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ABSTRACT

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

JEL Classification Codes: I21, I22

Key Words: academic output, educational incentives, universal scholarship, natural experiment

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INTRODUCTION

The Kalamazoo Promise provides an unusual model for revitalizing an urban school district and its community. Announced on November 10, 2005, the Kalamazoo Promise provides large college scholarship benefits to graduates of Kalamazoo Public Schools (KPS), a midsized school district (numbering a little over 10,000 students) with a racially and economically diverse student population. Anonymous donors promised to pay up to 100 percent of college tuition for any 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 9th grade, 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 ("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.[1]

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

[1] See http://www.upjohn.org/promise/promiseschol arships.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.[2] 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.

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 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.

This quasi-experimental investigation is needed because there is theoretical uncertainty about the magnitude and sign of the Promise's effects on student behavior and achievement. On the one hand, the Promise relaxes credit constraints of attending any public college in Michigan

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

and lowers the relative cost of selective state universities, which may spur the students to exert additional effort. On the other hand, the Promise may diminish the value of merit-based scholarships, leading the students to exert less effort. Furthermore, for students from disadvantaged families, the net benefit of the Promise may be small, as they are eligible for need-based financial aid. (However, many may not be fully aware of this eligibility). Some high school students may not respond to the Promise because they do not understand the mapping between education inputs and outputs. Other students may not respond to the Promise because its benefits are too delayed and uncertain. In sum, the Promise's impact on student outcomes is an empirical question.

Our analysis finds that the Kalamazoo Promise has statistically and substantively significant effects in 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. When confronted with incentives generated by the Promise, students are more likely to react along a margin they perceive that they can control, such as improving their behavior. On the other hand, for African American students, we estimate a dramatic increase in GPA, ranging from about 0.17 standard deviations to about 0.60. For these students, whose baseline achievement and behavior indicators lag 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 the virtue of students being present in the classroom for some critical number of days. Finally, our estimated effects are only apparent when the analysis controls for

student “fixed effects”—that is, when it actually considers differences in behavior of the same student before and after the Promise announcement.

The remainder of the paper is organized as follows. The following section discusses related previous research literature, including research on the effects of student financial incentives on student achievement and behavior and research on effects of the Kalamazoo Promise. The next section provides further background information on the KPS district and the Kalamazoo Promise. We then describe the data we use, as well as the econometric models we use to analyze the data, and then describe the results. The final section concludes.

RELATED LITERATURE

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

Several recent studies have examined how financial incentives affect student achievement and behavior. Kremer, Miguel, and Thornton (2009) study the effects of a merit-based randomized scholarship program for girls in primary schools in Kenya. The evaluation of the scholarship, which was implemented as a randomized experiment at the school level, provided the winning 6th grade girls with an award for the next two academic years. The authors find that the scholarships increased performance by 0.09–0.14 standard deviations. Interestingly, the authors find positive externalities for boys, who were not entitled to the scholarship, and for girls who had a relatively low probability of winning the scholarship.

Dhiraj Sharma (2010) studies the impact of a randomized cash rewards program among Nepalese 8th graders. Similar to Kremer, Miguel, and Thornton (2009), the randomization was

conducted across schools. The cash rewards were based on total scores on exams. Sharma finds that the financial impact of these incentives equaled about a 0.09 standard deviation gain in aggregate scores.

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 about 10 percent more likely to have finished 8th grade and scored about 0.2 standard deviations higher on achievement tests.

Some research examines student incentive effects in developed economies. Angrist and Lavy (2009) look at the effects of a cash rewards experiment on graduating from Israeli high schools. The authors find strong effects among high-ability women. Angrist, Lang, and Oreopoulos (2009) study the effects of merit-based scholarships on students who are solid performers, but not top-ranked, among entering first-year undergraduates at a large Canadian university. They too find strong effects for women, but none for men. In a similar study, Leuven, Oosterbeek, and van der Klaauw (2010) conduct a randomized experiment among first-year undergraduate students at the University of Amsterdam. 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.

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 it perfectly, the optimal contract should be conditional on output. However, this result 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).[3]

In the case of the Promise, the program provides complex and contradictory incentives for changes in student behavior and achievement, which may in turn be only partly understood by students. 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.

The Promise may also lower the value of merit-based scholarships. However, many students may not fully understand what is required for merit-based scholarships, or the Promise's effects on such scholarships. For low-income students, the Promise may in many cases simply replace institutional or government need-based aid. However, both the need-based aid system and eligibility requirements may be unclear for low-income students. In addition, from the perspective of the students, the tuition subsidies of the Promise might be too delayed and too uncertain.[4] For example, Levitt et al. (2012) argue that financial incentives are less potent if they are handed out with a delay. Finally, if high school students do not understand the mapping

[3] 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.

[4] 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.

between educational inputs and outputs, then the benefits of a college scholarship might appear too abstract to alter any behavior.

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 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 the reaction of the real estate market. One purpose of the Promise is to promote the Kalamazoo area's economic development. These economic development effects could occur because Kalamazoo now seems attractive to parents and businesses. Miller (2010) addresses these issues by studying whether the effects of the scholarship have been capitalized by the real estate market. Using a difference-in-differences design, Miller (2010) does not find positive effects of the Promise on 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, which will require higher student achievement during high school.

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 intense social and economic problems than its surrounding metropolitan areas. For example, family poverty rates as of the 2000 census were 13.6 percent in the city of Kalamazoo and 6.5 percent in all of Kalamazoo County.

Even before the Promise, the Kalamazoo school district had a considerable portion of poor students from a wide variety of ethnic groups. 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, although KPS retained a considerable percentage of white students and students who did not qualify for free and reduced price lunches, the percentage of students in these two groups 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 (Figure 3). 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 purpose of the Promise is to promote economic and community development, in part by attracting parents and businesses to the Kalamazoo area; to boost educational achievement and attainment; and 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.[5] The scholarship covers up to 100 percent of all tuition and mandatory fees for up to four years and must be used within 10 years of high school graduation. The benefit is graduated based on the length of attendance in the KPS system. Figure 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 9th grade (65 percent) and 10th 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

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

graduation, she is eligible for a 100 percent Promise tuition subsidy. If that student instead switches to another district in 5th grade and later reenrolls in KPS in 9th 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. They do, of course, do 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 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 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). The remainder, 181 students, enrolled in public universities, of which the majority enrolled at Western Michigan University

(101 students), followed by Michigan State University (37), and University of Michigan (17). We assume that these college-going probabilities remain constant over time and across different tuition subsidy groups.[6] 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 is about $27,413, while for someone who is eligible for a 65 percent subsidy, the present value is about $17,818.

Take-up of the Kalamazoo Promise and Variation in Eligibility

The Kalamazoo Promise has been widely used among KPS graduates. As Table 2 shows, in the various graduation years, 80–90 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 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 9th grade), eligible for a 65 percent tuition subsidy (entered KPS at 9th grade), and eligible for a 95 percent subsidy (entered KPS at first, second, or third grade).[7]

[6] In fact, the Kalamazoo Promise has altered these probabilities. Our aim with this calculation is for illustrative purposes only. In column (2) of Table 1, we lower the probability of going to a community college to 0.30. Since the likelihood of attending a four-year college correlates with family background, such weights might better reflect preferences of high-income families. This reweighting increases the present value of the scholarship, holding other parameters constant. For an in-depth study on how the Kalamazoo Promise altered the college choice set across time and different income groups, see Andrews, DesJardins, and Ranchhod (2010).

[7] Anecdotally, we know that many of those who enter KPS at 9th grade have previously been students who attended private or charter schools from kindergarten through 8th 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 9th grade and students entering at other grade levels.

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 9th 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 than students in earlier grades to believe that their achievement and behavior in school will affect admission prospects at more selective colleges.

Our regression sample consists of 9th–12th graders from the school years 2003–04 to 2007–08. 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 1997–98, we consistently track enrollment histories for everyone since 6th 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, types of courses taken, 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 2003–04 and 2004–05, these "virtual Promise benefits" are virtual in the sense that the student was unaware of them, as the Promise was not announced until November 2005. Therefore, we would assume that any effect of this simulated Promise benefit in those years reflects effects that are associated with what grade the student entered KPS, rather than the effect of a Promise benefit of which the student had no knowledge.

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

On November 10, 2005, students became aware of the potential Promise benefits that would accrue to them given their enrollment in KPS to date. This allows some effect of Promise benefits on student achievement and behavior after that date. However, it would be reasonable to assume that there might be some lag 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 2006–07 is a full post-Promise year. By fall 2006, students may more fully understand what the

Promise might mean for their future. Including the 2007–08 school year adds a second full post-Promise year to help confirm effects estimated for the 2006–07 year.

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.

Methods

Estimating the effects of the Kalamazoo Promise program is challenging. A key problem is the difficulty in identifying suitable comparison groups. For example, consider comparing a district with a Promise-style program with a district that lacks such a program. The achievement/behavior levels and trends in the Promise district could be due to either the Promise or other differences in the two school districts. One promising approach would be to use panel data for both the Promise and the non-Promise district and conduct a difference-in-differences analysis. Currently we do not have access to microdata from other non-Promise districts, which means our analysis must be restricted to the KPS district.

We can derive one possible comparison group within KPS by using variation in Promise eligibility stemming from differences in pre-Promise enrollment decisions. Because the announcement of the Promise in November 2005 was a surprise, families did not choose the timing of enrollment of their children in KPS in anticipation of the possibility of coverage by a universal scholarship program. Thus, some children already enrolled in KPS found themselves to be eligible for 100 percent tuition, some for 65 percent tuition, while others could expect to receive no tuition subsidy at all. This unanticipated and exogenous variation in the eligibility for

a college scholarship provides an opportunity to infer the causal impact of the Promise on student outcomes and behavior.

This differential change in tuition scholarships can be seen as a natural experiment: an exogenous change in policy assigns certain individuals to a "treatment," whereas other individuals are not affected (the "controls"). However, because it is a natural experiment, this differential change in tuition scholarship is not randomly assigned, but rather depends on pre-Promise enrollment decisions.

Because students enrolled in different grades will differ in their behavioral and achievement levels, it would be misleading to use a simple comparison of achievement levels between eligible and ineligible students to estimate the Promise's effects. There is no reason to think that students not eligible for Promise benefits because they enrolled in 10th grade or later will on average be similar in academic achievement or behavior to students eligible for various Promise tuition subsidies. By simultaneously focusing on not only changes over time but also differences across eligibility groups, we end up with comparisons that more plausibly represent Promise effects. In order to address concerns of bias due to systematic pre-Promise differences between groups of students that enrolled in KPS in 9th grade as opposed to, say, kindergarten, we conduct our long-term analysis in regression-adjusted difference-in-differences framework where, for the pre-Promise school years, we calculate the virtual benefit as if the Promise were in effect during those years. Hence, we treat the ineligible students as our control group.

This approach, however, has an important caveat, namely, that it is probable that the group of students entitled to zero benefits from the Promise is also affected by the Promise. Such Promise effects on ineligible students could be due to peer effects, or effects from school climate or policy changes induced by the Promise. If even this "zero eligibility" group has been affected

by the Promise, our estimated effects by comparing eligible versus ineligible students will understate the Promise's full effects.

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

$$y_{it} = \alpha + \varphi I\{Benefit > 0\}_i + \sum_t \delta_t T_t + \sum_t \gamma_t \, (T_t \times I\{Benefit > 0\}_i) + \, x'_{it}\beta \, + u_{it} \quad (1)$$

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. This dummy captures any fixed differences between the group eligible for the Promise and the group not eligible. The time effects T control for the way the outcome variable of interest is influenced over time. We regress outcome y on this indicator function, the time effects, and an interaction term between Promise indicator and time effects. In our regressions, we choose the "zero eligibility" category and the immediate year preceding the announcement of the Promise (2004–05) as our omitted reference categories. The rationale for the Promise eligibility dummy is that Promise eligibility versus no Promise eligibility may be more salient for most students and their families, and that we might get somewhat more precise and informative estimates by simply looking at the average effects of *any* Promise eligibility. The coefficients on the $T \times I\{Benefit > 0\}$ interactions are the differences between the pre- and post-Promise outcomes for the two groups determined by the generosity of the Promise formula.

The identifying assumption of model (1) is that the $T \times I\{Benefit > 0\}$ interaction terms are orthogonal to the error term u_{it}. We believe this untestable assumption to be plausible, as the fraction of the tuition covered by the Promise is a function of past decisions that were not made

in anticipation of the announcement of the scholarship. If this assumption holds, the estimated γ's will represent the effects of scholarship eligibility on outcomes.[8]

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

As with virtually any educational policy analysis, it is impossible in principle to exclude student 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 school district after the Promise. For example, we could imagine that some families with "better students"—in part, "better" for reasons that are unobserved—may be less likely to move students with zero eligibility out of KPS because of the Promise. This might occur if such students also have younger siblings who *are* eligible for the Promise. To control for such possible biases, we replace α in Equation (1) with α_i.

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

[8] In this specification we cannot assume the u_{it} to be independent over time; hence we compute individual cluster-robust standard errors.

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 9th grade after November 10, 2005 (as these students are entitled to 65 percent of tuition covered if they stay enrolled). We allow for new entrants in grades 10–12, as they are entitled to zero coverage and have no financial incentives to enroll in KPS because of the Promise. Nevertheless, in order to be prudent about maintaining the exogenous nature of how the Promise assigns the different levels of generosity, we conduct a robustness check by excluding these observations. This turns out not to matter much for our main results, though for some results it leads the point estimate losing some of its precision.

Descriptive Statistics

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

As can be seen from the demographic data, the student population of KPS over this time is certainly diverse. Many disadvantaged students are included, as well as many racial minorities, but there are also many white students and nondisadvantaged students. We notice several differences between the groups 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 Americans and beneficiaries of free and reduced price lunch.

As our dependent variable, we use several different metrics. For each student and year, we compute GPA. We assign A to equal 4, B to equal 3, C to equal 2, D to equal 1, and F to equal zero. We obtain the incidence of suspension and the number of days within the school year spent in suspension. We also collect data on the incidence of in-school suspension and look at the effects of the Promise on credits earned.[9]

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

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

Using GPA as a measure of achievement raises some concerns about whether changes in teacher behavior might drive any results—have teachers, for example, become more lenient following the announcement of the Promise? As long as any such changes in the school environment are uniformly affecting students, their impact is controlled for by the time fixed effects *T* in Equation (1). A potential pitfall for our empirical strategy would be if, post-Promise, teachers systematically graded a given eligible student (as we control for fixed effects) differently from a student who is not eligible. We deem this rather unlikely. However, in such cases, one would like to conduct the analysis using more standardized measures of high school

[9] We have also looked at the impact on the number of attempted advanced-placement (AP) credits. AP classes are intended to offer material at a similar level to undergraduate courses in college. KPS displays relatively low levels of enrollment in AP courses and of students obtaining AP credits.

achievement, such as the Michigan Educational Assessment Program test (MEAP).[10] Unfortunately, the MEAPs are only comparable since the fall of 2005, just about the time when the Kalamazoo Promise was announced. Thus, we cannot control for pre-Promise trends, which turn out to be important.

There also is considerable scope for student behavior to improve. A large proportion of students were suspended or detained each year, and the figure for mean days of detention and suspension (which includes those students who had zero days for the year) is large enough that we certainly could imagine some significant reduction in these indicators of poor behavior.

Table 4 shows that some 9th graders are not eligible for the Promise. In those cases, the student had enrolled after the state school fall census date, and according to conversations with the administrators of the scholarship, the enrollment of such a student counts as if the student had enrolled in 10th 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 9th grade after November 10, 2005.

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 2004–05 and the zero benefit category.

We do not report coefficients on other controls, which are included in these specifications. These other variables include controls for grade level, gender, and race/ethnic

[10] The MEAP is a standardized test used by the state of Michigan for No Child Left Behind accountability.

group (white, black, Hispanic) and a dummy, which indicates whether there is variation in the benefit across time after school year 2005–06 within students and any new enrollees post-2005–06. Obviously, these controls are eliminated when we include student fixed effects. The fixed effect regressions do control for free and reduced price lunch status, which is a time-varying variable.

Our focus is on the estimated effects of the Promise benefit categories interacted with the dummy for the year 2005–06 (the year of the announcement) and for 2006–07 and 2007–08, the post-Promise years. These interacted effects are relative to the effect for the zero-benefit category in the school year 2004–05. For the fixed-effect regressions, these estimated effects also control for the student's performance or behavior in other years. In other words, we 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. In particular, we find evidence that Promise eligibility had positive effects on GPA, although this effect is not precisely estimated.

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

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 2004–05. For the GPA, credits earned, and days suspended, the estimated effect sizes are about 0.1σ–0.16σ in absolute magnitude, which represent effect sizes that are typical of many educational interventions.[11]

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

As Tables 5 and 6 show, results differ considerably when controlling for individual student fixed effects. This implies that individual student fixed effects and their trends over time must be correlated with the interactions between year dummies and benefit categories.[12] 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

[11] 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 1st 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.

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

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-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 enable detection of existing preintervention group × time trends. The idea is that if our estimation procedure is sound, we would not see any significant effects for Promise-eligible groups versus non-Promise-eligible groups in the years preceding the announcement of the Promise. This is a type of falsification test for our model.

The various panels of Figures 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 2005–06 was only a partial Promise year, as the Promise was announced in November of 2005. We might expect effects in this first Promise year to be smaller, as it may take some time for students, parents, and teachers to make much of a substantial adjustment to the incentives provided by the Promise. In general, the effects are statistically insignificant for 2005–06.

Panel A of Figure 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. Our rationale for this exercise has

been that for both these groups of eligible students, teachers might be even less certain about eligibility levels. The findings for GPA are very similar to the trend displayed in Panel A of Figure 5.[13]

Turning to Panels B and C, which plot the effect on credits earned and whether the student earned any credits (i.e., the point estimates from columns [8] and [10] in Table 5), we observe that following 2005–06, any preexisting group × year trend appears to have been reversed. The point estimate in the school year 2007–08 suggests that the probability of earning any credits is about 8.8 percent higher for students eligible for some future tuition subsidy. This latter point estimate is significant at the 5 percent level.

Figure 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 2003–04 in Panel A through Panel D are approximately zero. Following the Promise, days spent in suspension decrease during the school year 2005–06 and continue to decrease.

Table 4 shows that the distribution of days spent in suspension and detention is quite skewed—as most students are not suspended or detained, there is a large cluster of zeros. In order to 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

[13] We observe a similar trend for enrollment in AP courses and the number of attempted AP credits. Attempted AP credits are significantly lower for Promise-eligible students in the 2003–04 pre-Promise year compared to the 2004–05 year. On one hand, this suggests that if we compared post-Promise AP credits for Promise-eligible students in 2006–07 with the 2003–04 year, rather than the 2004–05 year, the Promise effect might appear more significant. On the other hand, these results suggest that even in the pre-Promise period, there might have been some differential trends in AP enrollment among different Promise eligibility groups. For example, perhaps there were attempts to expand AP enrollment that particularly affected long-time KPS students versus newcomers. This therefore raises doubts about the post-Promise results for this variable. However, for the other variables, there are no significant differences between Promise-eligible groups versus ineligible groups across the two pre-Promise years.

the overall effect on days suspended is at least in part due to effects on the likelihood of being suspended.[14]

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

Robustness Checks

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 10th–12th grade in KPS after the Promise was announced in November 2005. (We already excluded 9th graders who came after the Promise, as they would be eligible for Promise benefits, which might differentially affect in-migration. However, we previously included 10th–12th graders who came after the Promise announcement, as they are ineligible for Promise benefits.) This reduced sample also excludes those who had a change in their benefit (dropped out and reenrolled, for example) in 2005–06 or later. In sum, 1,037 observations are dropped. Who are the students in this zero eligibility group?

[14] We do not model the analogous effect along the intensive margin due to the usual issues with regressions conditioning on the positive value of the outcome variable (see Angrist and Pischke [2010, Chapter 3]). In order to get an idea how much of this effect is due to the intensive margin, we conduct the following back-of-the-envelope calculation. When differentiating the equation $E(y|x) = E(y|x,y > 0)Pr(y > 0|x)$ with respect to x, we obtain that the overall average effect of a variable x on y is a weighted average of the intensive and extensive margins: $\frac{\partial E(y|x)}{\partial x} = \frac{\partial E(y|x,y>0)}{\partial x} Pr(y > 0|x) + \frac{\partial Pr(y>0|x)}{\partial x} E(y|x, y > 0)$. Plugging in sample means and regression effects from columns (2) and (4) of Table 6, for the school year 2007–08, we can back out the conditional effect on suspension equal to a reduction in less than six days in suspension.

- Students who enrolled in KPS in 2005 in their 9th grade year after the state fall count date or did not stay throughout the whole school year. Thus, the first year "countable" toward the Promise for them was when they were 10th graders and that makes them ineligible.
- Students who enrolled as 10th graders in 2005—these students will not get any benefit even if they came before November 10 because they were not in KPS as 9th graders.

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

The lower panel for Figure 8 shows the effect on days spent in suspension. This effect is still statistically different from zero, though the point estimates are a bit smaller in absolute magnitude: in 2007–08, the decrease in days spent in suspension is 1.55.

We also considered specifications in which we dropped all newly enrolled 9th graders for all years. We wanted to make sure that our baseline results were not driven by our decision to only drop newly enrolled 9th graders after the Promise announcement. We found that dropping all newly enrolled 9th graders for all years did not significantly change any of our results.

Analysis by Subsamples

Previous research studying effects of educational interventions often finds heterogeneous responses for boys and girls and by race/ethnicity. The economically and racially diverse nature

of KPS allows us to analyze student outcomes by race.[15] 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.

The results for African American students are striking. For black students, unlike for the entire sample, there do not appear to be as clear group × pre-Promise trends in GPA. Panel A suggests that following the Promise, GPA has increased and continued to improve for these Promise-eligible black students. There does not appear to be a clear pre-Promise effect in the school year 2003–04. The results are also very big in magnitude; for example, in the school year 2007–08 there was an increase of 0.70 in GPA. The GPA effects traced in Figure 8 translate to a 0.174σ increase in the school year 2005–06, followed by a 0.280σ increase in 2006–07, and an enormous 0.63σ increase in 2007–08. 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, which remains at a steady level past that. We would expect to find such a continuing increase if following the Promise there are synergies cross mapping into higher performance, 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 in suspension and detention. On average, the point estimate for black students implies a decrease of two days in suspension in the first full post-Promise year and a three-day decrease in 2007–08. Note that the effect on the number of days in detention is not precisely estimated.

[15] We also conducted separate regressions for boys but did not find the response different from the rest of the sample.

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

Our results relate directly to the body of work trying to understand the incentives in urban education. In his work on incentivizing students in urban schools, Fryer (2011) concludes that, in general, paying for inputs tends to give better results than conditioning rewards on student output. These findings are consistent with students not fully understanding the education production mapping between inputs and achievement. Specifically, Fryer 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 will directly lead to a higher GPA. One possible future role for school policies could be to help students better

understand the link between their student work effort and achievement and future returns to education.

As mentioned before, our paper focuses on short-run effects in the Kalamazoo Promise. Promise-caused trends may have increased further in subsequent years. 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 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 towards their futures. We hope in future work to analyze these subsequent effects.

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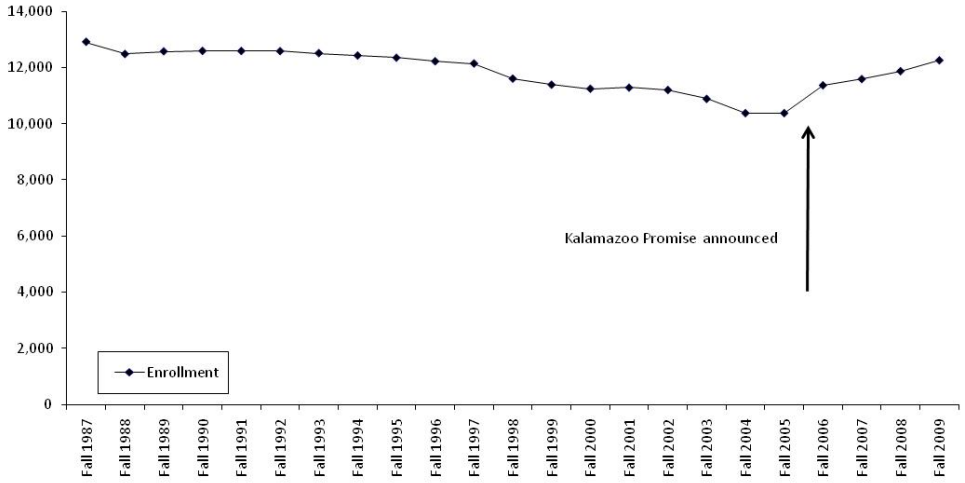
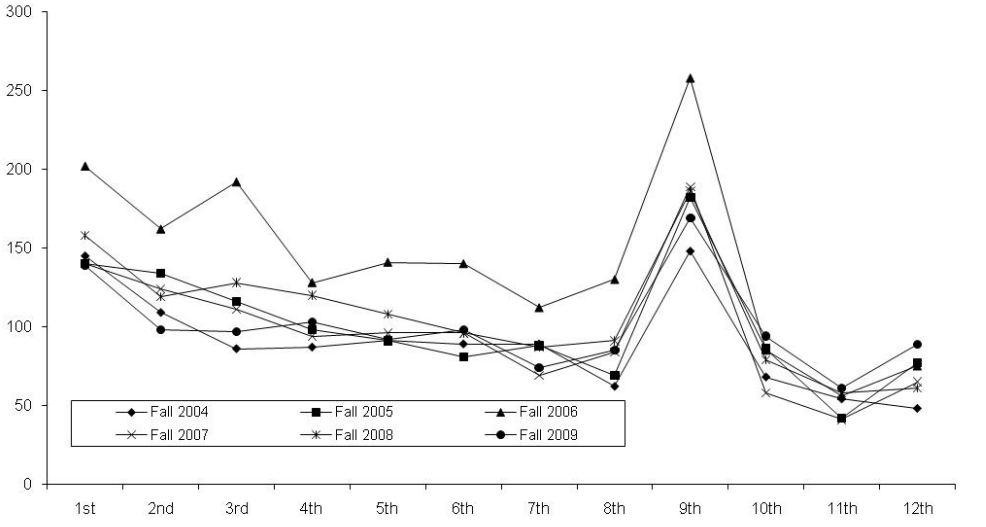
RESULTS

Figure 1 KPS Enrollment, by Year.



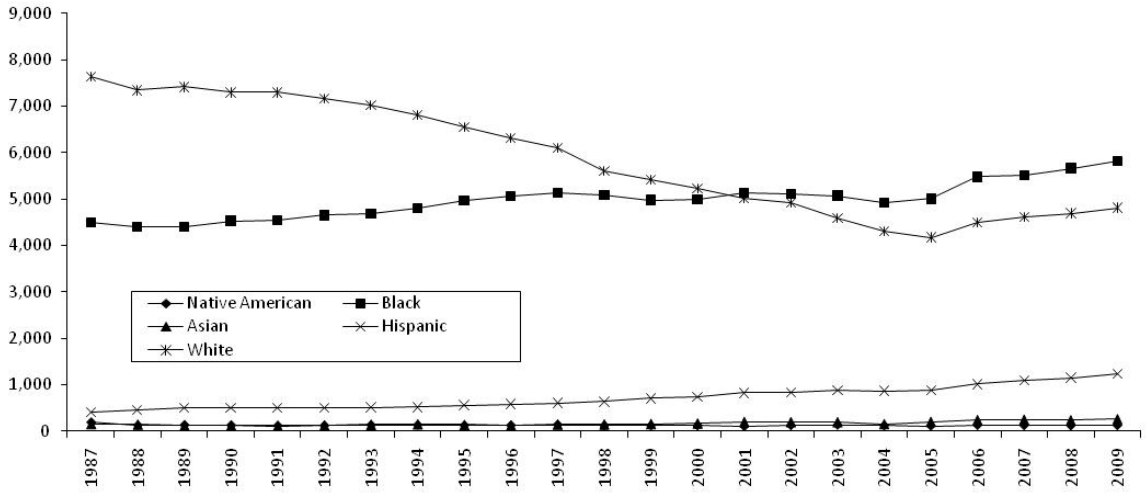
Note: Data come from KPS, and are based on state count. Numbers include special ed, alternative ed, and students at Kalamazoo Area Math and Science Center. These are total student counts, not full time equivalent counts. Numbers exclude out-of-district Education for Employment (vocational education)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

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



Note: Data come from micro student data from KPS. A new entrant is a student enrolled in KPS as of the fall count day of a particular school year who was not enrolled as of the fall count day the previous school year.

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



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

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

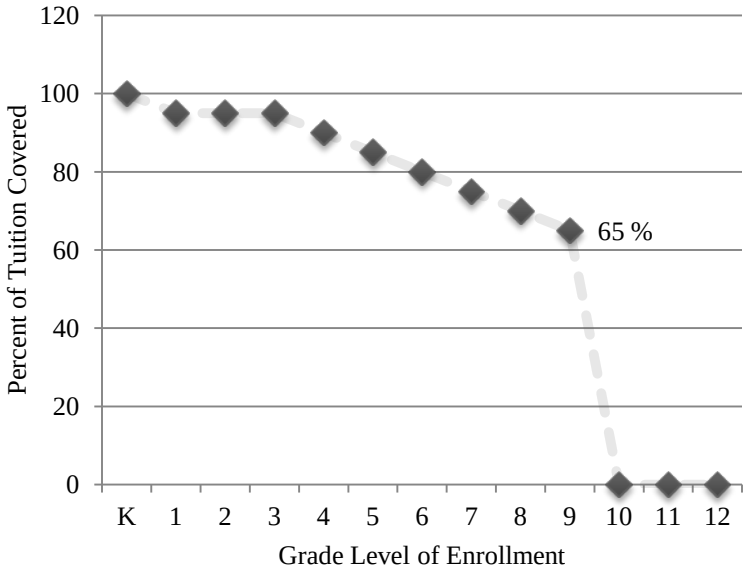
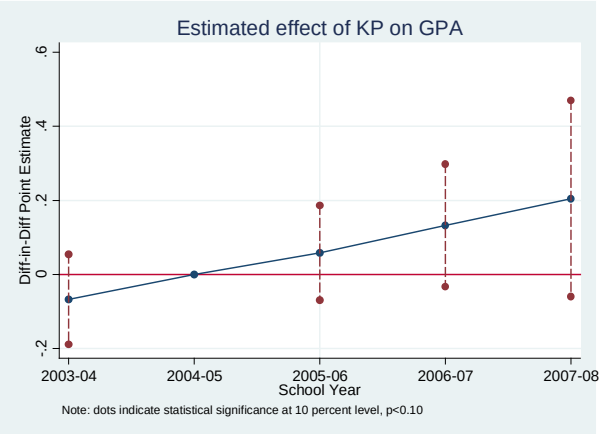
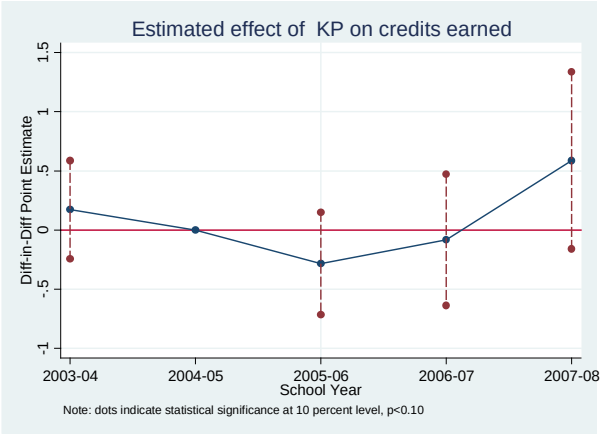


Figure 5 Estimated Effect (fixed effects) of the Kalamazoo Promise (KP) on Academic Achievement.

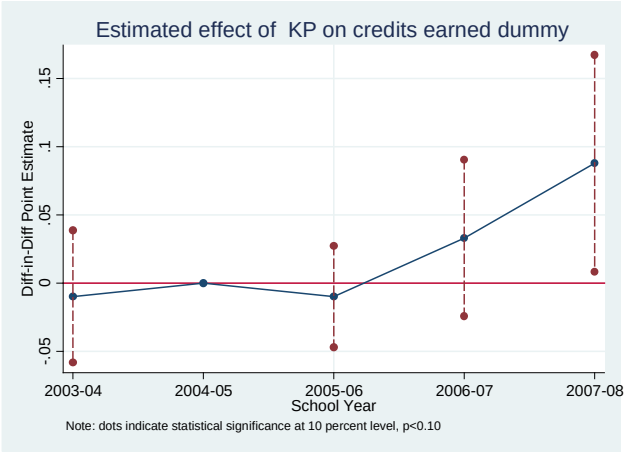
Panel A



Panel B



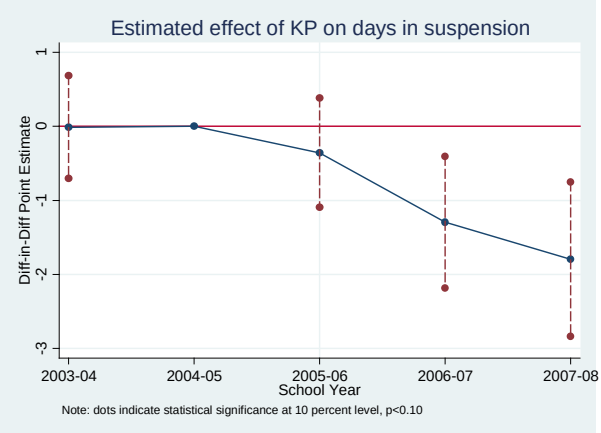
Panel C



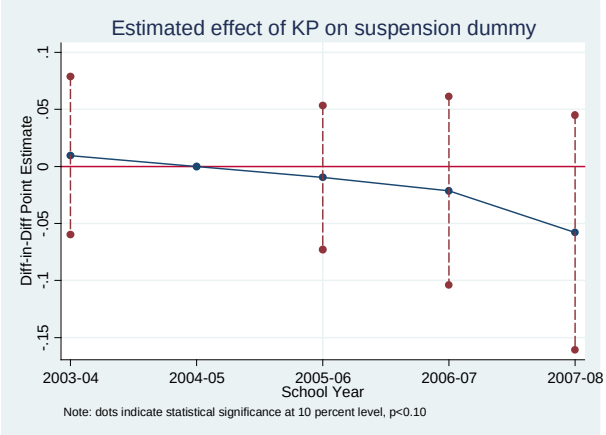
NOTE: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005–06). Panels A–C use the same specification as fixed effects regressions in Table 5. Dots around estimates indicate statistical significance at the 10 percent level, p<0.10.

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

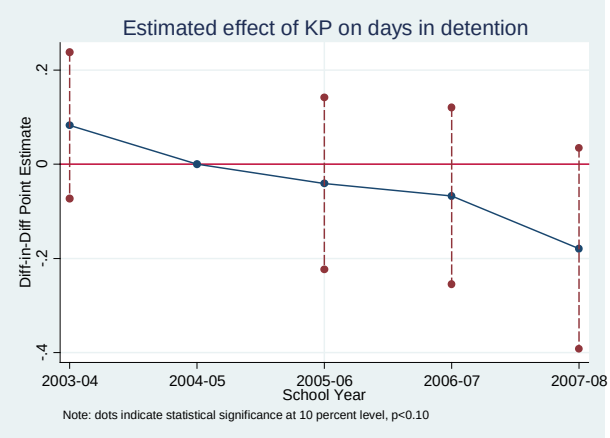
Panel A



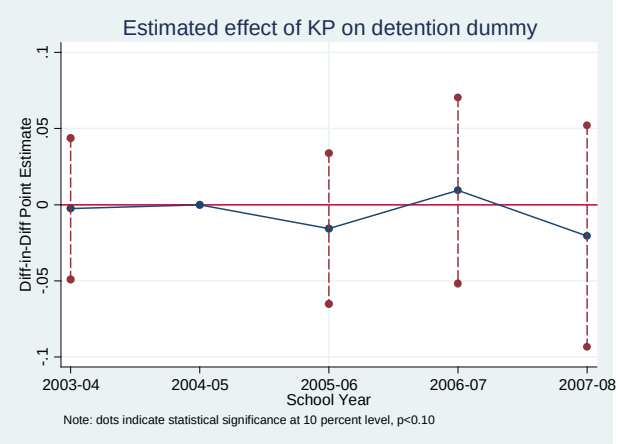
Panel B



Panel C



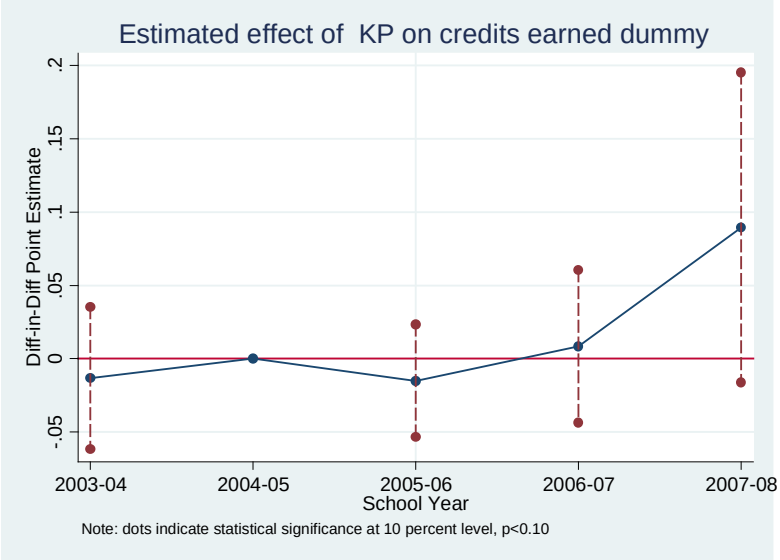
Panel D



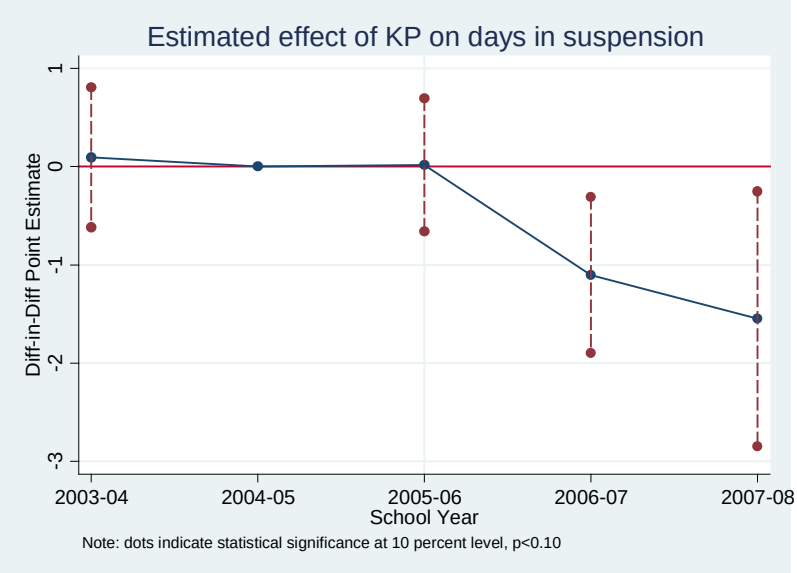
NOTE: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005–06). Panels A–D use the same specification as fixed effects regressions in Table 6. Dots around estimates indicate statistical significance at the 10 percent level, p<0.10.

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

Panel A



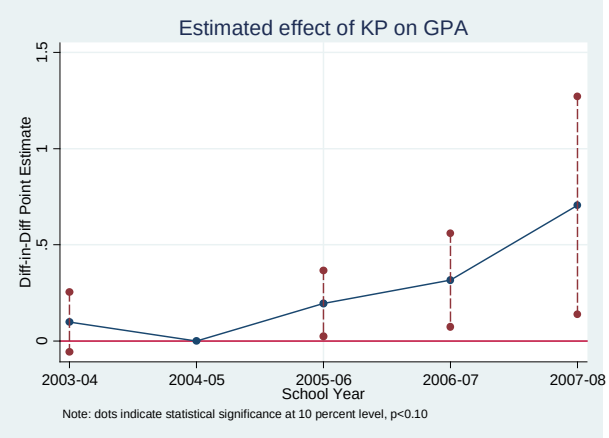
Panel B



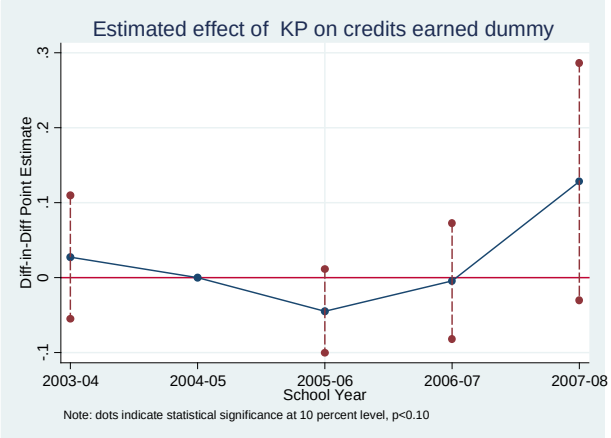
NOTE: The Kalamazoo Promise was announced on November 10, 2005 (school year 2005–06). In both specifications we drop the all the new enrollees since (and including 2005–06) and all of those who changes their eligibility level after and including 2005–06. This sample consists of 13,392 observations.

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

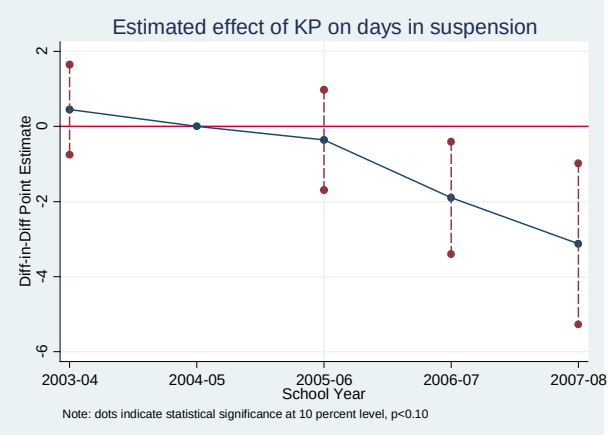
Panel A



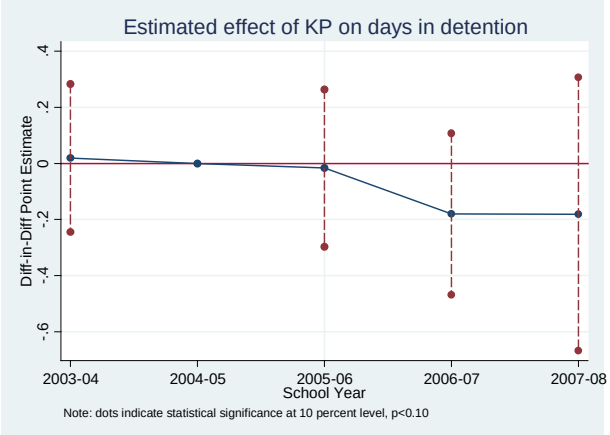
Panel B



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

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

Tuition Subsidy Group	Present Value (1)	Present Value (2)
0 %	$0.0	$0.0
65%	$17,818.2	$21,839.0
70%	$19,188.9	$23,518.9
75%	$20,559.5	$25,198.9
80%	$21,930.1	$26,878.8
85%	$23,300.8	$28,558.7
90%	$24,671.4	$30,238.6
95%	$26,042.0	$31,918.6
100%	$27,412.7	$33,598.5

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 2006–07of 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 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.

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 parenthesis), 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)
Enrolled in AP class (0/1)	0.03	0.07	0.05	0.10
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: Most students taking an AP course sign up for two credits during a school year, for both the fall and spring, out of eight possible credits. "Days suspended" is days of out-of-school suspension during the school year. GPA average is computed on the 4-point scale (A=4.0, B=3.0, C=2.0, D=1.0, F=0). The number of observations is the number of student-year cells used in computing the above statistics.
SOURCE: KPS.

Table 5 Estimated Effect of the Kalamazoo Promise on Academic Achievement

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
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 effect size is calculated by dividing the coefficient from the each regression by the standard deviation of dependent variable in the control year (school year 2004–05). Universe: Students enrolled in KPS in grades 9–12 during school years 2003–04 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
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.