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

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ABSTRACT

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

JEL Classification Codes: I21, I22, I24

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

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Appendix A (Not intended for print publication): Data

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

1. KPS ADMINISTRATIVE RECORDS AND VARIABLE DEFINITIONS

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

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

1.1 Definition of Kalamazoo Promise Eligibility

We use the KPS student entry and withdrawal records to construct our key explanatory variable, the indicator for whether a high school graduate is eligible for the Kalamazoo Promise. The KPS school tenure records go back to the 1996–1997 school year, which allows us to track continuous enrollment histories for most students, although earlier cohorts have truncated histories. For example, for the high school class of 2003, we observe records back to sixth grade (if the student graduated on time), but not earlier.

For the graduates from the classes of 2003 through 2005—the pre-Promise period—we define an eligibility variable that equals one if the student would have been eligible for any tuition subsidy (65 percent or more) had the Kalamazoo Promise been in effect at that time, and zero otherwise. We define a student as eligible if he or she resided in the district and had been continuously enrolled in KPS from before the fall count date in ninth grade. If the student enrolled after the ninth-grade fall count date, or was not a district resident, we count that student as ineligible in accord with Promise rules. In the post-Promise period (2006–2013), we use administrative records directly from the Kalamazoo Promise. In Appendix D, we present results where we use the algorithm to determine eligibility throughout the entire sample.

Appendix Table A1 compares Promise eligibility based on administrative data to Promise eligibility based on the algorithm and shows a close match: out of 3,947 observations in the post-Promise period, 3,400 are scored as eligible by both the algorithm and the administrative data and 371 are scored as ineligible by both the algorithm and the administrative data. Only 5 observations show eligibility under the algorithm but not in the administrative data. These cases appear to be special-education students who were not district residents; in our data, the special education code takes precedence over the code identifying resident status. For 171 observations, the algorithm finds that these students should not be eligible even though they apparently are. The majority of these are high-risk students who have moved in and out of KPS and thus had breaks in their continuous enrollment; however, about one-third are students who successfully appealed their eligibility with Promise administrators. We show below that our main results are robust to excluding cases where there is a disagreement between the algorithm and the administrative records.

Appendix Table A1: Eligibility and Ineligibility Comparison between Administrative Data and Assignment Algorithm in the Post-Promise Period

Class year	Eligible by admin. data, ineligible by algorithm	Ineligible by admin. data, eligible by algorithm	Ineligible by both admin. data and algorithm	Eligible by both admin. data and algorithm	Total number of observations
2006	22	2	59	366	449
2007	28	1	41	434	504
2008	16	1	53	414	484
2009	31	1	45	389	466
2010	21	0	46	431	498
2011	26	0	48	433	507
2012	16	0	49	461	526
2013	11	0	30	472	513
Total	171	5	371	3,400	3,947

1.2 Free or Reduced Lunch Program Participation in 2003

Because the KPS data do not record whether students participated in the federal assisted lunch program in 2003, we impute this variable using data from the other two pre-Promise years, 2004 and 2005. Using data for these two years, we predict the probability that a student is on the federal assisted lunch program by logistic regression of the lunch status dummy on a fully saturated (i.e., all possible interactions) vector of controls: gender, race/ethnicity, and high school indicator. The regression also includes achievement variables: cumulative school-year-level GPA as a cubic spline, cumulative AP classes taken, dummies for the highest math class taken, and a dummy for whether the student participated in the math and science magnet program.

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

2. DESCRIPTION OF NSC DATA

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

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

We are able to match more than 97 percent of the KPS graduates in our data to NSC records (Appendix Table A2).[1] Of the unmatched, nearly half are from 2003: for this year, NSC reports no graduates from KPS's alternative high school. Our estimates control for high school of graduation by year, so this exclusion is not problematic.

Appendix Table A2: Final Match Rates

Class year	Final Matched Estimation Sample	All KPS grads
2003	525	585
2004	551	552
2005	392	393
2006	449	457
2007	504	522

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

2008	484	501
2009	466	467
2010	498	500
2011	507	522
2012	526	531
2013	513	519
Total	5,415	5,549

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

Although the match rate is high, the NSC has shortcomings. As detailed by Dynarski, Hemelt, and Hyman (2015), NSC data do not cover all colleges, especially in the earlier period, and some records are blocked because of student or school requests under the Family Educational Rights and Privacy Act (FERPA). They show that coverage ranged from about 83 percent of students in 2003 to 90 percent in 2011. For Michigan colleges, coverage was slightly lower than for the nation in 2003 and slightly higher in 2011. For-profit institutions have lower coverage than other institution types.

The most relevant coverage issue for this paper is for Kalamazoo Valley Community College (KVCC), the local public two-year school, which approximately one-third of KPS graduates attend. KVCC did not provide student records to NSC before 2005. As a substitute, we obtained equivalent data for KPS graduates for the Summer 2003 through Summer 2005 period directly from KVCC upon special request, in cooperation with KPS and the Kalamazoo Promise. There are other schools for which NSC coverage began during our sample period, but none are (or were) attended in large numbers by KPS graduates.[2]

2.1 Construction of Outcome Variables

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

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

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

Appendix B (Not intended for print publication): Results Estimated Using Inverse Probability Weighting

To implement the inverse probability reweighting, we first estimate via logit the propensity score, $p(x) = \Pr(\text{Pre-Promise} = 1|x)$, where "Pre-Promise" equals 1 if the student graduated before the Promise, and is 0 otherwise.[3] We assign a weight, $w(x) = \frac{\hat{p}(x)}{1-\hat{p}(x)}$, if pre-Promise = 0, and $w(x)$ = 1 if pre-Promise = 1. We use the propensity score to reweight the students in the post-Promise period so that the distribution of covariates $\boldsymbol{x}$ of students in the post-Promise period resembles that of students in the pre-Promise period (DiNardo, Fortin, and Lemieux 1996). We perform this reweighting procedure separately for eligible and ineligible students. These $w(x)$ weights are then used to run weighted least squares difference-in-differences regressions. Table B1 shows how the distribution of covariates changes in the post-Promise period because of these weights.[4] The reweighting eliminates all significant differences in covariates between the pre-Promise and post-Promise periods for each eligibility group and helps to account for the fact that post-Promise, there is some negative selection (based on observables) into the group of eligible students. The reweighting is an empirically-based method of controlling for both linear and non-linear effects of observables upon outcomes. While this has advantages, it cannot control for unobservables.

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

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

Appendix Table B1 Descriptive Statistics, Reweighted Sample

	No reweighting				Reweighting			
Variable	Before	After	Diff	*p*-val	Before	After	Diff	*p*-val
Eligibles								
Male	0.470	0.477	0.007	0.660	0.470	0.470	−0.000	0.998
Black	0.346	0.416	0.070	0.000	0.346	0.346	0.001	0.972
Asian	0.017	0.026	0.009	0.047	0.017	0.016	−0.001	0.852
Hispanic	0.049	0.075	0.026	0.001	0.049	0.049	−0.001	0.919
White	0.584	0.473	−0.111	0.000	0.584	0.585	0.001	0.954
Subsid. lunch	0.340	0.547	0.207	0.000	0.340	0.339	−0.001	0.946
High school 1	0.491	0.525	0.035	0.036	0.491	0.491	0.001	0.963
High school 2	0.446	0.396	−0.050	0.002	0.446	0.446	−0.000	0.997
Ineligibles								
Male	0.442	0.439	−0.003	0.938	0.442	0.457	0.015	0.745
Black	0.481	0.532	0.051	0.220	0.481	0.498	0.018	0.709
Asian	0.056	0.037	−0.019	0.302	0.056	0.044	−0.011	0.609
Hispanic	0.086	0.077	−0.009	0.705	0.086	0.084	−0.001	0.961
White	0.369	0.340	−0.029	0.474	0.369	0.365	−0.004	0.922
Subsid. lunch	0.541	0.662	0.121	0.003	0.541	0.543	0.002	0.965
High school 1	0.399	0.546	0.146	0.000	0.399	0.403	0.003	0.937
High school 2	0.373	0.338	−0.035	0.374	0.373	0.388	0.015	0.745

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

Appendix Tables B2 through B5 present the IPW estimates, and compare these estimates with the original OLS estimates. In general, the IPW estimates are quite similar. Where they differ slightly, the IPW estimates tend to be somewhat larger in magnitude. Therefore, the OLS estimates presented in the text are conservative estimates of Promise effects to the extent that they do not fully account for (non-linear) negative selection into the eligible group in the post-Promise period.

Appendix Table B2 Promise Effects on Enrollment: OLS and IPW

	(1)	(2)
Panel A: Enrollment within 6 months		
(Mean of DV | after=0, elig.=1) = 0.612		
After × Eligible	0.083**	0.093**
	[0.042]	[0.045]
R^2	0.150	0.188
Panel B: Enrollment within 12 months		
(Mean of DV | after=0, elig.=1) = 0.673		
After × Eligible	0.059	0.074*
	[0.041]	[0.044]
R^2	0.164	0.206
Panel C: Enrollment at 4-yr. within 6 months		
(Mean of DV | after=0, elig.=1) = 0.402		
After × Eligible	0.094**	0.124***
	[0.038]	[0.040]
R^2	0.184	0.193
Panel D: Enrollment at 4-yr. within 12 months		
(Mean of DV | after=0, elig.=1) = 0.411		
After × Eligible	0.089**	0.121**
	[0.039]	[0.041]
R^2	0.187	0.193
Use IPW?	N	Y

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

Appendix Table B3 Promise Effects on Enrollment by Type of School: OLS and IPW

	(1)	(2)
Panel A: Enroll at a Promise school within 6 months		
(Mean of DV | after=0, elig.=1) = 0.480		
After × Eligible	0.178***	0.182***
	[0.042]	[0.046]
R^2	0.138	0.161
Panel B: Enroll at a 4-yr. Promise school within 6 months		
(Mean of DV | after=0, elig.=1) = 0.281		
After × Eligible	0.168***	0.195***
	[0.035]	[0.037]
R^2	0.161	0.172
Panel C: Enroll at a 4-yr. non-Promise school within 6 months		
(Mean of DV | after=0, elig.=1) = 0.132		
After × Eligible	−0.095***	−0.089***
	[0.023]	[0.023]
R^2	0.041	0.042
Use IPW?	N	Y

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

Appendix Table B4 Promise Effects on Credits Attempted: OLS and IPW

	(1)	(2)
Panel A: Credits attempted at 2 years		
(Mean of DV | after=0, elig.=1) = 25.00		
After × Eligible	3.24**	4.05**
	[1.65]	[1.78]
R^2	0.202	0.217
Panel B: Credits attempted at 3 years		
(Mean of DV | after=0, elig.=1) = 36.11		
After × Eligible	4.31*	5.70**
	[2.47]	[2.61]
R^2	0.215	0.226
Panel C: Credits attempted at 4 years		
(Mean of DV | after=0, elig.=1) = 46.59		
After × Eligible	6.56*	8.80**
	[3.36]	[3.51]
R^2	0.209	0.218
Use IPW?	N	Y

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

Appendix Table B5 Promise Effects on Degree Attainment: OLS and IPW

	(1)	(2)
Panel A: Any credential at 4 years		
(Mean of DV | after=0, elig.=1) = 0.186		
After × Eligible	0.008	0.016
	[0.031]	[0.034]
R^2	0.087	0.076
Panel B: Any credential at 6 years		
(Mean of DV | after=0, elig.=1) = 0.360		
After × Eligible	0.102**	0.121**
	[0.046]	[0.051]
R^2	0.146	0.132
Panel C: BA/BS at 4 years		
(Mean of DV | after=0, elig.=1) = 0.143		
After × Eligible	0.001	0.007
	[0.023]	[0.027]
R^2	0.116	0.101
Panel D: BA/BS at 6 years		
(Mean of DV | after=0, elig.= 1) = 0.300		
After × Eligible	0.074*	0.094**
	[0.040]	[0.045]
R^2	0.179	0.160
Use IPW?	N	Y

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

Appendix C (Not intended for print publication): Other Results Estimated Using the Between-District MCER Analysis Sample

This appendix presents a more complete set of results for the between-district estimates of the effects of the Kalamazoo Promise. Appendix Table C1 presents pre- and post-Promise summary statistics for KPS, other districts in the Middle Cities Education Association (MCEA), and other Michigan districts statewide.

Appendix Table C2 present difference-in-differences estimates of Promise effects, comparing KPS to MCEA districts (first two panels), and KPS to all other Michigan districts (last two panels). The MCEA includes most predominantly urban districts in the state of Michigan, except Detroit Public Schools.

In Appendix Figures C1 through C6, we use the cross-district data to analyze Promise effects using the synthetic control method of Abadie, Diamond, and Hainmueller (2010). The synthetic control method is motivated by the observation that a weighted average of non-treated districts can be a better counterfactual of a treated district than any single district. In practice, the synthetic control method allows us to use data-driven methods to select a suitable weighted average of comparison districts. The specific protocol is described below.

Table C1 Descriptive Statistics for KPS, MCEA districts, and all other Michigan districts

	KPS			MCEA districts other than KPS			All other districts		
	Post-period	Pre-period	Overall	Post-period	Pre-period	Overall	Post-period	Pre-period	Overall
Enrollment in 4 year within 6 months	45.7%	35.9%	43.0%	26.4%	22.8%	25.4%	38.6%	31.8%	36.8%
Enrollment in public 4 year within 6 months	41.6%	26.2%	37.4%	18.5%	17.1%	18.1%	27.9%	24.3%	27.0%
Enrollment in flagship within 6 months	12.7%	6.4%	11.0%	4.0%	4.7%	4.2%	8.0%	8.0%	8.0%
Enrollment in other 4 year within 6 months	4.2%	9.8%	5.7%	7.9%	5.6%	7.3%	10.7%	7.5%	9.9%
Degree completion within 6 years	37.1%	35.1%	36.1%	25.5%	26.9%	26.1%	36.4%	36.3%	36.3%
Bachelor's completion within 6 years	31.6%	30.2%	30.9%	19.3%	20.0%	19.6%	29.8%	29.1%	29.5%
Student teacher ratio	18.4	16.9	18.0	18.9	28.6	21.5	19.8	24.4	21.0
Percent FRL	56.4%	47.4%	53.9%	52.1%	38.4%	48.3%	32.6%	23.2%	30.2%
Percent Black	46.3%	42.8%	45.3%	39.5%	34.3%	38.1%	15.4%	13.5%	14.9%
Percent White	40.1%	47.3%	42.1%	51.4%	58.0%	53.2%	76.5%	80.2%	77.4%
Percent Hispanic	7.7%	6.5%	7.4%	5.7%	4.9%	5.5%	3.8%	2.8%	3.5%
Percent other non-White	5.9%	3.4%	5.2%	2.9%	2.6%	2.8%	4.0%	3.3%	3.8%

SOURCE: Authors' calculations using the Michigan Consortium for Educational Research (MCER) data of district by year averages.

Table C2 Promise Effects on Enrollment and Completion using Between-District Analysis (MCEA and all districts, with and without linear time trends)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Enrollment within 6 months at:				Credential at 6 years:	
	4-yr school	4-yr.Promise school	4-yr. non-Promise school	Flagship school	Any credential	BA/BS
MCEA: with district time trends						
After × KPS	0.071**	0.110**	−0.039*	0.085**	0.061	0.039
Robust standard error	[0.024]	[0.019]	[0.013]	[0.026]	[0.044]	[0.040]
Permutation *p*-value	0.032	0.032	0.065	0.032	0.258	0.258
Mean DV	0.259	0.181	0.0781	0.0414	0.238	0.179
MCEA: without district time trends						
After × KPS	0.059**	0.135**	−0.076**	0.072**	0.030	0.020
Robust standard error	[0.019]	[0.012]	[0.011]	[0.010]	[0.018]	[0.015]
Permutation *p*-value	0.032	0.032	0.032	0.032	0.161	0.194
All districts: with district time trends						
After × KPS	0.041	0.082**	−0.041*	0.080*	0.038	0.020
Robust standard error	[0.024]	[0.017]	[0.012]	[0.025]	[0.048]	[0.037]
Permutation *p*-value	0.116	0.031	0.055	0.055	0.308	0.382
Mean DV	0.328	0.236	0.092	0.054	0.331	0.257
All districts: without district time trends						
After × KPS	0.033	0.120**	−0.088**	0.066**	0.024	0.011
Robust standard error	[0.018]	[0.012]	[0.011]	[0.010]	[0.017]	[0.011]
Permutation *p*-value	0.153	0.025	0.027	0.025	0.210	0.288

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. p-value is obtained using a placebo-regression permutation inference described in the text. Regressions include district-by-year proportions of students to teachers, students eligible for subsidized lunch, white students, nonwhite students, and Hispanic students. For observations missing a covariate, we include a dummy for missing and assign the sample mean. The regressions control for district fixed effects and year-of-graduation time effects. Observations are weighted by the number of graduates in each district-year. The mean of the dependent variable is for the control districts in the pre-Promise period. The control districts consist either of the Michigan Middle Cities Education Association (MCEA) districts (top two panels) or of all districts in Michigan (bottom two panels).

Synthetic Control Protocol

We begin with the potential synthetic control donor pool of 511 districts. We keep only those with a full balanced panel across years, which reduces the set to 315 districts. Taking averages across time, we drop districts whose values for the outcomes and student-teacher ratio, percent free or reduced- price lunch (FRL), and percent white differ by more than 2.43 standard deviations of the statewide distribution from KPS's values, leaving a donor pool of 100 districts. Most of the districts trimmed from the donor pool have either high concentrations of whites or very low concentrations of FRL students; however, a few very highly nonwhite and high FRL districts are also trimmed. Districts whose share of FRL students were within [0.039, 0.909] and whose white student share were within [0.021, 0.924] are kept; these two variables drive much of the trimming.

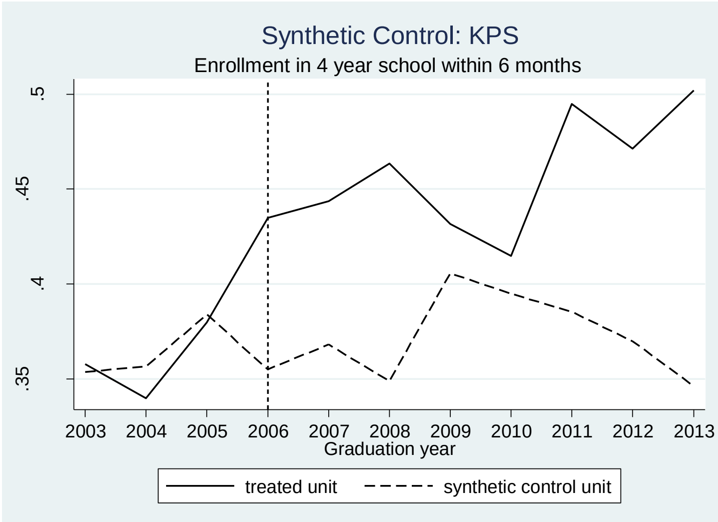
We use the "synth" command in Stata, with KPS as the treated unit, selecting the 2003 and 2005 values of the outcome variable and the average values over 2003–2005 of the student-teacher ratio, percent FRL, and percent white as the matching variables.

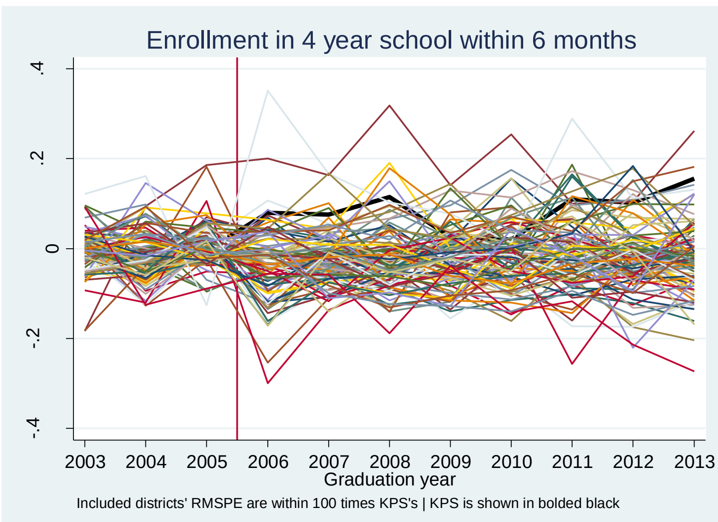
For inference, we repeat this procedure, assigning each of the other donor pool districts to be the treated unit. We collect both the treated unit's treatment effect and the synthetic control's treatment effect for each run of "synth" and calculate their difference for each year, along with the root mean squared predicted errors (RMSPE).

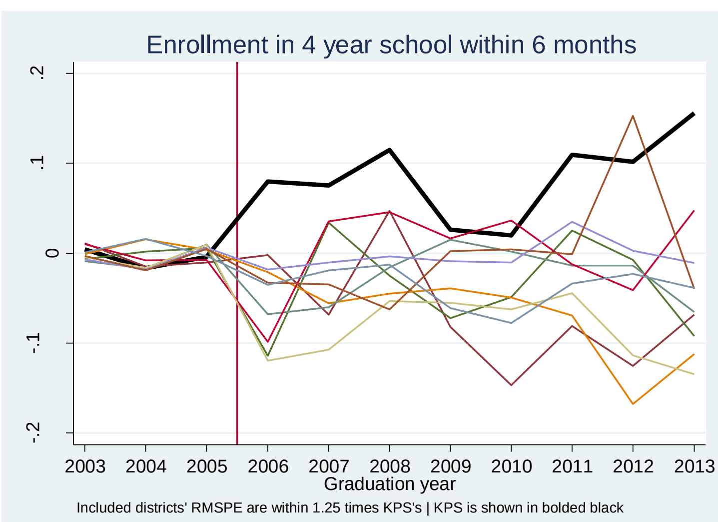
Finally, we plot the net effect of the Promise across time for all the districts whose RMSPE is less than a specified cutoff in terms of multiples of KPS's RMSPE. Our preferred cutoff is 1.25 x KPS's RMSPE. Below, we present result figures from this analysis.

The results are roughly consistent with the between-district regression analysis. For college enrollment outcomes, synthetic controls closely match KPS before the Promise, and Promise effects are unusually high relative to those of placebo districts. For degree completion outcomes, synthetic controls match KPS less closely before the Promise, and effects, while positive, are noisier, and inference is less conclusive.

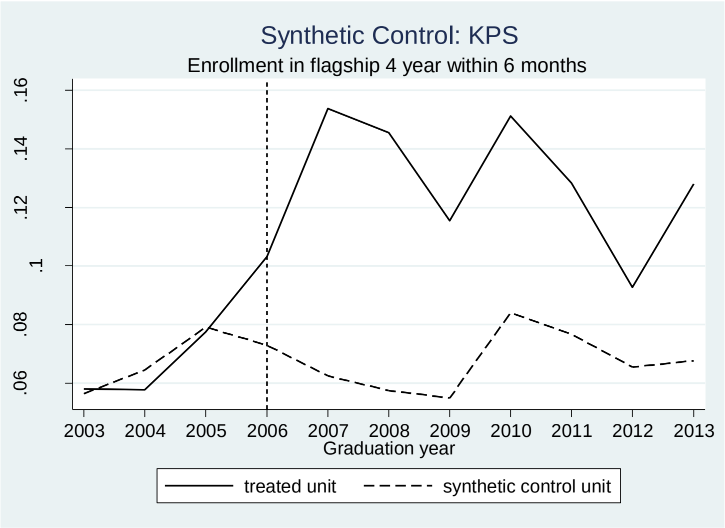
Appendix Figure C1: Enrollment in 4-year college within six months of HS graduation

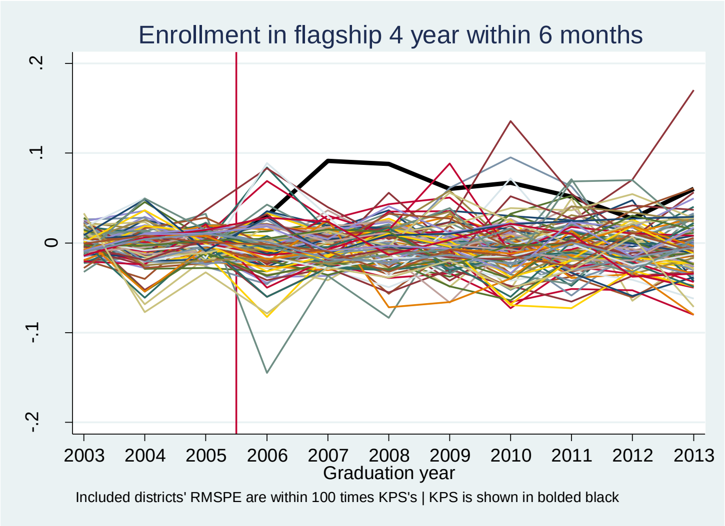


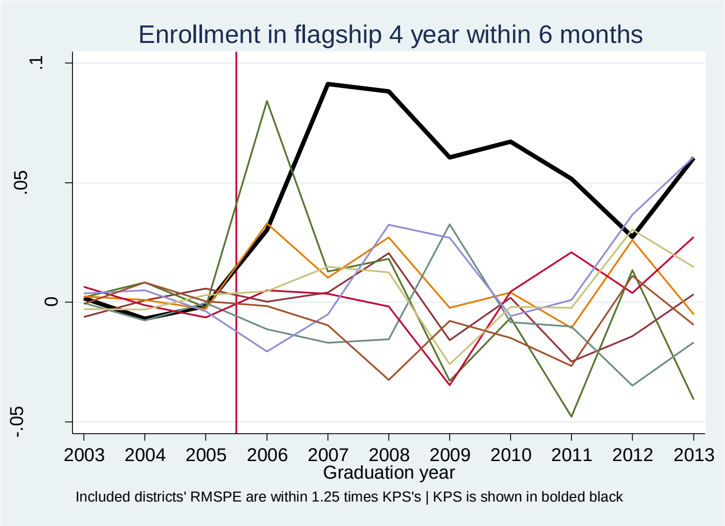




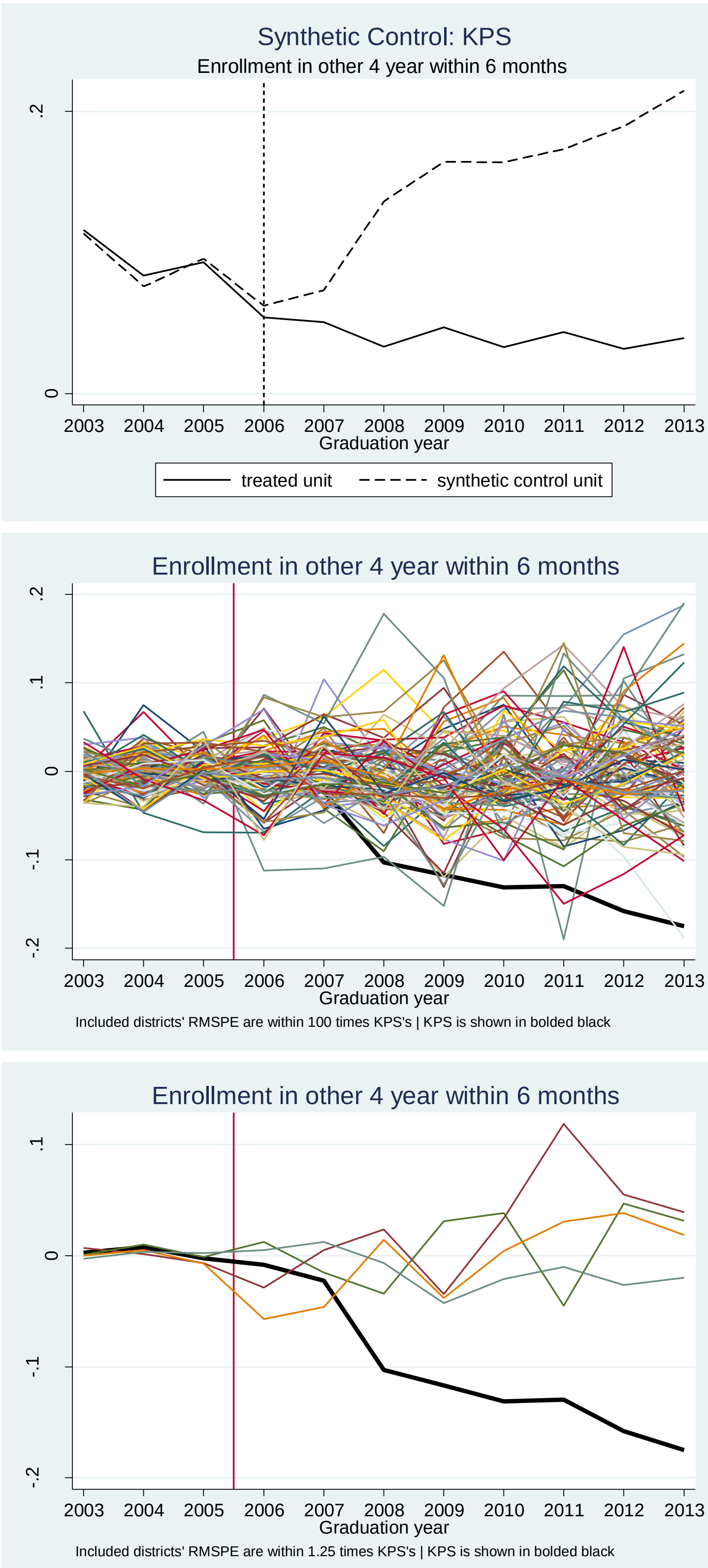
Appendix Figure C2: Enrollment in MSU or UM-AA within six months of HS graduation



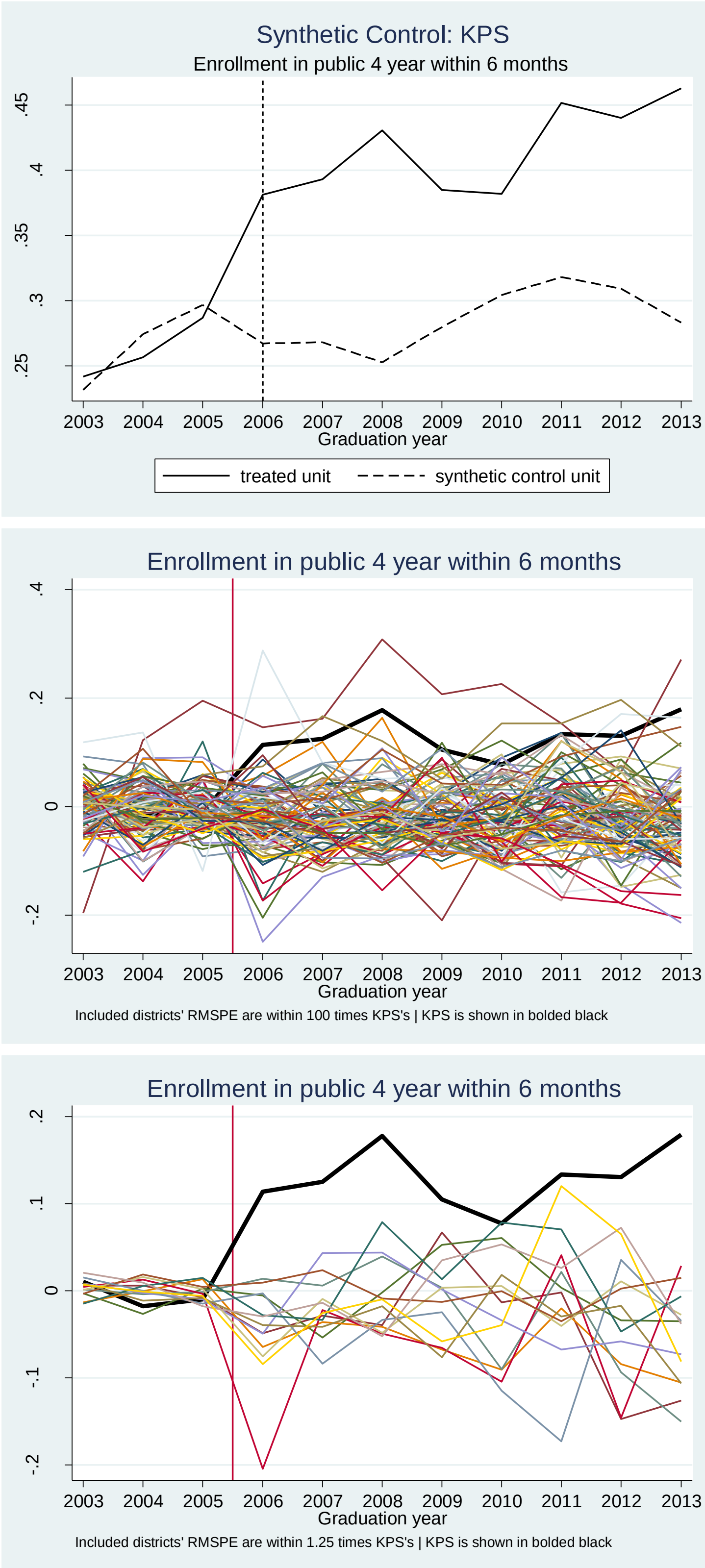




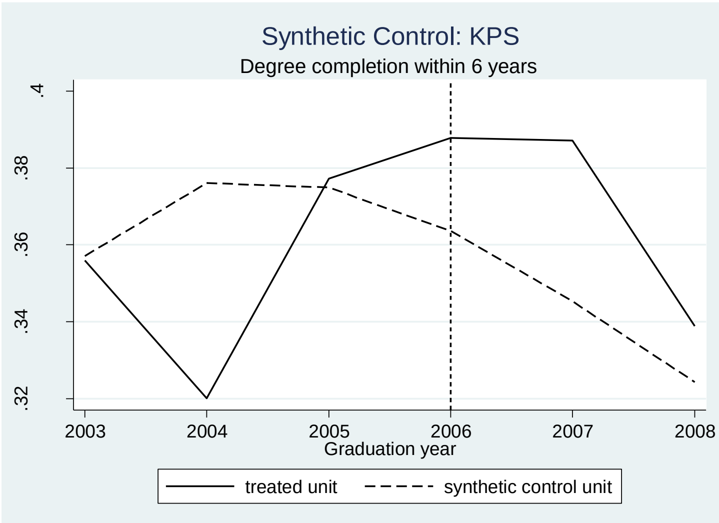
Appendix Figure C3: Enrollment in non-Promise 4-year within 6 months of HS graduation

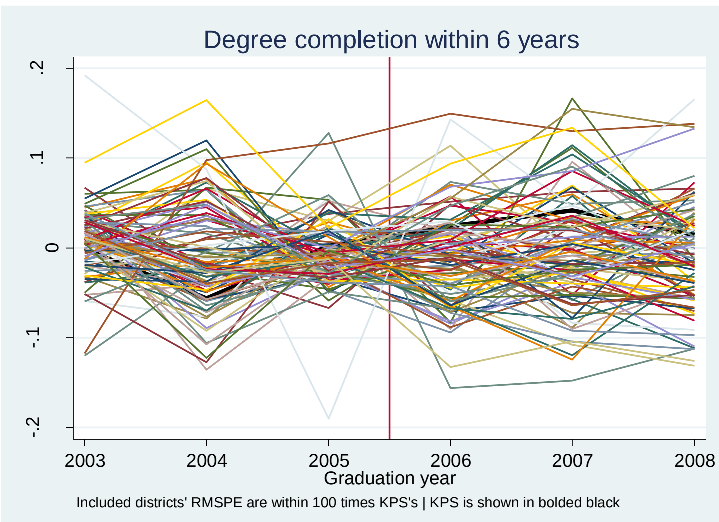


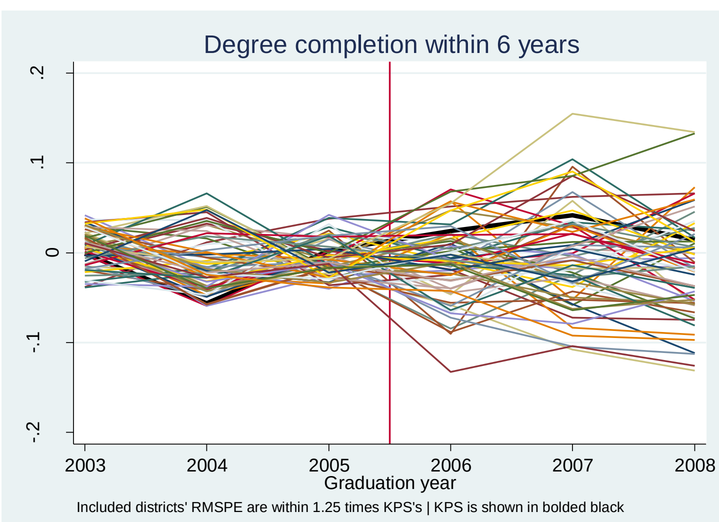
Appendix Figure C4: Enrollment in Promise 4-year within 6 months of HS graduation



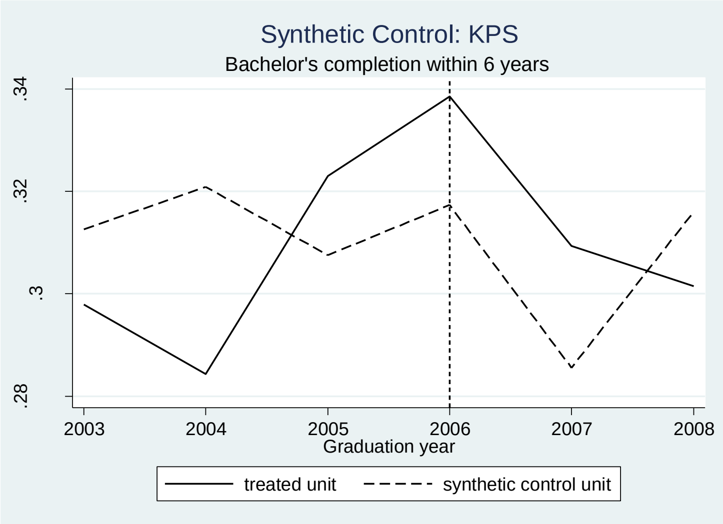
Appendix Figure C5: Any degree completion within six years of HS graduation

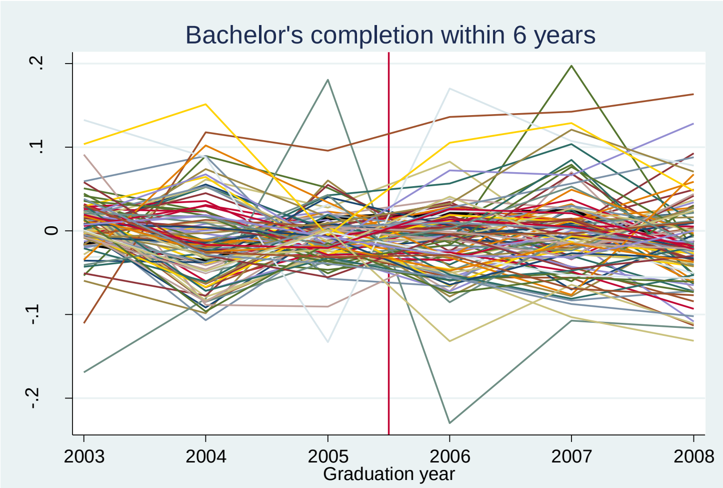




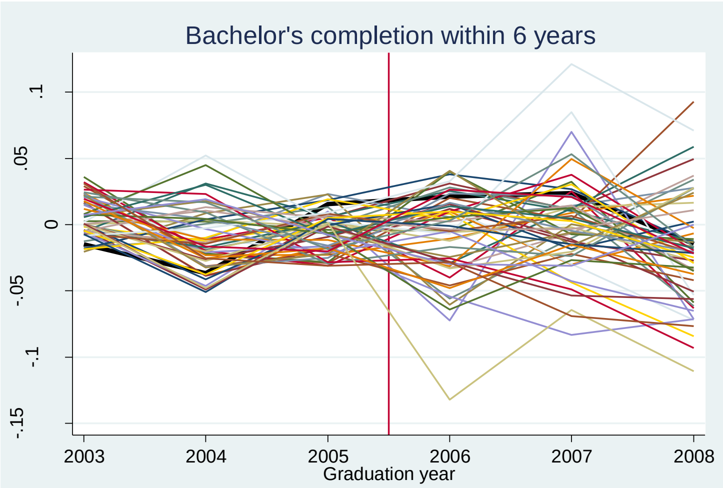


Appendix Figure C6: Bachelor's completion within six years of HS graduation





Included districts' RMSPE are within 100 times KPS's | KPS is shown in bolded black



Included districts' RMSPE are within 1.25 times KPS's | KPS is shown in bolded black

Appendix D (Not intended for print publication): Other robustness checks

This appendix presents further robustness checks that include estimates from models that allow student characteristics to affect outcomes differentially before and after the Promise and estimates from models that use an alternative definition of the eligibility indicator.

The first column of each table in this appendix presents the estimated effect of the Promise using the baseline eligibility indicator, i.e., the indicator obtained from administrative data. The second column in each table shows the estimated effect of the Promise using the baseline eligibility indicator but allowing student characteristics to vary with the outcome over time by interacting the characteristics with an "After" dummy.

As shown in Appendix Table A1, there is slight disagreement between Promise eligibility as determined by the algorithm (predicted eligibility) and eligibility as determined by the administrative data. In the third column of each table, we show the estimated effect of the Promise using the algorithm-based eligibility indicator. Lastly, the fourth column presents estimates that use the algorithm-based eligibility indicator and that additionally interact student characteristics with the "After" dummy.

We expect the algorithm-only results to be biased toward zero, as this specification treats as ineligible some students who are in fact eligible. These 171 exceptions are disproportionately disadvantaged: 68 percent are African-American, 81 percent are eligible for assisted lunch, and 28 percent graduated from the alternative high school; for students for whom both the administrative data and algorithm find ineligible, the respective numbers are 51 percent, 61 percent, and 16 percent. However, the exceptions may be especially motivated to attend college, even if their characteristics make them unlikely to complete it. This observable selection—that the exception students are marginal on college enrollment but likely not on completion—

suggests that algorithm results (relative to the baseline results) should be more attenuated for the enrollment outcomes than completion outcomes.

Indeed, the appendix tables generally show this pattern: lower magnitudes of Promise effects for the enrollment-only eligibility results, but for the critical six-year completion results, the estimates are quite similar using either eligibility indicator.

With respect to time-varying coefficients, the estimates are in general little affected by this change.

Appendix Table D1 Promise Effects on Enrollment: Robustness Checks

	(1)	(2)	(3)	(4)
Panel A: Enrollment within 6 months (Mean of DV | after=0, elig.=1) = 0.612				
After × Eligible	0.083**	0.106**	0.049	0.076*
	[0.042]	[0.042]	[0.039]	[0.040]
R^2	0.150	0.156	0.150	0.156
Panel B: Enrollment within 12 months (Mean of DV | after=0, elig.=1) = 0.673				
After × Eligible	0.059	0.084**	0.016	0.044
	[0.041]	[0.041]	[0.039]	[0.039]
R^2	0.164	0.172	0.161	1.700
Panel C: Enrollment at 4-yr. within 6 months (Mean of DV | after=0, elig.=1) = 0.402				
After × Eligible	0.094**	0.096***	0.049	0.051
	[0.038]	[0.039]	[0.037]	[0.037]
R^2	0.184	0.185	0.182	0.183
Panel D: Enrollment at 4-yr. within 12 months (Mean of DV | after=0, elig.=1) = 0.411				
After × Eligible	0.089**	0.087**	0.042	0.040
	[0.039]	[0.040]	[0.037]	[0.038]
R^2	0.187	0.188	0.185	0.185
Eligibility Measure	Baseline	Baseline	Algorithm	Algorithm
X coefficients vary before and after?	No	Yes	No	Yes

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

Appendix Table D2 Promise Effects on Enrollment Sector: Robustness Checks

	(1)	(2)	(3)	(4)
Panel A: Enroll at a Promise school within 6 months				
(Mean of DV | after=0, elig.=1) = 0.480				
After × Eligible	0.178***	0.192***	0.134***	0.151***
	[0.042]	[0.043]	[0.040]	[0.041]
R^2	0.138	0.142	0.136	0.141
Panel B: Enroll at a 4-yr. Promise school within 6 months				
(Mean of DV | after=0, elig.=1) = 0.281				
After × Eligible	0.168***	0.158***	0.115***	0.106***
	[0.035]	[0.036]	[0.034]	[0.034]
R^2	0.161	0.162	0.158	0.159
Panel C: Enroll at a 4-yr. non-Promise school within 6 months				
(Mean of DV | after=0, elig.=1) = 0.132				
After × Eligible	−0.095***	−0.086***	−0.084***	−0.075***
	[0.023]	[0.023]	[0.021]	[0.021]
R^2	0.042	0.044	0.042	0.044
Eligibility Measure	Baseline	Baseline	Algorithm	Algorithm
X coefficients vary before and after?	No	Yes	No	Yes

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

Appendix Table D3 Promise Effects on School Choice: Robustness Checks

Panel A: Enroll at a given school within 6 months, Baseline

	KVCC	WMU	MSU	UM	Flagships	K
After × Eligible	0.011	0.072**	0.056***	0.010	0.066***	0.003
(fixed coefficients)	[0.038]	[0.029]	[0.015]	[0.012]	[0.019]	[0.010]
After × Eligible	0.033	0.085**	0.045***	0.006	0.051***	0.008
(time-varying coefficients)	[0.038]	[0.030]	[0.015]	[0.011]	[0.018]	[0.010]

Panel B: Enroll at a given school within 6 months, Algorithm

	KVCC	WMU	MSU	UM	Flagships	K
After × Eligible	0.022	0.049*	0.046***	0.003	0.049***	0.002
(fixed coefficients)	[0.036]	[0.028]	[0.015]	[0.011]	[0.018]	[0.010]
After × Eligible	0.046	0.063**	0.035***	−0.001	0.034*	0.008
(time-varying coefficients)	[0.036]	[0.029]	[0.015]	[0.011]	[0.018]	[0.010]

NOTE: Standard errors robust to heteroskedasticity are in parentheses ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. See note to Table 4A. KVCC stands for Kalamazoo Valley Community College, WMU stands for Western Michigan University, MSU stands for Michigan State University, UM stands for University of Michigan-Ann Arbor, Flagships stands for either MSU or UM, and K stands for Kalamazoo College.

Appendix Table D4 Promise Effects on Credits Attempted: Robustness Checks

	(1)	(2)	(3)	(4)
Panel A: Credits attempted at 2 years (Mean of DV | after=0, elig.=1) = 25.00				
After × Eligible	3.24** [1.65]	3.42** [1.66]	0.97 [1.56]	1.21 [1.58]
R^2	0.202	0.203	0.199	0.200
N	4,902	4,902	4,902	4,902
Panel B: Credits attempted at 3 years (Mean of DV | after=0, elig.=1) = 36.11				
After × Eligible	4.31* [2.47]	4.41* [2.49]	1.33 [2.32]	1.51 [2.35]
R^2	0.215	0.215	0.212	0.213
N	4,376	4,376	4,376	4,376
Panel C: Credits attempted at 4 years (Mean of DV | after=0, elig.=1) = 46.59				
After × Eligible	6.56* [3.36]	6.64* [3.39]	2.49 [3.17]	2.66 [3.21]
R^2	0.209	0.210	0.207	0.208
N	3,869	3,869	3,869	3,869
Eligibility Measure	Baseline	Baseline	Algorithm	Algorithm
X coefficients vary before and after?	No	Yes	No	Yes

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

Appendix Table D5 Promise Effects on Credentials Completed: Robustness Checks

	(1)	(2)	(3)	(4)
Panel A: Any credential at 4 years (Mean of DV | after=0, elig.=1) = 0.186				
After × Eligible	0.008 [0.031]	0.007 [0.031]	−0.028 [0.030]	−0.030 [0.030]
R^2	0.087	0.089	0.086	0.088
Panel B: Any credential at 6 years (Mean of DV | after=0, elig.=1) = 0.360				
After × Eligible	0.102** [0.046]	0.094** [0.046]	0.082* [0.043]	0.073* [0.043]
R^2	0.146	0.149	0.146	0.149
Panel C: BA/BS at 4 years (Mean of DV | after=0, elig.=1) = 0.143				
After × Eligible	0.001 [0.023]	0.002 [0.023]	−0.020 [0.022]	−0.019 [0.021]
R^2	0.116	0.119	0.115	0.118
Panel D: BA/BS at 6 years (Mean of DV | after=0, elig.= 1) = 0.300				
After × Eligible	0.074* [0.040]	0.067* [0.040]	0.056 [0.037]	0.047 [0.037]
R^2	0.179	0.181	0.179	0.181
Eligibility Measure	Baseline	Baseline	Algorithm	Algorithm
X coefficients vary before and after?	No	Yes	No	Yes

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

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Appendix E (not intended for print publication): Instrumental Variable Estimates of Promise Effects

In this appendix, we present estimates where we deal with discrepancies between the rules-based Promise eligibility indicator and actual Promise assignment through use of instrumental variables. The instrumental variable context is unusual here: in the pre-Promise period, there is no discrepancy between the two measures by construction: the Promise did not exist so no exceptions to the rules were possible. Furthermore, in the post-Promise period, almost all the "error" in Promise eligibility is one-sided, with some students who are ineligible under Promise rules being awarded eligibility by Promise administrators. Ordinary 2SLS procedures using the full sample would be inefficient—even more than usual—because they would mis-predict eligibility in the pre-Promise period, when we have perfect assignment.

To deal with this problem, we use a modified two-step instrumental variable procedure. First, we estimate the "effect" of Promise eligibility in the pre-Promise period using OLS (with heteroskedasticity-robust standard errors). That is, we calculate the difference in outcomes by Promise pseudo-eligibility, according to the eligibility rules, conditional on observables. Second, in the post-Promise period, we estimate the "effects" of Promise eligibility using 2SLS, where we instrument actual (administrative) eligibility with eligibility according to the rules-based algorithm.

We then calculate the difference between the 2SLS coefficient on Promise eligibility from the post-period and the OLS coefficient on Promise eligibility from the pre-period. The standard error of this difference is readily calculated from the standard errors in these two distinct samples.

This procedure is essentially a difference-in-differences procedure. However, it also implicitly allows for the coefficients of all the demographic controls to differ between the pre-

Promise period and the post-Promise period. As shown in Appendix D, this has little effect on the estimates.

Appendix Table E1 presents results from this procedure. These instrumental variable estimates are compared with the estimates from Appendix D that use our baseline eligibility indicator throughout, but also allow the coefficients on the demographic controls to differ between the pre-Promise and post-Promise period. These OLS estimates with time-varying observables are the most comparable to the IV estimates, as both methods allow the student demographics to vary differentially pre-Promise and post-Promise.

Appendix Table E1 Comparison between baseline eligibility estimates, and instrumental variable (IV) estimates of Promise effects

Dependent variable	(1) OLS estimates (with time-varying coefficients)	(2) IV estimates
Enrollment outcomes		
Enrollment within 6 months	0.106	0.152
	(0.042)	(0.046)
Enrollment within 12 months	0.084	0.105
	(0.041)	(0.044)
Enrollment at 4-yr. within 6 months	0.096	0.112
	(0.039)	(0.043)
Enrollment at 4-yr. within 12 months	0.087	0.095
	(0.040)	(0.043)
Enroll at a Promise school within 6 months	0.192	0.240
	(0.043)	(0.047)
Enroll at a 4-yr. Promise school within 6 months	0.158	0.176
	(0.036)	(0.040)
Enroll at a 4-yr. non-Promise school within 6 months	-0.086	-0.088
	(0.023)	(0.024)
Enroll at KVCC within 6 months	0.033	0.066
	(0.038)	(0.042)
Enroll at WMU within 6 months	0.085	0.096
	(0.030)	(0.033)
Enroll at MSU within 6 months	0.045	0.053
	(0.015)	(0.017)
Enroll at UM within 6 months	0.006	0.004
	(0.011)	(0.013)
Enroll at flagship within 6 months	0.051	0.057
	(0.018)	(0.021)
Enroll at K College within 6 months	0.008	0.012
	(0.010)	(0.010)
Credits attempted		
Credits attempted at 2 years	3.420	3.844
	(1.660)	(1.815)
Credits attempted at 3 years	4.410	5.843
	(2.490)	(2.754)
Credits attempted at 4 years	6.640	8.393
	(3.390)	(3.802)
Credentials obtained		
Any credential at 4 years	0.007	-0.024
	(0.031)	(0.036)

Any credential at 6 years	0.094 (0.046)	0.126 (0.052)
BA/BS at 4 years	0.002 (0.023)	0.001 (0.026)
BA/BS at 6 years	0.067 (0.040)	0.097 (0.046)

NOTE: The estimates in column (1) are the baseline estimates from Appendix D allowing the demographic controls to have varying coefficients before and after the Promise. These estimates are compared with instrumental variable (IV) estimates in column (2). Both sets of estimates condition on observables: sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. Heteroskedasticity-robust standard errors are in parentheses. KVCC stands for Kalamazoo Valley Community College, WMU stands for Western Michigan University, MSU stands for Michigan State University, UM stands for University of Michigan-Ann Arbor, Flagships stands for either MSU or UM, and K stands for Kalamazoo College.

As shown in Table E1, the instrumental variable procedure unsurprisingly tends to increase standard errors for the usual reasons. More salient, the instrumental variable estimates themselves are somewhat greater in magnitude than the baseline.

In particular, our bottom-line outcomes “Any credential at 6 years” and “BA/BS at 6 years” both increase in magnitude by about one-third. The instrumental variable estimates suggest that the Promise increases attainment of any credential after six years by 12.6 percentage points, and attainment of a BA or BS by 9.7 percentage points.

This instrumental variable procedure supports the hypothesis that the estimates presented in the main text are conservative.

Appendix F (not intended for print publication): Promise analysis restricted to 7th-9th grade entrant eligibles

In this appendix, we present estimates in which we restrict the sample of eligible students to those entering KPS in 7th through 9th grade (rather than all students who entered before 10th grade). As the ineligible group consists of students who first entered in 10th grade or later, by making the eligibles a group of earlier movers (rather than long-term KPS students), the intent of this restriction is to make the eligible group more comparable to the ineligible group. However, in practice, this restriction makes the eligible and ineligible groups less comparable. Students who enter KPS at 9th grade come disproportionately from private schools, due to the few private high schools in the Kalamazoo area. As a result, the 7th to 9th grade entrants are more advantaged than students who have been in KPS for a longer time and those who entered after 9th grade.

Table F1, which is the analogue to Table 2 in the main text, demonstrates this. For example, note the much higher fraction of Promise-eligible white students and the much lower fraction of Promise-eligible students eligible for subsidized lunch. This sample restriction implies that the estimated average treatment effect of the Promise is on a more-advantaged population, one that is less marginal on many college success measures and one that is not representative of the actual treated population. Additionally, the restriction diminishes the sample size of the eligible group dramatically, hurting precision. We thus believe that estimates based on this sample are less compelling in calculating an average treatment effect.

It is therefore not surprising that in subsequent tables F2–F6 almost all Promise effects are severely attenuated, except for substitution among schools. These point estimates are effectively estimating a different parameter than in our main estimates, and it is questionable whether this parameter is the policy-relevant one.

Table F1 Descriptive statistics of sample restricted to 7th-9th grade entrant eligibles

		Before		*After*		*DD*
Variable	All	Eligibles	Ineligibles	Eligibles	Ineligibles	[standard error]
Demographics						
Male	0.452	0.377	0.442	0.493	0.439	0.119 [0.062]*
Black	0.375	0.188	0.481	0.258	0.532	0.018 [0.056]
Asian	0.054	0.052	0.056	0.068	0.037	0.035 [0.028]
Hispanic	0.084	0.071	0.086	0.092	0.077	0.029 [0.034]
White	0.478	0.688	0.369	0.574	0.340	–0.086 [0.059]
Subsid. lunch	0.505	0.266	0.541	0.446	0.662	0.058 [0.059]
High school 1	0.484	0.396	0.399	0.505	0.545	–0.037 [0.062]
High school 2	0.422	0.591	0.373	0.458	0.338	–0.097 [0.061]
N	1,232	154	233	469	376	

NOTE: Numbers represent authors’ calculations of demographic characteristics of KPS graduates for the classes of 2003 through 2013 (excluding alternative education programs). From 2006 onward, eligibility is taken from administrative records from the Kalamazoo Promise. Before 2006, eligibility is assigned based on Promise rules had the Promise been in effect for those cohorts. “Before” represents the cohorts 2003 through 2005; “After” represents cohorts 2006 through 2013. “DD” represents the difference between eligibles after and before the Promise and ineligibles after and before the Promise; results are not appreciably affected if we use the full specification described by equation (1) below. Standard errors are robust to heteroskedasticity.

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

Table F2 Promise effects on enrollment using sample restricted to 7th-9th grade entrant eligibles

	(1)
Panel A: Enrollment within 6 months (Mean of DV | after=0, elig.=1) = 0.688	
After × Eligible	−0.011 [0.060]
R^2	0.196
Panel B: Enrollment within 12 months (Mean of DV | after=0, elig.=1) = 0.747	
After × Eligible	−0.026 [0.058]
R^2	0.212
Panel C: Enrollment at 4-yr. within 6 months (Mean of DV | after=0, elig.=1) = 0.519	
After × Eligible	0.041 [0.059]
R^2	0.217
Panel D: Enrollment at 4-yr. within 12 months (Mean of DV | after=0, elig.=1) = 0.532	
After × Eligible	0.039 [0.059]
R^2	0.219

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for eligible population in the pre-Promise period. Sample size is 1,232.

Table F3 Promise effects on enrollment by type of school using sample restricted to 7th-9th grade entrant eligibles

	(1)
Panel A: Enroll at a Promise school within 6 months	
(Mean of DV | after=0, elig.=1) = 0.481	
After × Eligible	0.123*
	[0.064]
R^2	0.142
Panel B: Enroll at a 4-yr. Promise school within 6 months	
(Mean of DV | after=0, elig.=1) = 0.312	
After × Eligible	0.167***
	[0.056]
R^2	0.163
Panel C: Enroll at a 4-yr. non-Promise school within 6 months	
(Mean of DV | after=0, elig.=1) = 0.208	
After × Eligible	−0.134***
	[0.041]
R^2	0.067

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for the eligible population in the pre-Promise period. Sample size is 1,232.

Table F4 Promise effects on college first attended using sample restricted to 7th-9th grade entrant eligibles

Panel A: Enroll at a given school within 6 months

	KVCC	WMU	MSU	UM	Flagships	K
After × Eligible	−0.025	0.070	0.047*	−0.006	0.041	−0.027
	[0.053]	[0.046]	[0.027]	[0.026]	[0.037]	[0.022]
Mean of DV	0.149	0.149	0.052	0.071	0.123	0.065

Panel B: Enroll at a given school within 12 months

	KVCC	WMU	MSU	UM	Flagships	K
After × Eligible	−0.050	0.074	0.049*	−0.006	0.043	−0.027
	[0.057]	[0.047]	[0.027]	[0.026]	[0.037]	[0.022]
Mean of DV	0.195	0.156	0.052	0.071	0.123	0.065

NOTE: Standard errors robust to heteroskedasticity are in parentheses ***, **, and * indicate p < 0.01, 0.05, or 0.10. See note to Table 4A. KVCC stands for Kalamazoo Valley Community College, WMU stands for Western Michigan University, MSU stands for Michigan State University, UM stands for University of Michigan-Ann Arbor, Flagships stands for either MSU or UM, and K stands for Kalamazoo College.

Table F5 Promise Effects on credits attempted using sample restricted to 7th-9th grade entrant eligibles

	(1)
Panel A: Credits attempted at 2 years (Mean of DV | after=0, elig.=1) = 29.41	
After × Eligible	0.01 [2.54]
R^2	0.242
N	1,137
Panel B: Credits attempted at 3 years (Mean of DV | after=0, elig.=1) = 42.97	
After × Eligible	0.74 [3.84]
R^2	0.264
N	1,011
Panel C: Credits attempted at 4 years (Mean of DV | after=0, elig.=1) = 56.75	
After × Eligible	−0.12 [5.32]
R^2	0.251
N	880

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

Table F6 Promise effects on degree attainment using sample restricted to 7th-9th grade entrant eligibles

	(1)
Panel A: Any credential at 4 years	
(Mean of DV | after=0, elig.=1) = 0.260	
After × Eligible	−0.002
	[0.053]
R^2	0.13
Panel B: Any credential at 6 years	
(Mean of DV | after=0, elig.=1) = 0.448	
After × Eligible	0.075
	[0.071]
R^2	0.17
Panel C: BA/BS at 4 years	
(Mean of DV | after=0, elig.=1) = 0.247	
After × Eligible	−0.038
	[0.047]
R^2	0.181
Panel D: BA/BS at 6 years	
(Mean of DV | after=0, elig.= 1) = 0.422	
After × Eligible	0.019
	[0.065]
R^2	0.223

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Outcome timing is since high school graduation. Regressions include dummies for after the Promise, individual (pseudo-)eligibility, sex, race/ethnicity, free/reduced-price lunch status, and high school of graduation-by-graduation year. The mean of the dependent variable is for the eligible population in the pre-Promise period. Sample sizes are 880 at four years and 701 at six years.