full post-Promise year. By fall 2006, students may have more fully understood what the Promise might mean for their future. Including the 2007–2008 school year adds a second full post-Promise year to help confirm effects estimated for the 2006–2007 year.

Restricting the analysis to these five years limits the extent to which other changes in KPS's policies and practices might differentially affect "length of enrollment" groups, which may differ in unobserved characteristics. Furthermore, 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.

By comparing changes in the behavior and achievement of Promise-eligible versus Promise-ineligible students, we are likely understating the overall effects of the Promise. It is plausible that even Promise-ineligible students are positively affected by the Promise. If Promise-eligible students improve their achievement and behavior, Promise-ineligible students will be positively affected by peer effects. In addition, the Promise triggered efforts by the school district to increase overall academic standards and college focus among all students. Because of the Promise, teachers and parents may have increased their educational expectations. Higher expectations may have spilled over into benefits for all students, whether Promise-eligible or ineligible. Our comparison of changes from before and after the Promise for Promise-eligible versus ineligible students will only capture the narrow effects of the Promise's monetary offer. This comparison will not fully capture the Promise's effects on overall school climate, through changes in attitudes and actions of administrators, teachers, and parents.

In our sample, we excluded students who moved into the school district at ninth grade after the Promise was announced. These students' families might well have moved into the district because of the Promise. Including such students might lead to Promise-related differences between different groups of students. We do include data for students who moved in at tenth grade or higher grades after the Promise announcement. These students were ineligible for the Promise, so the Promise's monetary benefits are unlikely to be directly motivating them. We also explore how estimates vary if we exclude all such in-migrants.

No data are available on post-KPS academic achievement or behavior for students who left the district. Therefore, we cannot include academic outcomes and behavior for student out-migrants after they leave KPS. There may be differences because of the Promise in the types of students leaving the district. Fully controlling for these differences is impossible in the absence of data on post-out-migration behavior and achievement. As a partial control, and as we will explain further below, our model includes controls for student fixed effects. These student fixed effects use each student as a control for that student's own behavioral and academic achievement tendencies. This minimizes statistical problems due to differences in unobserved characteristics among out-migrants from the district before and after the Promise announcement.

However, we do include achievement and behavior data while in the KPS district for students who end up leaving the district before graduating. These students end up not receiving the Promise. However, if they were potentially Promise-eligible at the time of their enrollment in the district, they are counted as Promise-eligible in our estimates. This definition of Promise eligibility is chosen by us because the decision to leave the district is endogenous, and if we defined the Promise eligibility variable based on students' decision to leave the district, our policy variable would depend on a choice of the student taken with full awareness of the existence of the scholarship (i.e., conditioning on an outcome). Indeed, part of the Promise effect may be due to inducing students to pass more credits, and behave better, and therefore to be less likely to drop out of school. We therefore chose to define the Promise eligibility variable in a way that is less subject to students' endogenous choices. However, this means that our estimates should be interpreted as "intention to treat" estimates and hence are likely to understate the Promise's effects on student achievement and behavior, relative to hypothetical effects on students who knew for certain that they would remain in the district.

We analyze a wide variety of dependent variables reflecting possible student responses to the Promise. The dependent variables include measures of whether students were suspended and days suspended from school, as well as whether students received in-school detention. We also look at various academic achievement measures, such as credits earned and grade point average.[15,16] Student behavior, such as suspensions or detentions, may be more straightforwardly

[15] We also did some estimates of effects on enrollment in Advanced Placement (AP) classes, but we do not find any significant effects, so these results are not presented in this paper. AP enrollment in the District in this time period is relatively small, generally less than 10 percent for most benefit groups either before or after the Promise.

and immediately affected by student choices than the academic achievement measures. Academic achievement could be argued to be the ultimate “bottom-line” measure of whether the program has been effective in improving student outcomes. However, policymakers may also care about whether the program affects “soft skills,” which may be reflected in student behavior. Furthermore, student behavior should eventually have effects on academic achievement. For all these reasons, both student behavior and academic achievement are of interest and are examined in this paper.

Table 4 presents descriptive statistics for the sample. We pooled the years together into “before” (2003–2004 and 2004–2005) and “after” (2005–2006 through 2007–2008) periods. We also 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 demographic differences between the groups that were eligible for some future tuition subsidy and those that were not. Before the announcement of the Promise, the recent enrollees—entitled to no future tuition subsidy—were more likely to be African American and beneficiaries of free and reduced-price lunches. These differences may in part reflect differential out-migration behavior as well as our exclusion of post-Promise ninth-grade in-migrants.

[16] Standardized achievement measures are not included because the state-required tests changed timing and format in the fall of 2005, just about the time when the Kalamazoo Promise was announced. Thus, we cannot control for pre-Promise trends in these achievement tests. As our empirical results on other variables will show, controlling for pre-Promise trends is important.

For our dependent variables on student behavior and achievement, low baseline levels of behavior and achievement leave plenty of room for improvement, whether because of the Promise or other influences. Prior to the Promise, over 20 percent of students received an out-of-school suspension each year. Average GPA levels were low, at around 2.0 (a C average). Average credits earned were around six credits out of the eight credits per year normally available under the district's block scheduling program. [17] Although these numbers are low, they do not appear to be unusual for urban school districts.[18]

Table 4 also emphasizes the need to include careful controls to uncover the true effects of the Promise, as well as possible contradictory results for different variables. For example, a simple post-Promise announcement comparison of GPA and credits earned between Promise-eligible and ineligible students suggests somewhat greater academic achievement by Promise-eligible students. However, the post-Promise behavior of Promise-eligible students, compared to ineligible students, is actually somewhat worse.

In addition, although academic achievement was somewhat higher post-Promise for Promise-eligible students, the trends appear worse when we look at pre-Promise data. Even prior to the Promise, students whose enrollment decisions would have made them eligible for the Promise had higher academic achievement. If anything, it appears as if the academic gap between eligible

[17] The district during this period used a four × four block schedule. Each student normally took four courses a semester. Each course was counted as if it were equivalent to a full year of a course under the previous six-period day. Students who are behind may participate in a credit-recovery system. Under this system, they can take course modules using a computer system, which allows these students, if they pass the exams, to accumulate more credits per year than the normal eight per school year. We top-coded the maximum number of credits earned at 12. This procedure affects 59 observations. It is also possible for students to fail all courses and earn zero credits in a given school year. This does necessarily imply dropping out, as we observe that about half of the students with zero credits earned in one school year come back for the following school year. About 7 percent of students report earning zero credits.

[18] The mean pre-Promise GPA in our sample is 2.07, averaged over both eligible and ineligible students. In Chicago Public Schools, the average ninth-grade GPA is around 1.7 or 1.8 (Lesnick et al. 2010, calculated using information on p. 23 and in Table 3 on page 13). The six credits earned in our pre-Promise sample is 75 percent of the normal course load of KPS. Chicago Public Schools reports pass rates for first-year high school students that have varied over the years from 74 percent to 80 percent (Miller, Allensworth, and Kochinek 2002).

students and ineligible students has narrowed from before the Promise to after the Promise. For behavior, the trend from before and after the Promise announcement shows some worsening of behavior in the Promise-eligible group relative to the ineligible group.

These patterns suggest that it will be difficult to find Promise effects by making simple comparisons of group differences in levels or trends of achievement or behavior. A variety of factors, such as differential out-migration, may confuse such comparisons. This justifies a more careful effort to fully control for unobserved influences on student achievement and behavior, which we do using student fixed effects.

As the Table shows, the data include a wide variety of grade levels, from ninth through twelfth. We might expect that different grades would have their own characteristic patterns of GPA, credits earned, and behavior. Therefore, it is important to control for grade differences. All of our statistical analysis includes controls for grade effects.

Table 4 shows that some ninth-graders are not eligible for the Promise. In those cases, the student had enrolled after the state's fall census date for schools, and according to 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.

As the data show, the overwhelming majority of students who are eligible for any benefit are eligible for a benefit of 80 percent or more of college tuition.[19] This suggests that it will be difficult to make fine distinctions for students receiving subsidies for different tuition subsidy groups. In addition, it is unclear whether the difference between a 65 percent subsidy and greater

[19] We cannot for all students tell whether their subsidy is 80 percent or some percentage greater than that, because our enrollment data only go back to 1997–1998.

subsidies is salient for most high school students. Our empirical work therefore focuses on differences in academic achievement and behavior between Promise-eligible and ineligible students, before and after the Promise for the same students.[20]

Methods

We measure the effects on student behavior and achievement of the Promise's monetary offer by comparing differences, from before and after the Promise, for Promise-eligible vs. ineligible students. This comparison controls for fixed effects for the year of the observation, for the grade, and for the student. Our key focus is on estimating the effect of the interaction between the year of the observation and the student's Promise-eligibility status. If we see a clear change or trend in this variable after the Promise, with no sign of a clear trend before the Promise, we regard this as good evidence of a Promise effect. We summarize this model in Equation (1):

$$y_{it} = \sum_t \delta_t T_t + \varphi I\{Benefit > 0\}_i + \sum_t \gamma_t \left(T_t \times I\{Benefit > 0\}_i\right) + x_{it}'\beta + F_i + G_{it} + u_{it} \qquad (1)$$

where y_{it} is a dependent variable showing student i's achievement or behavior at year t (GPA, credits earned, suspensions, detentions). The fixed effect T controls for the way the outcome variable of interest varies by year, where the t subscript indexes the year. The indicator function $I\{Benefit > 0\}$ equals 1 if the student i would be eligible for any tuition subsidy from the Promise scholarship (65 percent or more), given that he or she continues attending KPS until graduation. The interaction between the year effect and the Promise eligibility allows for unrestricted variation by year in how Promise eligibility is related to student achievement or behavior. The fixed effect F_i holds constant any fixed student characteristics, observed or unobserved, that

[20] We also did some examination of differential effects for different subsidy percentages, but did not find anything of interest.

influence student achievement or behavior.[21] x_{it} are time-varying student characteristics, such as free and reduced-price lunch status, which might influence academic achievement or behavior. (Time-invariant student characteristics, such as gender or race, are captured by the fixed effect for each student.) The grade effect (G_{it}) controls for effects of a given grade (ninth through twelfth) on average academic achievement or behavior. u_{it} is the disturbance term.[22]

The model is estimated via a regression in all cases. In some cases, y_{it} is a discrete zero-one variable (whether any credits earned that year, whether any suspensions that year, whether any detentions that year). In those cases, the model estimated is a linear probability model.

The model is estimated using data over all students in the sample for which descriptive statistics are provided in Table 4. This includes any student in KPS high schools at any point in the five years 2003–2004 through 2007–2008, except for those who moved in after the Promise

[21] Because we include a fixed effect for unobserved student characteristics, it might seem that we do not need to include the Promise eligibility variable by itself, as it is captured by the student fixed effects. However, some students' eligibility for the Promise changes because they move out of the District and then move in again, and Promise eligibility is based on the most recent date of continuous enrollment. We do include as a control a dummy variable for whether the student's Promise eligibility changed over time. In addition, as we will discuss later, we see how robust our results are to excluding such students from the estimation.

[22] We cluster the standard errors on the individual student to take into account effects that are correlated across years for the same individual. In our application, one might worry that the appropriate cluster is not the individual student but rather a more aggregate group, such as a school. Including individual fixed effects in the regression controls for some of the within-group error correlation, but this might not suffice, depending at which level one assumes the "between"-group correlation not to be problematic for inference (this level is usually unknown). This potential problem of grouped structure is referred to as the Moulton problem (Moulton 1986, 1990) and has recently received much attention in applications using difference-in-difference methods (see, e.g., Bertrand, Duflo, and Mullainathan [2004] and Angrist and Pischke [2009] for a textbook treatment). Bertrand, Duflo, and Mullainathan (2004) recommend clustering the standard errors on the level of aggregation where a policy change takes place. Donald and Lang (2007) suggest conducting the analysis on data aggregated to the group level and use a *t*-distribution for inference. In our case, with only six schools, and with a majority of students concentrated in only two, this approach becomes problematic. Conley and Taber (2011) focus on a case with few policy changes (as in our case) and a large number of comparison groups. This case is difficult to implement in our setting as, again, we have a small number of control clusters. Given these data limitations, we try to infer how sensitive our standard errors are to clustering at the class-school level. As expected, this typically inflates the standard errors, but usually not to the point where all the difference-in-difference coefficients of a given model lose their precision. To illustrate this, in our preferred fixed effects specification using days spent in suspension as the outcome variable, the cluster-robust standard error on the coefficient on the interaction term for the year 2006–2007 (coefficient equals −1.296) equals 0.577, whereas when clustering on the individual student it equals 0.541. However, the cluster-robust coefficient for the interaction term on the year 2007–2008 (equal to −1.796) loses its precision (the standard error increases from 0.634 to 1.13). Given the small number of more aggregate clusters, we choose to report the individual-cluster-robust standard errors in the tables. The results using the school-grade-cluster-robust results are available from the authors.

was announced and were eligible for the Promise (e.g., moved in at the beginning of ninth grade). However, because of the inclusion of student fixed effects, students with only one annual observation must be dropped from the estimation using student fixed effects.

Our preferred model includes student fixed effects. As with virtually any educational policy analysis, it is impossible in principle to exclude student fixed 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 $T \times I\{Benefit > 0\}$ interaction terms. The student fixed effects will be correlated with year dummy × Promise eligibility interaction terms when there is differential migration of different Promise eligibility groups into or out of the KPS 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 our regressions, we choose the "zero eligibility" category and the immediate year preceding the announcement of the Promise (2004–2005) as our omitted reference categories. Our interest is the time pattern of effects of Promise eligibility by year versus those omitted reference categories. The assumption is that, after controlling for student fixed effects and all the other fixed effects, if there is no clear time trend prior to the Promise announcement in the effects of Promise eligibility, but there are clear effects on the level and trend of student achievement and behavior after the Promise announcement, then this provides reasonable evidence of a true

Promise effect. In other words, it seems implausible to us that such a pattern of Promise eligibility effects over time would be due to time trends in the disturbance term that just happen to be correlated with the year × Promise eligibility interaction terms in a way that produces this pattern. More formally, we are assuming that controlling for student fixed effects, year fixed effects, Promise eligibility fixed effects, and grade fixed effects, the disturbance term is either exogenous to the year × Promise eligibility interaction term or, more weakly, is not correlated in a way that will produce this pattern. However, if we see an effect on the post-Promise trend in some outcome but, at the same time, a trend is also evident in pre-Promise data, we deem that post-Promise trend to not provide convincing evidence of a true Promise effect. To see whether student fixed effects make a difference, we also estimate a model without student fixed effects. Such models add in controls for observable characteristics of the students that do not vary over time, such as gender and race.[23]

As stated earlier, our data set 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. Because 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 have 65 percent of the 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 incentive 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

[23] The models without student fixed effects account for random effects that are correlated across years for the same student in calculating standard errors and *t*-statistics.

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 estimates to lose some precision.

Results

Main Results

Table 5 shows results for academic achievement-dependent variables, and Table 6 shows results for behavioral-dependent variables. The omitted dummies are the immediate pre-Promise year of 2004–2005 and the zero benefit category.

We do not report coefficients on other controls. In the specifications without student fixed effects, these other variables include controls for gender and race/ethnic group (white, black, Hispanic). Such non-time varying controls are dropped from the fixed-effect specifications. Both fixed-effect specifications and non-fixed specifications include controls for year of the observation by itself and by grade level. In addition, all specifications include control for free and reduced-price lunch status, which varies over time. Finally, all specifications include a dummy that indicates whether there is variation in the student's expected benefit across time after the school year 2005–2006 within students and any new enrollees post-2005–2006. Such variation could occur if a student moved out of the district and then back in, as the Promise benefit is based on length of continuous enrollment.

Our focus is on the estimated effects of the Promise benefit categories interacted with the dummy for the year 2005–2006 (the year of the announcement) and for 2006–2007 and 2007–2008, the post-Promise years. These interacted effects are relative to the effect for the zero-benefit category in the school year 2004–2005. For the fixed-effect regressions, these estimated effects also control for the student's performance or behavior in other years. In other words, we look at whether students in the various Promise benefit categories differentially changed in the years following the announcement relative to their own history, and then compare 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. For example, the GPA effects switch to being of the expected sign in the FE specification.

Although results are more often of the "expected sign" in the FE specifications, estimates are sometimes imprecise. Estimated effects in the FE specifications on GPA, number of credits earned, the zero-one dummy for whether suspended, and the detention variables are all of the expected sign (e.g., positive effects on GPA and credits earned, negative effects on the behavioral variables). However, these estimates are imprecise enough that we cannot rule out a wide range of effects.

However, some of the FE estimates are of the expected sign and statistically significant. In these FE specifications, we find statistically significant positive effects (at a 10 percent level) of Promise eligibility in 2007–2008 on whether a student earned any credits at all during the year. We also find statistically significant negative effects (at a 5 percent level or more) of Promise eligibility in 2006–2007 and 2007–2008 on the number of days a student was suspended from school during the year.

The bottom rows of Tables 5 and 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 2004–2005. For GPA, the estimated effect sizes in the fixed-effects model are about 0.05σ–0.16σ in magnitude, which represent effect sizes that are typical of many educational interventions.[24]

The average number of days of out-of-school suspension declined for Promise beneficiaries in 2006–2007, 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 the average number of days suspended over all students (which is about two days). We see that this effect is even more pronounced in the school year 2007–2008, 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.[25] 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. For the zero benefit group, this out-

[24] 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.

[25] In an appendix, available upon request, we present some figures showing trends in average fixed effects over time for different benefit categories.

migration and in-migration has tended to lead to higher student fixed effects of the students that remain, whereas for the students in the positive benefit categories this is not as true. The causes of this differential migration form an interesting topic that we hope to explore in future research.

Multiyear difference-in-differences analysis can be represented in a graph and enables detection of existing pre-intervention 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 5 and 6 plot the difference-in-differences point estimates from the fixed-effects regressions, along with 90 percent confidence intervals, across the pre- and post-Promise years. Recall that 2005–2006 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 2005–2006.

Panel A of Figure 5 shows 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 calculation 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 a 65 percent tuition subsidy or a subsidy that is 80 percent or more. The findings for GPA are very similar to the trend displayed in Panel A of Figure 5.

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 [4] and [6] in Table 5), we observe that following 2005–2006, any preexisting group × year trend appears to have been reversed. The point estimate in the school year 2007–2008 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 6 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 2003–2004 in Panel A through Panel D are approximately zero. Following the Promise, days spent in suspension decrease during the school year 2005–2006 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 determine 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 6. This implies that the overall effect on days suspended is at least in part due to effects on the likelihood of being suspended.[26]

[26] We do not model the analogous effect along the intensive margin because of the usual issues with regressions conditioning on the positive value of the outcome variable (see Angrist and Pischke [2009, Chapter 3]). In order to get an idea of 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: $\frac{\partial E(y|x)}{\partial x} = \frac{\partial E(y|x,y>0)}{\partial x} \Pr(y>0|x) + \frac{\partial \Pr(y>0|x)}{\partial x} E(y|x,y>0)$. Plugging in sample means and regression effects from columns (2) and (4) of Table 6, for the school year 2007–2008, we can back out the conditional effect on suspension equal to a reduction of less than six days of suspension.

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 driven by the intensive margin.

Robustness checks

Figure 7 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 1) we deem them not to display pre-Promise trends, and 2) 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–twelfth grade in KPS after the Promise was announced in November 2005. (We already excluded ninth-graders who came after the Promise, as they 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 2005–2006 or later. In sum, 1,037 observations are dropped. Who are the students in this zero eligibility group? They consist of the following groups:

- Students who enrolled in KPS in 2005 in their ninth-grade year after the state fall count date or did not stay throughout the whole school year. Thus, the first year "countable" toward the Promise for them was when they were tenth-graders, and that makes them ineligible.

- Students who enrolled as tenth-graders in 2005—these students will not get any benefit, even if they came before November 10, because they were not in KPS as ninth-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 7 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 2007–2008, the decrease in days spent in suspension is 1.55.

We also considered specifications in which we dropped all newly enrolled ninth-graders for all years. We wanted to make sure that our baseline results were not driven by our decision to only drop newly enrolled ninth-graders after the Promise announcement. We found that dropping all newly enrolled ninth-graders for all years did not significantly change any of our results.

Analysis by subsamples

Previous research studying the effects of educational interventions often finds heterogeneous responses for boys and girls and by race/ethnicity. The economically and racially diverse nature of KPS allows us to analyze student outcomes by race.[27] Specifically, in Figure 8 we focus on African American students and impose the same sample restrictions as used in the robustness analysis in Figure 7. This subsample consists of 6,385 observations—5,808 eligible for any tuition subsidy and 577 observations not eligible for anything.

[27] We also conducted separate regressions for boys but did not find the response different from the rest of the sample, i.e., from girls.

The results for African American students are striking. For black students, unlike for the entire sample, there do not appear to be clear group × pre-Promise trends in GPA. Panel A suggests that following the Promise, GPA has increased and continues to improve for these Promise-eligible black students. There does not appear to be a clear pre-Promise effect in the school year 2003–2004. The results are also very big in magnitude; for example, in the school year 2007–2008 there was an increase of 0.70 in GPA. The GPA effects traced in Figure 8 translate to a 0.174σ increase in the school year 2005–2006, followed by a 0.280σ increase in 2006–2007, and an enormous 0.63σ increase in 2007–2008. One might wonder why these difference-in-differences point estimates keep trending up following the Promise, as opposed to observing a one-time increase in GPA. We would expect to find such a continuing increase if following the Promise there are synergies cross-mapping into higher performance—for example, higher effort and performance in one school year could lead to still higher performance the next school year.

Panels C and D show the impact on days spent in suspension or detention. On average, the point estimate for black students implies a decrease of two days of suspension in the first full post-Promise year and a three-day decrease in 2007–2008. Note that the effect on the number of days spent 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. 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 that 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 2006–2007. 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 finds 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 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 in student achievement and behavior if students understand 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.

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 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 to strengthen 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 would 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. In addition, our paper, by its very necessity, can only examine individual effects of the Kalamazoo Promise. Promise effects that stem from changes in the school district's atmosphere or morale or from better peer effects cannot be estimated by our methodology. Certainly, school administrators and the

Kalamazoo community have been trying both to help more students access the Promise and to change attitudes of students toward their futures. We hope in future work to analyze these subsequent effects.

References

Agee, Mark D., and Thomas D. Crocker. 1996. "Parents' Discount Rates for Child Quality." *Southern Economic Journal* 63(1): 36–50.

Andrews, Rodney J., Stephen DesJardins, and Vimal Ranchhod. 2010. "The Effects of the Kalamazoo Promise on College Choice." *Economics of Education Review* 29(5): 722–737.

Angrist, Joshua, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael R. Kremer. 2002. "Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment." *American Economic Review* 92(5): 1535–1558.

Angrist, Joshua, Eric Bettinger, and Michael R. Kremer. 2006. "Long-Term Educational Consequences of Secondary School Vouchers: Evidence from Administrative Records in Colombia." *American Economic Review* 96(3): 847–862.

Angrist, Joshua, Daniel Lang, and Philip Oreopoulos. 2009. "Incentives and Services for College Achievement: Evidence from a Randomized Trial." *American Economic Journal: Applied Economics* 1(1): 136–163.

Angrist, Joshua, and Victor Lavy. 2009. "The Effects of High Stakes High School Achievement Awards: Evidence from a Randomized Trial." *American Economic Review* 99(4): 1384–1414.

Angrist, Joshua, and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics—An Empiricist's Companion.* Princeton, NJ: Princeton University Press.

Avery, Christopher, and Thomas J. Kane. 2004. "Student Perceptions of College Opportunities: The Boston COACH Program." In *College Choices: The Economics of Where to Go, When to Go, and How to Pay for It*, Caroline M. Hoxby, ed. Chicago: University of Chicago Press, pp. 355–394.

Bartik, Timothy J., Randall W. Eberts, and Wei-Jang Huang. 2010. *The Kalamazoo Promise, and Enrollment and Achievement Trends in Kalamazoo Public Schools*. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1): 249–275.

Bettinger, Eric P., Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu. 2011. "The Role of Simplification and Information in College Decisions: Results from the H&R Block FAFSA Experiment." NBER Working Paper No. 15361. Cambridge, MA: National Bureau of Economic Research.

Bloom, Howard S., Carolyn J. Hill, and Mark W. Lipsey. 2008. "Performance Trajectories and Performance Gaps as Achievement Effect–Size Benchmarks for Educational Interventions." MDRC Working Papers on Research Methodology. New York and Oakland, CA: MDRC.

Conley, Timothy G., and Christopher R. Taber. 2011. "Inference with 'Difference in Differences' with a Small Number of Policy Changes." *Review of Economics and Statistics* 91(1): 113–125.

Cornwell, Christopher M., Kyung Hee Lee, and David B. Mustard. 2005. "Student Responses to Merit Scholarship Retention Rules." *Journal of Human Resources* 40(4): 895–917.

Cornwell, Christopher M., David B. Mustard, and Deepa J. Sridhar. 2006. "The Enrollment Effects of Merit-Based Financial Aid: Evidence from Georgia's HOPE Program." *Journal of Labor Economics* 24(4): 761–786.

Donald, Stephen G., and Kevin Lang. 2007. "Inference with Difference-in-Differences and Other Panel Data." *Review of Economics and Statistics* 89(2): 221–233.

Dynarski, Susan. 2002. "The Behavioral and Distributional Implications of Aid for College." *American Economic Review* 92(2): 279–285.

———. 2004. "The New Merit Aid." In *College Choices: The Economics of Where to Go, When to Go, and How to Pay for It*, Caroline Hoxby, ed. Chicago: University of Chicago Press, pp. 63–100.

Dynarski, Susan M., and Judith E. Scott-Clayton. 2006. "The Cost of Complexity in Federal Student Aid: Lessons from Optimal Tax Theory and Behavioral Economics." NBER Working Paper No. 12227. Cambridge, MA: National Bureau of Economic Research.

Economist. 2008. "Rescuing Kalamazoo: A Promising Future." *Economist*, February 7. http://www.economist.com/node/10650702 (accessed March 1, 2012).

Fryer, Roland G. 2011. "Financial Incentives and Student Achievement: Evidence from Randomized Trials." *Quarterly Journal of Economics* 126(4): 1755–1798.

Henry, Gary T., and Ross Rubenstein. 2002. "Paying for Grades: Impact of Merit-Based Financial Aid on Educational Quality." *Journal of Policy Analysis and Management* 21(1): 93–109.

Jackson, C. Kirabo. 2010. "A Little Now for a Lot Later: A Look at a Texas Advanced Placement Incentive Program." *Journal of Human Resources* 45(3): 591–639.

Kane, Thomas J. 2003. "A Quasi-Experimental Estimate of the Impact of Financial Aid on College-Going." NBER Working Paper No. 9703. Cambridge, MA: National Bureau of Economic Research.

———. 2006. "Evaluating the Impact of the D.C. Tuition Assistance Grant Program." *Journal of Human Resources* 42(3): 555–582.

Kremer, Michael R., Edward Miguel, and Rebecca Thornton. 2009. "Incentives to Learn." *Review of Economics and Statistics* 91(3): 437–456.

Lesnick, Joy, Robert M. George, Cheryl Smithgall, and Julia Gwynne. 2010. "Reading on Grade Level in Third Grade: How Is It Related to High School Performance and College Enrollment?" Chicago: Chapin Hall at the University of Chicago.

Leuven, Edwin, Hessel Oosterbeek, and Bas van der Klaauw. 2010. "The Effect of Financial Rewards on Students' Achievement: Evidence from a Randomized Experiment." *Journal of the European Economic Association* 8(6): 1243–1265.

Levitt, Steven D., John A. List, Susanne Neckermann, and Sally Sadoff. 2012. "The Behavioralist Goes to School: Leveraging Behavioral Economics to Improve Educational Performance." NBER Working Paper No. 18165. Cambridge, MA: National Bureau of Economic Research.

Miller, Ashley. 2010. "College Scholarships as a Tool for Community Development? Evidence from the Kalamazoo Promise." Working paper. Princeton, NJ: Princeton University.

Miller, Shazia Rafiullah, Elaine M. Allensworth, and Julie Reed Kochinek. 2002. "Student Performance: Course Taking, Test Scores, and Outcomes." Chicago: Consortium on Chicago School Research.

Miller-Adams, Michelle. 2009. *The Power of a Promise: Education and Economic Renewal in Kalamazoo.* Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Moulton, Brent R. 1986. "Random Group Effects and the Precision of Regression Estimates." *Journal of Econometrics* 32(3): 385–397.

———. 1990. "An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Unit." *Review of Economics and Statistics* 72(2): 334–338.

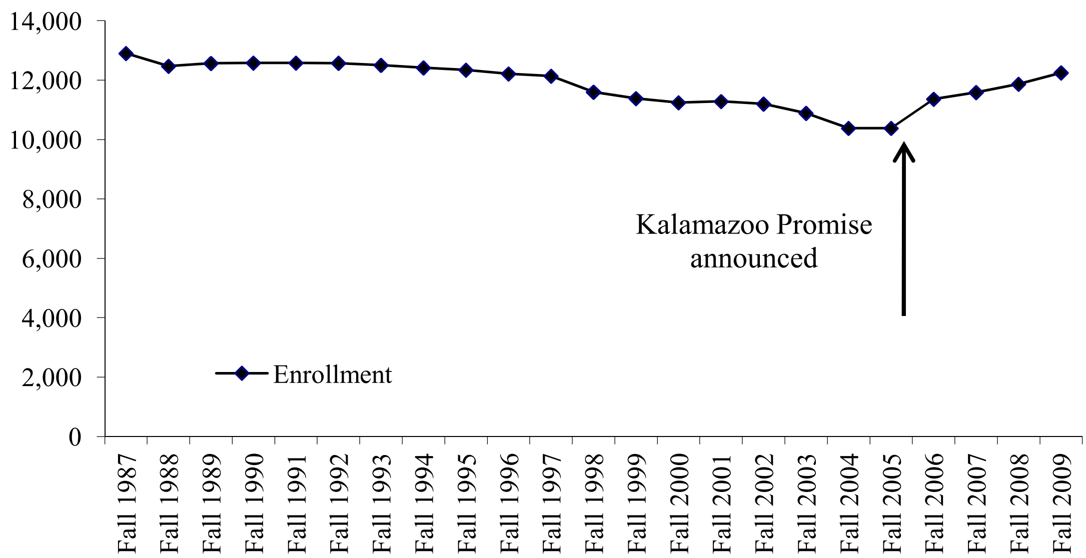
Pallais, Amanda. 2007. "Taking a Chance on College: Is the Tennessee Education Lottery Scholarship Program a Winner?" *Journal of Human Resources* 44(1): 199–222.

Scott-Clayton, Judith E. 2010. “On Money and Motivation: A Quasi-Experimental Analysis of Financial Incentives for College Achievement.” *Journal of Human Resources* 46(3): 614–646.

Sharma, Dhiraj. 2010. “The Impact of Financial Incentives on Academic Achievement and Household Behavior: Evidence from a Randomized Trial in Nepal.” Unpublished paper. Ohio State University, Columbus, OH. http://dx.doi.org/10.2139/ssrn.1681186 (accessed August 19, 2012).

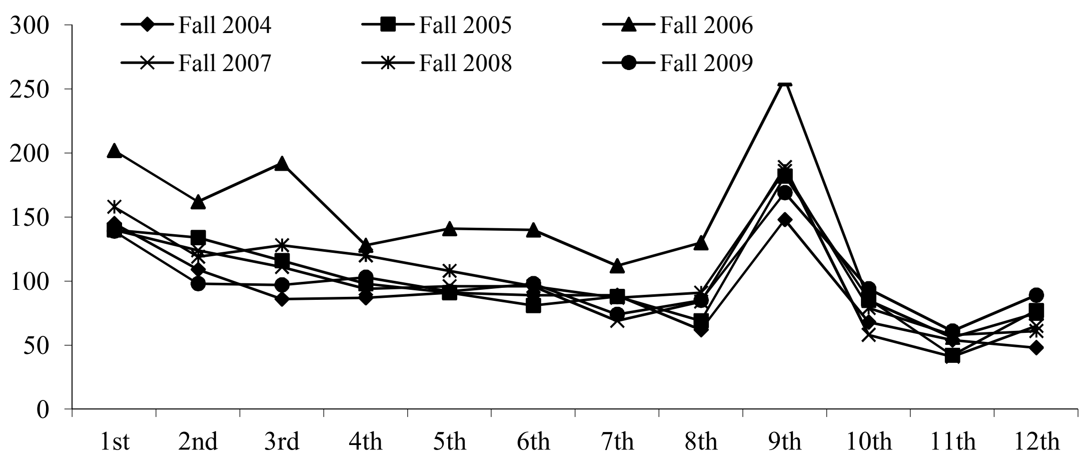
Results

Figure 1: KPS Enrollment, by Year



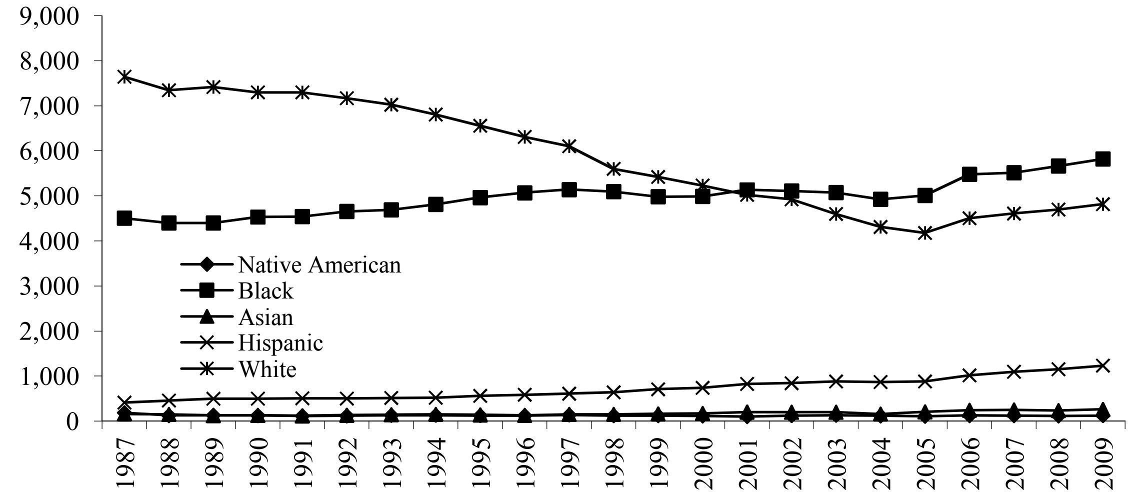
SOURCE: Bartik, Eberts, and Huang (2010)

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



SOURCE: Bartik, Eberts, and Huang (2010)

Figure 3: Number of KPS Students in Various Ethnic Groups, 1987–2009



SOURCE: Bartik, Eberts, and Huang (2010)

Figure 4: Generosity of the Kalamazoo Promise Scholarship, by Grade of Enrollment

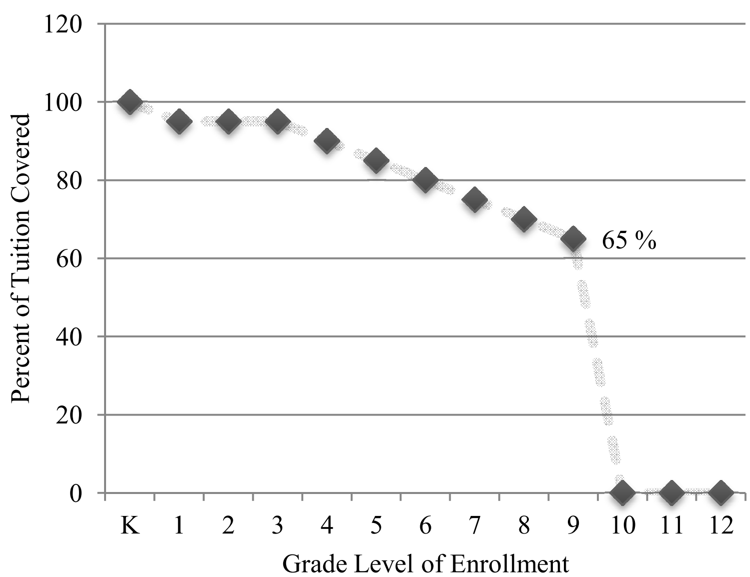


Table 1: Present Value of the Kalamazoo Promise for Graduates of KPS

Tuition subsidy group (%)	Present value (1) ($)	Present value (2) ($)
0	0	0
65	17,818	21,839
70	19,189	23,519
75	20,560	25,199
80	21,930	26,879
85	23,301	28,559
90	24,671	30,237
95	26,042	31,919
100	27,413	33,599
Present value of Kalamazoo Valley Community College (KVCC) ($)	4,731	
Present value of University of Michigan ($)	55,545	

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 four-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 four-year university at 0.55. We base these percentages on enrollment numbers in 2006–2007of 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 four-year university to 0.7. We assume the tuition cost of community colleges to be equal to $2,385 per year (15 credits). Within the universe of four-year universities, we assume that 13 percent attend the 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. The last two rows show the discounted present value of a 100 percent tuition subsidy of going to KVCC for two years and of four years at the University of Michigan.
Source: Tuition costs for community college are based on the 2011–2012 tuition costs for KVCC: http://www.michigancc.net/data/tuition (accessed August 17, 2012). Tuition costs for four-year universities are based on Michigan State Notes: http://www.senate.michigan.gov/sfa/Publications/Notes/2011Notes/NotesSum11bb2.pdf (accessed August 17, 2012).

Table 2: Trends in Kalamazoo Promise Scholarship Use

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

SOURCE: Kalamazoo Promise.

Table 3: Promise Eligibility Summary

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

SOURCE: Kalamazoo Promise.

Table 4: Summary Statistics: Means (standard deviations in parentheses), before and after the Promise, by Eligibility for the Promise (no benefit versus 65 percent or more)

	Before (2003/04–2004/05)		After (2005/06–2007/08)	
	No benefit	Benefit > 0	No benefit	Benefit > 0
Variable				
Demographic characteristics				
Female	0.50	0.48	0.55	0.48
Free/reduced price lunch	0.60	0.49	0.53	0.54
White	0.36	0.45	0.38	0.40
Black	0.51	0.46	0.52	0.50
Hispanic	0.10	0.07	0.06	0.08
Outcome variables				
Suspended (0/1)	0.20	0.22	0.15	0.23
Days suspended	1.12	1.73	0.89	2.13
	(3.39)	(9.50)	(3.64)	(8.32)
In detention (0/1)	0.07	0.09	0.08	0.12
Credits earned (0/1)	0.87	0.96	0.88	0.93
Credits earned	4.62	6.12	5.25	5.77
	(3.23)	(2.63)	(3.31)	(2.77)
GPA	1.57	2.15	1.78	2.05
	(1.22)	(1.21)	(1.27)	(1.25)
Grade				
Grade 9	0.19	0.40	0.06	0.36
Grade 10	0.30	0.25	0.28	0.24
Grade 11	0.25	0.19	0.30	0.21
Grade 12	0.26	0.17	0.36	0.18
Benefit				
Benefit = 0	1.00	0.00	1.00	0.00
Benefit = 65	0.00	0.15	0.00	0.09
Benefit = 70	0.00	0.06	0.00	0.04
Benefit = 75	0.00	0.06	0.00	0.06
Benefit = 80+	0.00	0.73	0.00	0.80
Number of observations	786	5,226	724	7,693

NOTE: “Days suspended” is days of out-of-school suspension during the school year. GPA average is computed on the four-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. Standard deviations for continuous variables are in parentheses below the sample mean.
SOURCE: KPS.

Table 5: Estimated Effect of the Kalamazoo Promise on Academic Achievement

Variables	(1) OLS GPA	(2) FE GPA	(3) OLS Credits earned	(4) FE Credits earned	(5) OLS Credits earned (0/1)	(6) FE Credits earned (0/1)
Interaction terms: γ_t						
2003–04 × Benefit > 0	0.0779	-0.0675	0.812***	0.172	0.0492**	-0.00978
	(0.964)	(-0.913)	(3.707)	(0.681)	(2.099)	(-0.331)
2004–05 × Benefit > 0	--	--	--	--	--	--
2005–06 × Benefit > 0	0.0450	0.0584	-0.239	-0.284	0.00243	-0.00987
	(0.496)	(0.750)	(-0.955)	(-1.083)	(0.0998)	(-0.436)
2006–07 × Benefit > 0	-0.159	0.133	-0.437	-0.0830	-0.00278	0.0331
	(-1.428)	(1.315)	(-1.418)	(-0.246)	(-0.0880)	(0.949)
2007–08 × Benefit > 0	-0.330**	0.205	-0.466	0.587	-0.000759	0.0879*
	(-2.526)	(1.274)	(-1.278)	(1.293)	(-0.0197)	(1.819)
Constant	2.042***	2.075***	4.637***	5.514***	0.860***	0.879***
	(18.87)	(102.7)	(20.36)	(83.44)	(40.18)	(111.8)
Observations (NT)	14,429	14,429	14,429	14,429	14,429	14,429
Observations (N)		6,618		6,618		6,618
R-squared	0.298	0.019	0.196	0.044	0.059	0.077
F-test	4.906	0.550	0.278	2.448	0.0163	2.514
p-value	0.00743	0.577	0.758	0.0865	0.984	0.0810
Effect size 2005–06	0.0354	0.0459	-0.0861	-0.103	0.0103	-0.0418
Effect size 2006–07	-0.125	0.104	-0.158	-0.0300	-0.0118	0.140
Effect size 2007–08	-0.259	0.161	-0.168	0.212	-0.00322	0.373

NOTE: Robust t-statistics in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Regressions include the following controls: female, free and 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 F-test and p-value show test statistics from a joint test of significance for the interaction terms γ_t for the years 2005–06, 2006–07, and 2007–08. The effect size is calculated by dividing the coefficient from 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 2003–04 through 2007–08 subject to sample restrictions; see the text for details. SOURCE: KPS.

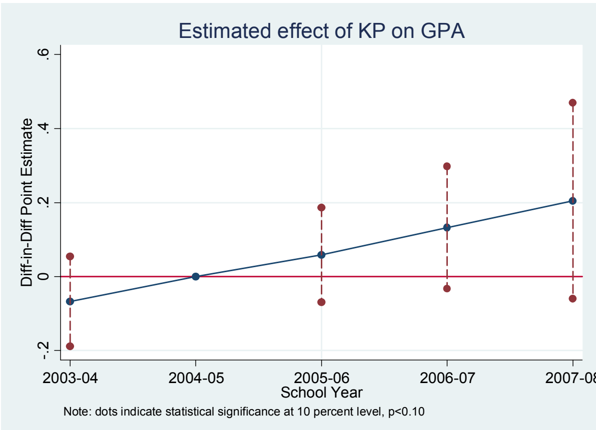
Table 6: Estimated Effect of the Kalamazoo Promise on Student Behavior

Variables	(1) OLS Suspended (0/1)	(2) FE Suspended (0/1)	(3) OLS Days suspended	(4) FE Days suspended	(5) OLS In detention (0/1)	(6) FE In detention (0/1)	(7) OLS Days in detention	(8) FE Days in detention
Interaction terms: γ_t								
2003–04 × Benefit > 0	0.00894	0.00950	-0.134	-0.0117	0.0259	-0.00258	0.0613	0.0825
	(0.313)	(0.226)	(-0.396)	(-0.0277)	(1.342)	(-0.0917)	(1.026)	(0.873)
2004–05 × Benefit > 0	--	--	--	--	--	--	--	--
2005–06 × Benefit > 0	0.00786	-0.00969	0.369	-0.357	-0.00712	-0.0157	0.0466	-0.0411
	(0.260)	(-0.253)	(0.914)	(-0.796)	(-0.298)	(-0.521)	(0.593)	(-0.370)
2006–07 × Benefit > 0	0.0249	-0.0215	-0.115	-1.296**	0.0542**	0.00936	0.0933	-0.0671
	(0.693)	(-0.427)	(-0.235)	(-2.396)	(2.147)	(0.252)	(1.218)	(-0.588)
2007–08 × Benefit > 0	0.0378	-0.0579	-0.502	-1.796***	-0.00185	-0.0207	-0.0687	-0.179
	(1.034)	(-0.924)	(-1.036)	(-2.833)	(-0.0675)	(-0.468)	(-0.938)	(-1.379)
Constant	0.207***	0.210***	1.772***	1.521***	0.122***	0.106***	0.245***	0.197***
	(6.876)	(18.09)	(5.040)	(4.218)	(5.798)	(11.38)	(4.470)	(4.888)
Observations (NT)	14,429	14,429	14,429	14,429	14,429	14,429	14,429	14,429
Observations (N)		6,618		6,618		6,618		6,618
R-squared	0.149	0.056	0.047	0.023	0.090	0.042	0.056	0.037
F-test	0.411	0.431	2.018	3.319	4.394	0.491	3.834	1.019
p-value	0.663	0.650	0.133	0.0363	0.0124	0.612	0.0217	0.361
Effect size 2005–06	0.0187	-0.0230	0.0310	-0.0301	-0.0231	-0.0509	0.0426	-0.0376
Effect size 2006–07	0.0593	-0.0510	-0.00965	-0.109	0.176	0.0304	0.0853	-0.0613
Effect size 2007–08	0.0899	-0.138	-0.0423	-0.151	-0.00602	-0.0671	-0.0628	-0.164

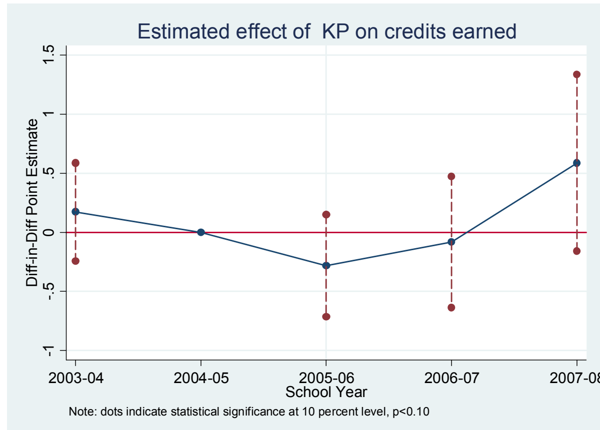
NOTE: Robust *t*-statistics in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.
Same as in Table 5. Universe: Students enrolled in KPS in grades 9–12 during school years 2003–04 through 2007–08 subject to sample restrictions, see the text for details. SOURCE: KPS.

Figure 5: Estimated Effect (fixed effects) of the Kalamazoo Promise (KP) on Academic Achievement

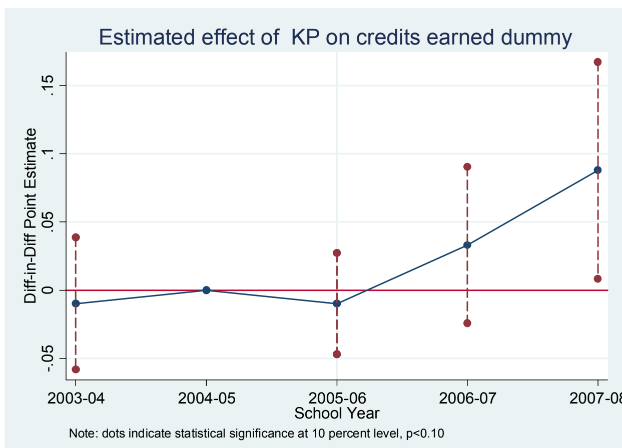
Panel A



Panel B



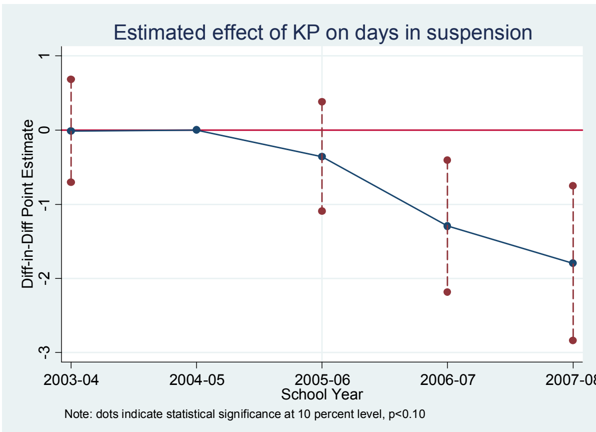
Panel C



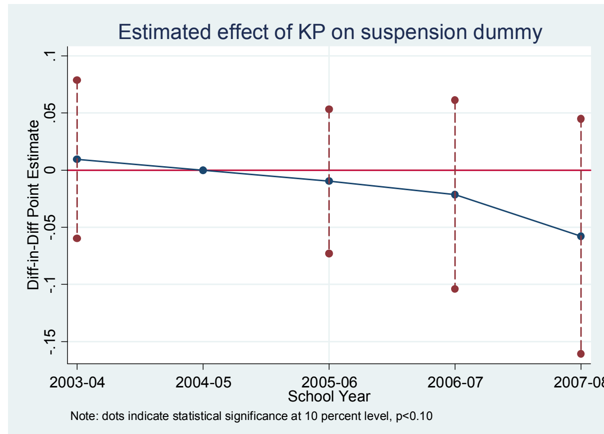
NOTE: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005–06). Panels A–C use the same specification as fixed-effects regressions in Table 5. Dots around estimates indicate statistical significance at the 10 percent level, $p < 0.10$.

Figure 6: Estimated Effect (fixed effects) of the Kalamazoo Promise (KP) on Student Behavior

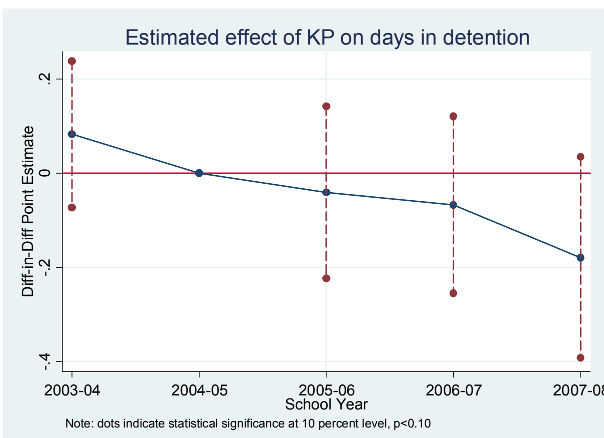
Panel A



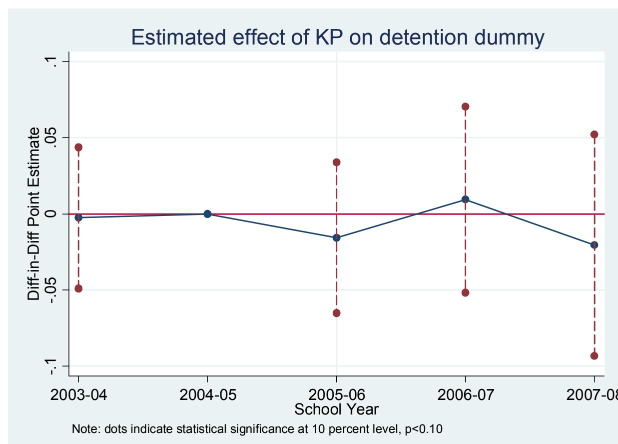
Panel B



Panel C



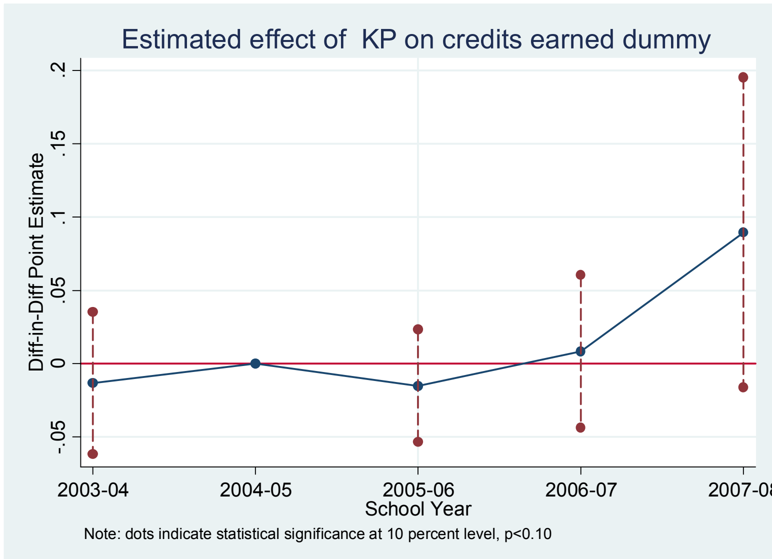
Panel D



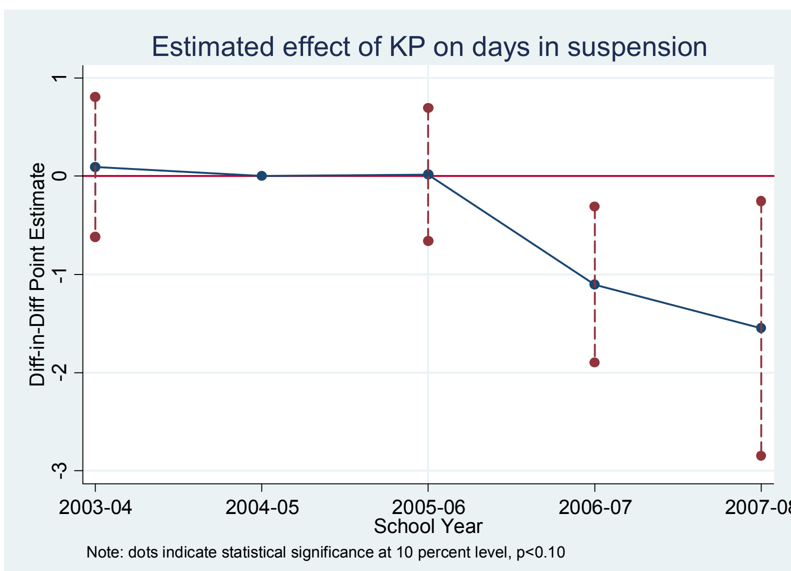
NOTE: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005–06). Panels A–D use the same specification as fixed-effects regressions in Table 6. Dots around estimates indicate statistical significance at the 10 percent level, $p < 0.10$.

Figure 7: Estimated Effect (fixed effects) of the Kalamazoo Promise (KP) on Outcomes—Robustness Checks for Selected Results

Panel A



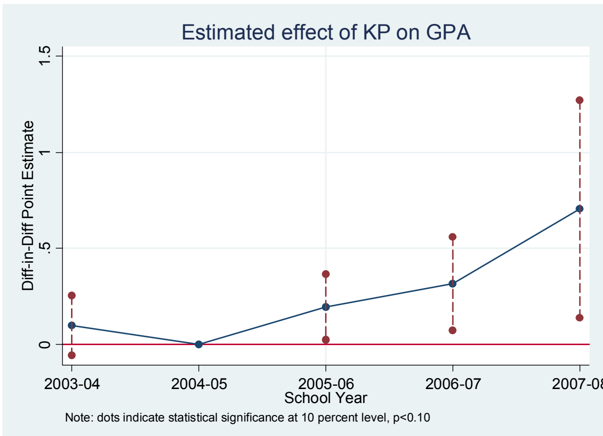
Panel B



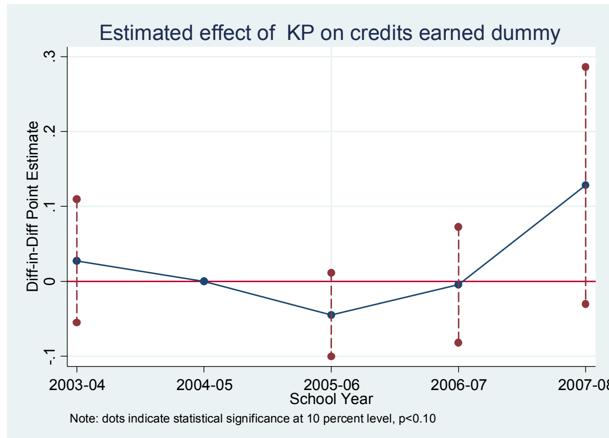
NOTE: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005–06). In both specifications we drop the all the new enrollees since (and including 2005–06) and all of those who changed their eligibility level after and including 2005–06. This sample consists of 13,392 observations.

Figure 8: Estimated Effect (fixed effects) of the Kalamazoo Promise (KP) on Outcomes—Selected Results for the Subsample of African American Students Only

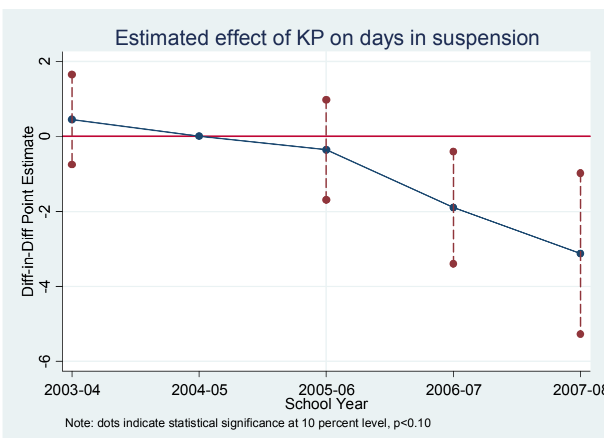
Panel A



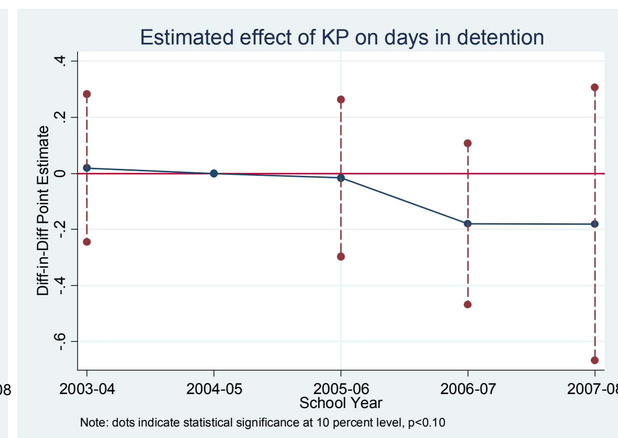
Panel B



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 changed their eligibility level after and including 2005–06. This sample consists of 6,385 observations.