Third-Year Report: Evaluating the Effects of the Say Yes to Education Program in Buffalo, NY

Robert Bifulco

Ross Rubenstein

Hosung Sohn

Jud Murchie

Prepared for the Say Yes to Education Foundation

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EXECUTIVE SUMMARY

This report presents findings from the third year of the external evaluation of Say Yes to Education in Buffalo, NY. More specifically, the report examines the following questions:

1) How did Say Yes affect school enrollments in Buffalo and Syracuse?
2) How did Say Yes affect housing prices in Buffalo and Syracuse?
3) How did Say Yes affect elementary and middle grade standardized test scores in Buffalo
4) How did Say Yes affect high school graduation, college matriculation and college persistence in Buffalo?

Enrollment and Housing Prices

Changes in enrollments and housing prices are indicators of the extent to which these Say Yes cities are attracting or retaining residents, which is the primary mechanism through which place-based scholarship programs may spark community revitalization. Existing studies of the effects of place-based scholarships on enrollments and housing prices have typically focused on a single program in a single location, such as the Kalamazoo Promise, which limits the ability to draw more general conclusions or to examine the differential effects across locations. The adoption of Say Yes at two different discrete points in time in different locations allows us to develop a broader perspective on the potential effects of place-based scholarships as well as the Say Yes to Education program specifically.

Section 1 of this report examines district enrollment trends from 2000 through 2014. We find that after years of steady declines in enrollments, both Syracuse and Buffalo saw enrollment increases that coincide with the adoption of the Say Yes to Education program. Over the same time periods, enrollments continued to decline in the suburbs surrounding these cities and in

similar upstate New York city school districts without the program, suggesting that these increases were city-specific and not due to broader developments affecting the region. Because enrollment increases are discernible in Buffalo as well as Syracuse, at different points in time and relative to other nearby city school districts, it is unlikely that the increases can attributed to the Great Recession.

Supplementary analyses show that the enrollment increases in Syracuse public schools coincided with declines in enrollment in nearby suburbs, while increases in enrollment in Buffalo public schools coincided with decreases in private school enrollments in the Buffalo area. Moreover, in each location, enrollment increases during the post-Say Yes period were concentrated in the districts' higher performing schools.

Analyses of changes in residential housing prices provide some evidence that increases in prices accompanied the adoption of Say Yes in Syracuse, but not in Buffalo. These results are consistent with findings that enrollment growth in Buffalo may have been driven by students who would otherwise have attended private schools, while enrollment growth in Syracuse may have been driven by students who would otherwise have attended schools in the surrounding suburbs.

Elementary and Middle Grades Test Scores

Section 2 of this report examines trends in grades 3-8 test scores in math and English Language Arts (ELA) after the start of Say Yes in Buffalo. We use student-level data to compare each student's test score performance in the three years after the start of the program to his or her performance prior to the start of the program, controlling for each student's and

school's fixed characteristics. We also use state-level data to standardize each student's score relative to the state average, allowing us to compare performance over time.

We find that, while performance in Buffalo is low relative to the state, math achievement increased significantly in the years following the start of Say Yes. This improvement does not appear to be an artifact of the alignment of New York State tests with the Common Core Standards that coincided with the start of Say Yes, as we do not observe similar increases in Rochester or Syracuse. We do not find increases in ELA performance after Say Yes, however.

Disaggregating the data by student race, we find larger math achievement increases among white students than among African-American students, and larger decreases in ELA performance among African-American students. Thus, test score gaps by race have increased in Buffalo, particularly in the third year of Say Yes.

Secondary and Post-Secondary Effects

A key goal of the Say Yes program is to increase access to post-secondary institutions for students in Buffalo, and to improve their likelihood of success in higher education. In section 3 of this report, we compare 12th graders who started 12th grade in years prior to the start of the program to 12th graders within the same schools who began 12th grade in years after the start of the program and examine high school graduation among 12th graders, college matriculation one year after starting 12th grade, and persistence into a second year of college two years after starting 12th grade, controlling for student and school characteristics. We also compare the effects for those eligible for scholarship by virtue of enrollment in grades 9-12 to students not eligible.

We find consistent evidence of increases in college matriculation. These increases are statistically significant and economically meaningful. The increases for African-American students appear at each point in the pipeline: high school graduation, college matriculation among high school graduates, and persistence among college matriculants. For white students, the increases appear to be entirely from a much higher percentage of high school graduates enrolling in four-year colleges.

We also see a higher percentage of students in the post-Say Yes cohorts persisting into a second year of college. Thus, it appears that Say Yes is not merely encouraging college matriculation among marginal students who are destined to drop out, but rather is attracting students into college for sustained lengths of time. In some cases, such as for African- American students at two-year institutions, Say Yes appears to increase persistence among those who matriculate—suggesting additional benefits of a transparent offer of financial aid.

We find that the positive effects are entirely concentrated among BPS students who are eligible for the scholarships. Not only does this strengthen the conclusion that increase in college matriculation and persistence can be attributed to Say Yes, but it also provides suggestive evidence about the likely mechanisms of the Say Yes effect. The effects on college-going and persistence likely occur through three primary mechanisms: better information on college costs, direct financial resources, and the school-wide effects of increased school supports and additional college-going peers. The comparison of scholarship-eligible and ineligible students suggests that information and financial resources are the primary factors related to improvements in post-secondary outcomes.

Several pieces of information suggest that the informational effects may be stronger than the resource effects in the Say Yes to Education case. First, we find stronger effects on college

matriculation than on college persistence. Matriculation effects are likely to be related to both information and resources. Persistence is likely to be affected only by the financial resources available through the scholarships, as college students would already be aware of the financial aid they received.

Second, as a last-dollar scholarship, it is possible that many lower-income students receive relatively little financial aid from the program. This is particularly true for two-year colleges, where Pell grants would cover the full-cost of tuition for most public institutions. In the analyses for African-American students we find significant increases in two-year college attendance. While our data do not allow us to directly identify students eligible for Pell Grants or other need-based financial aid, 76 percent of the district's students were classified as economically disadvantaged in 2013-14. Thus, it is likely that many of the students attending two-year institutions receive little or no direct financial support from the program. For those attending four-year institutions, particularly private universities, both the information and financial resources are likely to be important.

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PART 1: THE EFFECTS OF SAY YES TO EDUCATION ON ENROLLMENT AND HOUSING PRICES IN SYRACUSE AND BUFFALO[1]

I. Introduction

Place-based scholarship programs award grants for college tuition based on residence in a specific school district or city rather than merit or need. The Kalamazoo Promise is frequently identified as the first, major place-based scholarship program, and since it was announced in 2005, place-based scholarship programs have been established in over 20 cities and districts across the country.[2] The scholarship programs are sometimes accompanied by educational supports for students and serve as a catalyst for community-wide efforts to improve schools.

Like other financial aid programs, place-based scholarships seek to improve college access among groups that are underrepresented in higher education. Unlike other financial aid programs, however, place-based scholarships often explicitly include local community development goals. These programs have typically been established in central cities that have high rates of poverty and that have experienced economic decline. By offering generous educational benefits to residents, the scholarships may create an important locational advantage to these areas in efforts to attract new residents and businesses. Thus, these programs are often promoted as a potential catalyst for local economic development (Miller-Adams, 2015).

[1] The analyses in this section are forthcoming in Hosung Sohn, Ross Rubenstein, Judson Murchie and Robert Bifulco, "Assessing the Effects of Place Based Scholarships on Urban Revitalization: The Case of Say Yes to Education," *Educational Evaluation and Policy Analysis*, 2017.

[2] See the list compiled by Michelle Miller-Adams at the Upjohn Institute, http://www.upjohn.org/sites/default/files/promise/Lumina/Promisescholarshipprograms.pdf?_ga=1.266472014.1147411544.1394049270. The list includes 21 district-wide programs, that provide substantial funding for multiple years that can be used at a range of colleges state- or nation-wide. At least two of those programs, New Haven Promise and Hartford Promise, use significant merit-based criteria as well as residency requirements in making awards, and some of these programs, such as Denver's, combine place-based targeting with financial need criteria.

This chapter examines the impacts of Say Yes to Education on changes in school enrollments and housing prices following the initiation of the Say Yes to Education program in Syracuse, New York in 2008 and in Buffalo, New York in 2012. Changes in enrollments and housing prices are indicators of the extent to which these cities are attracting or retaining residents, which is the primary mechanism through which place-based scholarship programs may spark community revitalization.

Examining district enrollment from 2000 through 2014, we find that after years of steady declines in enrollments, both Syracuse and Buffalo saw enrollment increases that coincide with the adoption of the Say Yes to Education program. These increases occurred at different points in time in each city. Over the same post-treatment periods, enrollments continued to decline in the suburbs surrounding these cities and in similar upstate New York city school districts without the program, suggesting that these increases were city-specific and not due to broader developments affecting the region. The fact that enrollment increases are discernible in Buffalo as well as Syracuse, and relative to other nearby city school districts makes it unlikely that enrollment increases can be attributed to the Great Recession. Supplementary analyses show that the enrollment increases in Syracuse public schools coincided with declines in enrollment in nearby suburbs, while increases in enrollment in Buffalo public schools coincided with decreases in private school enrollments in the Buffalo area. Moreover, in each location, enrollment increases during the post-Say Yes period were concentrated in the district's higher performing schools.

To isolate the impact of Say Yes on housing prices, we use a panel data set on individual home sales to estimate hedonic price models that control for neighborhood fixed effects and trends, as well as analyses that make use of repeat sales, and which compare housing price changes across similar upstate New York cities. The results provide some evidence that increases

in housing prices accompanied the adoption of Say Yes in Syracuse, but not in Buffalo. These results are consistent with findings that enrollment growth in Buffalo may have been driven by students who would otherwise have attended private schools, while enrollment growth in Syracuse may have been driven by students who would otherwise have attended school in the surrounding suburbs.

The results suggest that the ability of place-based scholarships to attract residents into a central city is likely to depend on both the specific provisions of the program and the context in which it is implemented, and in the concluding section of this article, we discuss the implications for policy and future research. The remainder of the paper is organized as follows. Section II provides background on the Say Yes to Education program, placed-based scholarships more generally, and the existing research on these programs. Section III describes the data used in our analyses. Section IV presents our analysis of enrollments, and Section V lays out our analysis of housing market values. The final section of the paper discusses the implications of our findings.

II. Background on Place-Based Scholarships

In a review of "place-based" scholarship programs, Miller-Adams (2015) emphasizes that programs across the country differ in potentially important features—including eligibility requirements, participating colleges and universities, and accompanying initiatives. The Say Yes programs in Syracuse and Buffalo are similar to the Kalamazoo Promise program in that they place minimal restrictions on scholarship eligibility. This feature distinguishes what Miller-Adams calls "universal programs" from the programs in Pittsburgh, New Haven, and Hartford that have more restrictive GPA and attendance requirements. Also, many programs provide scholarships for a single local college or a very limited set of local colleges. Even some of the more expansive programs, including the Kalamazoo Promise, are limited to in-state, public

colleges and universities. In contrast, Say Yes students can receive scholarships at a wide range of private institutions across the country as well as in-state public colleges and universities. Finally, while the introduction of place-based scholarships often spurs school improvement efforts and community-wide action to improve student and family supports, such efforts are more explicitly part of the Say Yes intervention than in some other locations. Despite the differences across place-based scholarship programs, Say Yes to Education shares with these other programs the goals of increasing college access, building a college-going culture, and spurring community development by offering well publicized scholarships based primarily on place of residence and a credible guarantee that scholarship offers will continue long-term (Miller-Adams, 2015).

An element of place-based scholarships that distinguishes them from other financial aid programs is the explicit goal of promoting local economic development. The residency requirements of place-based scholarships are "widely interpreted as a strategy to draw families into the area's urban core and retain those already there" (Miller-Adams, 2015). Changes in district enrollments and housing values can be viewed as indicators of whether place-based scholarship programs are helping to promote this goal.

Most of the evidence on the impacts of place-based scholarships on enrollments and housing values come from evaluations of the Kalamazoo Promise program. Initial analyses found that enrollment in the Kalamazoo public schools increased by 12 percent in the two years immediately following announcement of the Promise program, after falling by over five percent in the three years immediately preceding the announcement (Miron and Cullen, 2008). Bartik, Eberts, and Huang (2010) estimated that enrollments in Kalamazoo in 2009 were nearly 25 percent higher than what they were projected to be in the absence of the program. These authors also find that the Promise stabilized the racial-ethnic composition in the district by stemming

decades of white-flight. Later work finds that the majority of new students in Kalamazoo came from other Michigan districts, and most markedly from one adjacent, relatively high poverty suburban district (Hershbein, 2013). Evidence from Pittsburgh and El Dorado, Arkansas also found that after many years of steady decline, public school enrollment stabilized following the announcement of the Pittsburgh Promise (Ash and Ritter 2014; Gonzalez, Bozick, Tharp-Taylor, and Phillips 2011; Iriti, Bickel, and Kaufman 2012). Miller (2011) uses nine years of data on home sales in the county and finds no evidence that the Promise increased home values in Kalamazoo despite the positive impacts on enrollment and other school characteristics.

In an unpublished paper, LeGower and Walsh (2014) estimate enrollment and housing value effects across multiple place-based scholarship programs. They find that place-based scholarships have been associated with increases in public school enrollments and increases in housing prices relative to their surrounding suburbs.[3] Comparison with surrounding suburban communities, however, is potentially problematic for two reasons. First, within a metropolitan area, enrollments and housing prices in the central city and suburban areas may move in opposite directions, making the suburbs a questionable basis for estimating the counterfactual enrollments and/or housing prices for the central city.[4] Second, if a place-based scholarship serves to draw families into the city who would otherwise choose the surrounding suburbs, then enrollment and housing prices in the suburbs may themselves be influenced by the treatment. In addition, given the time frame of the analysis conducted by LeGower and Walsh (2014), estimated effects of place-based college scholarships may be confounded with the effects of the Great Recession.

[3] Syracuse is included in both the samples used to analyze enrollments and housing values, but Buffalo is not.
[4] Below we show evidence that the trends in housing prices in the Buffalo and Syracuse differ substantially from those in their surrounding suburbs in the years preceding the announcement of Say Yes.

Our analyses add to the existing literature in at least two ways. First, we are able to estimate the effects of place-based scholarship programs that are similar in important details but implemented in two different places at two distinct points in time. Second, our analyses focus on, arguably, the most expansive and generous place-based scholarship program and one of the few to include a broad range of elite private higher education institutions. The Syracuse program, in particular, sets a very low bar for eligibility (attendance in the city schools for grades 10–12) and includes the city's major private university.

III. Data

The enrollment data used in our analyses are from the New York State School Report Cards, which report enrollments by grade, ethnicity, and eligibility for free- and reduced-price-lunch, English as a second language, and special education services from the fall of 2000 to the fall of 2014. We augment district enrollment counts provided by the School Report Cards with counts of students residing in each district who attend charter schools, which we obtained by request from the New York State Education Department.

In addition to examining enrollment trends in Buffalo and Syracuse, we examine concurrent enrollment trends in other public and private schools in the metropolitan areas surrounding these cities using data on private school enrollments from the New York State Education Department.[5] Finally, we also examine trends in Rochester, New York, and its surrounding metropolitan area. Rochester is a district located between Buffalo and Syracuse (less than 90 miles from each) that did not implement the program but had similar student demographics and enrollment trends prior to the adoption of Say Yes.

[5] http://www.p12.nysed.gov/irs/statistics/nonpublic/home.html

To examine the effect of Say Yes on housing values, we use data on home sales from the New York State Office of Real Property Services (ORPS). These data include the universe of property transfers in the state of New York (excluding New York City) from 2000 through the second quarter of 2014, and include the sales price and date. We limit our sample to arms-length sales and also apply a number of filters to ensure that the data exclude extreme outliers and include only valid sales of single residence homes. These files also include property addresses that we use to place the properties in Census block. Finally, we link each property to tax assessment files, also provided by ORPS, which have a wide range of information on property characteristics. Because we have sales data for the entire state, we are also able to compare changes in Syracuse and Buffalo to changes in other school districts in the surrounding metropolitan areas and to changes in Rochester and its surrounding districts.

IV. Enrollment Analysis

Figure 1.1 shows public school enrollment trends for Syracuse, Buffalo, and Rochester, and their surrounding suburbs. In each case, enrollment counts include students who reside in the district boundaries and who attend either district-run schools or charter schools, as students in both types of public school are eligible for Say Yes. Enrollment counts are normalized so that average enrollment in the district (or the set of districts in the cases of the suburban time-series) across all years equals 100.[6]

As shown in Figure 1.1, after years of declines, averaging 1.4 percent annually over the eight years preceding the announcement of Say Yes in Syracuse, enrollments leveled off in the first year and then increased in the second year after Say Yes began. Enrollment in Syracuse

[6] Specifically, the average enrollment across all years for the district (or set of districts) is subtracted from the enrollment count in each year, divided by the average enrollment and multiplied by 100.

dropped by 1,376 students, 6.3 percent, in the three years preceding Say Yes, but increased by 397 students, 1.9 percent, in the three years following the announcement of Say Yes. Enrollment was still 908 students higher seven years after Say Yes than in the last year prior to Say Yes. In contrast, enrollments in the suburbs surrounding Syracuse continued on their pre-existing downward trend following the adoption of Say Yes in the city.

Enrollment patterns in Buffalo are similar to those in Syracuse. After years of declines that averaged 1.7 percent annually, enrollment increased by an average of 1.4 percent annually in the first three years following the adoption of Say Yes. Over the same period, enrollments in the suburban districts around Buffalo continued to decline.

Public school enrollments in Rochester, in contrast, do not show clear changes that coincide with the adoptions of Say Yes in Syracuse or Buffalo. In the four years following the announcement of Say Yes in Syracuse, enrollments in Rochester fell by 220 students, or 0.7 percent. In the three years following the announcement of Say Yes in Buffalo, enrollments in Rochester declined by 452, or 1.4 percent.

Estimation Methods

To estimate the changes in enrollments associated with the adoption of Say Yes, we employ three analyses. The first uses enrollment data solely from the district that adopted Say Yes to estimate the following model:

$$\ln Y_t = \beta_0 + \beta_1 D1_t + \beta_2 D2_t + \beta_3 D3_t + \beta_4 T_t + \varepsilon_t, \quad (1)$$

where $\ln Y_t$ is the natural log of enrollment in year *t*;[7] $\beta_0 + \beta_4 T_t$ represents the intercept and slope of the linear enrollment trend in the district; and $D1_t$, $D2_t$, and $D3_t$ indicate the first, second, and third year, respectively, after the announcement of Say Yes in the district. We limit the sample to three years after the announcement of Say Yes, so these dummy variables are exhaustive of the post-period.[8] This model uses pre-treatment enrollment counts to fit a trend line, projects that trend into the post-period, and then β_1, β_2, and β_3 measure the difference between observed and projected enrollment in each of the post-Say Yes years. Because the outcome variable is the log of enrollment, the coefficient estimates (multiplied by 100) can be interpreted as percent increase in enrollment associated with Say Yes.

In the second analysis, we add control districts that have not been exposed to the treatment during the period observed. When analyzing changes in enrollment associated with Say Yes in Syracuse we add Buffalo and Rochester to the sample. When analyzing changes associated with Say Yes in Buffalo we add Rochester. Using these samples, we estimate the following model:

$$\ln Y_{it} = \beta_0 + \beta_1 D1_{it} + \beta_2 D2_{it} + \beta_3 D3_{it} + \phi_{1i} + \phi_{2i} T_t + \gamma_t + \varepsilon_{it} . \quad (2)$$

In this model, $\beta_0 + \phi_{1i} + \phi_{2i} T$ capture the intercept and slope of each district-specific trend line, and γ_t captures year-specific enrollment shocks that are common across treatment and comparison districts. The estimates of β_1, β_2, and β_3 in this model can be interpreted as

[7] We use the natural log of raw enrollment in each of the analyses that follow, not normalized enrollment as in Figure 1.1.

[8] In the case of Buffalo, only three post-treatment years are available in our data. We also limit the sample to three years post-Say Yes in the Syracuse analysis for three reasons—consistency with Buffalo; it allows us to use Buffalo as a comparison for Syracuse because Buffalo was not exposed to Say Yes for the first three years after the announcement of Say Yes in Syracuse; and the further into the post-treatment period that a pre-treatment trend is projected, the more potential bias there is in impact estimates due to misspecification of the trend and because there are more intervening events that complicate interpretation of deviations from trend.

difference-in-differences estimates. Specifically, they capture the difference between the deviation from projected trends in the Say Yes district in each post-Say Yes year and the deviation from projected trends in the other large city districts in western New York that had not (yet) adopted Say Yes.

The difference-in-differences estimates effectively control for any factors or events that might have influenced enrollments in the cities of Syracuse, Buffalo and Rochester similarly. The difference-in-differences estimates do not necessarily control for factors that had a unique influence on enrollments in a particular metropolitan area during the post-Say Yes period, however. Thus, as a robustness check, we implement an alternative estimation strategy that controls for any metropolitan-specific shocks that influence all districts in a metropolitan area equally. Mechanically, this alternative procedure is computed in the manner of a triple-differences estimator, which compares the difference between deviations from trends in the treated district and its surrounding suburbs to the similar differences between the central city and the suburban districts in the comparison metropolitan area(s). This estimator has the advantage of controlling for metropolitan-specific shocks that might coincide with the adoption of Say Yes. If Say Yes affected enrollments in the suburbs surrounding the city with the program, however, then this triple-differences estimate cannot be interpreted as the increase in enrollment in the Say Yes district that resulted from Say Yes. Rather, it should be interpreted as an indicator of whether or not Say Yes may have contributed to a divergence (or convergence) in enrollments between the city where it was adopted and its surrounding suburbs.

Specifically, we use enrollments in the treated and comparison districts, and their surrounding suburbs to estimate equation (3).

$$\begin{aligned}\ln Y_{dct} = {} & \alpha_0 + \alpha_1 (D1 \times City \times Treated)_{dct} + \alpha_2 (D2 \times City \times Treated)_{dct} \\ & + \alpha_3 (D3 \times City \times Treated)_{dct} + \beta_1 (D1 \times City)_{dct} + \beta_2 (D2 \times City)_{dct} \\ & + \beta_3 (D3 \times City)_{dct} + \gamma_d + \varphi_d T_t + \eta_{ct} + \varepsilon_{dct},\end{aligned} \tag{3}$$

where $\ln Y_{dct}$ is the log of enrollment in district *d* in metro area *c* in year *t*. For suburban districts, we sum enrollment in each year across all the suburban districts in the metropolitan area surrounding a particular city district and treat that as a single district.[9] $D1_t$, $D2_t$, and $D3_t$ represent the first, second, and third year after the announcement of Say Yes, *City* indicates the central city of the metropolitan area (Buffalo, Rochester, or Syracuse), and *Treated* indicates the district where Say Yes is adopted. β_i is the difference in the deviation from pre-Say Yes trends in the central city district and the suburban districts in the comparison metropolitan area(s) during post-Say Yes year *i*, and α_i is the triple-differences estimate of the effect of Say Yes on enrollments. $\gamma_d + \varphi_d T_t$ controls for district-specific trends and η_{ct} is a metropolitan area-by-year fixed effect.[10]

All models are estimated using eight and four years of pre-Say Yes data, as well as three years of post-Say Yes data. The expansion of charter schools was most rapid during the 2000 to 2005 period, particularly in Buffalo, and charter school expansion may serve to draw students from private schools into the public schools.[11] As a result, the estimate of pre-treatment trends

[9] Grouping suburban districts as a single district is intended as a conservative inference approach that does not inflate sample size.

[10] In the difference-in-differences analysis, equation (2), the sample includes only one district in each metropolitan area and so the year fixed effect in equation (2) is equivalent to the metropolitan area-by-year fixed-effect used in this analysis.

[11] Between 2000 and 2005, the percent of public school students residing in the district who attended charter schools increased from 0.2 to 13.3 in Buffalo, from 0 to 6.1 in Rochester, and from 0 to 4.9 in Syracuse. As of 2014, the percent of public school students residing in the district who attended charter school was 18.9 in Buffalo, 12.2 in Rochester, and 6.7 in Syracuse. Students can attend charter schools in Syracuse and Buffalo if they live in the suburbs. However, the counts of charter school students that we include in our district enrollment figures are counts of district residents who attend charter schools, not the number of students in charter schools located in the city. Charter school students who reside in Syracuse and Buffalo are eligible for the Say Yes scholarship.

based on eight years of data might underestimate the declines in enrollments due to underlying economic and demographic factors, which can distort estimates of the effects of Say Yes. For this reason, we believe the estimates using four-years of pre-Say Yes data may provide a more valid estimate of counterfactual enrollments.

Results

Table 1.1 presents the results of our estimations for Syracuse and Buffalo. In keeping with the graphical depiction in Figure 1.1, the first and fourth columns of the top panel of Table 1.1 indicate that enrollments in Syracuse during the post-Say Yes years are higher than predicted by the pre-Say Yes trend. Specifically, three years after the announcement of Say Yes, enrollments are approximately 8.6 percent higher than projected when four-years of pre-Say Yes observations are used, and 4.1 percent higher when eight-years of pre-Say Yes data are used. The estimates are somewhat imprecise though and the estimated increases in enrollments are only marginally statistically significant.

The estimates in columns (1) and (4) of Table 1.1 quantify the deviations from pre-treatment trends evident in Figure 1.1. Of course, any non-linear shock that may have coincided with the adoption of Say Yes and influenced enrollment could provide an alternative explanation for the observed deviations from trend. To begin ruling out potential alternative explanations, we compare deviations in trend in the Say Yes district to those in comparison districts. When the enrollment increases associated with Say Yes in Syracuse are estimated using the difference-in-differences framework of equation (2), as well as the triple-differences framework of equation (3), the estimated increases in enrollment associated with Say Yes are smaller. These smaller estimated enrollment increases reflect the fact that the rate of enrollment declines in Buffalo and

Rochester also slowed during years following the recession of 2008, which coincides with the post-Say Yes period for Syracuse. Unlike in Syracuse, however, neither Buffalo nor Rochester saw actual increases in enrollments during this period, and both the difference-in-differences and triple-differences estimates do show that enrollment increases relative to prior trends during the post-Say Yes period were larger in Syracuse than in Buffalo and Rochester. These difference-in-differences and triple-differences estimates are, however, not reliably different from zero. The results of synthetic control analyses, presented in the online-only appendix, indicate larger enrollment effects for Syracuse—an increase of approximately 4.4 percent—than either the difference-in-differences or the triple-differences model.

In the case of Buffalo (in the bottom panel of Table 1.1), estimated increases in enrollment are 6.4 percent or 7.5 percent higher than projected trends, depending on whether four or eight years of pre-Say Yes observations are used. The enrollment increases associated with Say Yes are a bit more precisely estimated in the case of Buffalo and are statistically significant at conventional levels in the sample with eight years of pre-Say Yes observations. For Buffalo, the difference-in-differences and triple-differences estimates also indicate that post-Say Yes enrollments increased more relative to prior trends than they did in Rochester during the same period. The estimated increases in enrollment three years after the adoption of Say Yes in Buffalo are approximately 7 to 8 percent when four years of pre-Say Yes data are included and approximately 6 percent when eight years of pre-Say Yes data are used, and the estimated differences are mostly statistically significant. [12]

[12] Because Buffalo is much larger than any other district in our sample, the synthetic control method primarily uses Rochester to create the synthetic control for Buffalo and is not appreciably different from the difference-in-difference model. We present synthetic control models for both Syracuse and Buffalo in the housing results below.

These estimates are in line with other evaluations of Promise-type programs, such as LeGower and Walsh (2014) who find an approximately 8 percent increase in enrollment for universal Promise programs such as Say Yes, and the 9 percent increase above expected trends in El Dorado, Arkansas reported by Ash and Ritter (2014) for the El Dorado Promise. The increase is somewhat smaller than the estimate of between an 8 and 25 percent enrollment increase in Kalamazoo, depending on the assumptions used in the model (Bartik, Eberts and Huang, 2010).[13]

Identifying the Source of Enrollment Increases

If the Say Yes program is drawing new students to city schools, there are three plausible sources of these students: schools outside the region, other public schools in the region, and private schools. Although we cannot track migration of individual students, we can compare enrollment trends across different types of schools in each region. Specifically, for both Syracuse and Buffalo, we estimate a version of equation (1) separately for four different sets of schools: the central city (Say Yes) district, the adjacent, inner ring suburban public school districts, outer ring suburban public school districts in the same county, and private schools. In these estimates, we use enrollment counts rather than the log of enrollment, so that we can interpret the estimated coefficients on the post-Say Yes variables as changes in enrollment counts relative to projected trends. Table 1.2 presents the results for Syracuse and Buffalo, respectively. For each analysis, we use four years prior to the announcement of Say Yes in the focal district to extrapolate trends.[14]

[13] Note that the LeGower and Walsh (2014) estimates use a comparison group of surrounding districts while the Kalamazoo and El Dorado estimates are based on pre-post comparisons.

[14] 2014–15 private school enrollments were not available, and so only two years of post-Say Yes data are included for the Buffalo analysis. We use four years rather than eight to extrapolate trends to minimize potential effects of increases in charter schools in Syracuse, which largely occurred from four to eight years before the start of Say Yes in the city.

Table 1.2 indicates that increases in enrollments above projected trends in Syracuse during the Say Yes period were accompanied by decreases in enrollments relative to projected trends in the suburban districts around Syracuse. In contrast, enrollment increases relative to projected trends in Buffalo were accompanied by decreasing enrollments in private schools in the area, relative to pre-existing trends. These results suggest that in Syracuse, the enrollment increases that followed the announcement of Say Yes may have been driven by students who otherwise would have enrolled in the nearby suburbs, while enrollment increases in Buffalo may have come primarily from students who otherwise would have enrolled in private schools.

The Catholic Diocese of Buffalo has closed and consolidated a number of Catholic schools in the Buffalo area over the last decade, but we do not think Catholic school closures can fully explain the deviation from enrollment trends that followed the announcement of Say Yes for several reasons. First, the largest set of private school closures (12 of 103 schools) took place in 2006–07, which is before the window of data used to measure trends in Table 1.2. We do not observe contemporaneous increases in Buffalo Public Schools enrollment during or just before this earlier period. Second, the number of private schools was steady in the first two years immediately after the announcement of Say Yes. Although the need to close Catholic schools in Buffalo was announced by the Diocese in 2011, schools were not actually closed until 2014–15 (the third year after Say Yes), and the 10 schools that were closed in 2014–15 did not show unusual or large drops in enrollment in either of the first two years after the announcement of Say Yes. While it is possible that the threat of closures led some families to choose other schools, there is no evidence to suggest they would have chosen Buffalo public schools in the absence of Say Yes.

Heterogeneity in Enrollment Changes

Table 1.3 shows estimated deviations from pre-Say Yes enrollment trends by race/ethnicity. Here, we estimate equation (2) substituting the log of enrollment by student race for the log of total enrollment. The coefficients compare the deviations from pre-program trends in white and non-white student enrollments in Syracuse and Buffalo with deviations from trends in each city's comparison districts. In both Syracuse and Buffalo, declines in white enrollment in the years leading up to Say Yes were particularly marked. In Syracuse, year-to-year decreases in white enrollment averaged 6.4 percent over the ten years preceding Say Yes. In the first three years following Say Yes, decreases in white enrollments slowed to an average of 2.9 percent per year. Similarly, in Buffalo, year-to-year decreases in white enrollments averaged 4.6 percent over the ten years preceding Say Yes, and slowed to 1.5 percent per year in the three years following the announcement of Say Yes. Compared with deviation from projected trends in Rochester and Buffalo, the number of white students in Syracuse city public schools increased by over 8 percent three years after the start of Say Yes, a substantially larger increase than for non-white students, though the increase is not significant at conventional levels. As a result, the share of white students in Syracuse shows an increase, although that increases in not statistically significant. In Buffalo, there was a significant increase in white students of almost seven percent, which was similar to the increase in non-white students. Thus, the share of white students in the district did not change over the period.[15]

Finally, in Table 1.4, we shift from district-level to school-level analyses to examine enrollment changes by school performance levels. Specifically, we split the sample of

[15] We also examined changes in enrollment of students eligible for free-lunch as well as students not eligible for free-lunch. However, counts of free-lunch eligible students in both treated and untreated districts vary widely around estimated trends during both the pre-treatment and post-treatment period. As a result, estimated changes in enrollment by free-lunch eligibility were much too imprecise to be informative.

elementary schools in each district into three groups based on average fourth grade math and English language arts test scores in the last year prior to the start of Say Yes. Then, using the sample of schools in each treatment districts and its comparison districts, we regress the log of enrollment on indicators of post-Say Yes years, school fixed effects, a school-specific time trend, and year fixed effects. Note that the sample size is larger than in the previous tables because schools are the unit of analysis.

The Say Yes scholarship offer is more valuable to families who expect to send their children to college, and particularly four-year colleges, and thus, we expect that enrollment increases in reaction to Say Yes would be concentrated in higher performing schools. Indeed, the results in Table 1.4 indicate that in Syracuse increases in enrollment were limited to middle and high performing schools. In Buffalo, increases in enrollment were more marked in middle and higher performing schools in the first two years after the announcement of Say Yes, but not in the third Say Yes year. Although many other factors could be driving larger-than-usual increases in higher performing schools, these findings are consistent with the idea that Say Yes is drawing students into the district schools.

In sum, both Syracuse and Buffalo public schools saw enrollment increases relative to projected trends controlling for enrollment changes in the similar nearby cities in the three years following the announcement of Say Yes. In both districts, the increases in enrollment were concentrated in higher performing schools. The increases in enrollments relative to projected trends in Syracuse were accompanied by decreases in enrollment relative to the projected trends in the nearby suburbs, and the increases in enrollments relative to projected trends in Buffalo were accompanied by unusually large decreases in private school enrollments in the area. There is some reason to believe that the increase in enrollments following Say Yes in Syracuse were

the result of more general changes in the mobility of students across the city and the suburbs in western and central New York during the years following the Great Recession. It is, however, unlikely that private school closure can account for the shift of enrollments from private schools to the Buffalo city public schools.

V. Housing Market Analysis

One might suspect that increases in enrollments, particularly if those enrollments are drawn from surrounding districts, would be accompanied by increased demand for housing and thus, increased housing prices in the central cities. Figure 1.2 presents a time-series of median residential housing sales prices in Syracuse, Buffalo, Rochester, and their surrounding suburbs. Median sale prices are normalized to the average median sales price over all the years within each time-series.

In Syracuse, median home sales prices did, in fact, increase substantially above previous trends in the first three years following the adoption of Say Yes, while average prices in the suburbs surrounding Syracuse dropped below prior trends over the same period. These changes in trends are consistent with people relocating from the suburbs to the central city. A similar pattern in housing prices in the central city and its surrounding suburbs is, however, evident for Rochester and Buffalo during this period, which suggests that increases in home sales prices in Syracuse in the years following the announcement of Say Yes may reflect general changes in metropolitan housing markets in western and central New York during the Great Recession, rather than any impact of Say Yes. There is no indication of any increases in median home sales prices following the announcement of Say Yes in Buffalo in Figure 1.2. In fact, median home sales prices dropped steeply in the years immediately following the announcement of Say Yes, although that decline began the year prior to the announcement of Say Yes.

Of course, the simple time-series of median home prices reflects a wide range of factors. For example, the sample of homes sold in a district changes each year, and so changes in median sale prices reflect changes in the types of homes being sold as well as changes in the prices of individual houses. In this section, we use hedonic housing price models, estimated using individual home sales, to try to isolate changes in housing values associated with the adoption of Say Yes.

Estimation Methods

To estimate the increase in housing values associated with Say Yes, we employ a difference-in-differences approach comparing deviations from pre-existing trends in house values in the Say Yes districts to deviations from trends in comparison districts during the same period. Figure 1.2 suggests pre-Say Yes trends in housing values in Syracuse were similar to those in Buffalo and Rochester, and the three cities are also similar in terms of socioeconomic and demographic characteristics, the age of their housing stock, and their role in their larger metropolitan economies. Thus, Buffalo and Rochester are appropriate comparisons for Syracuse. For similar reasons, Rochester is an appropriate comparison for Buffalo.

Housing values in the suburbs surrounding Syracuse and Buffalo exhibit different trends than those in the cities in the years leading up to Say Yes, thus they may not be appropriate comparison districts. Also, if Say Yes attracts families to Syracuse and Buffalo who might otherwise choose to live in the nearby suburbs, then the Say Yes program could influence housing prices in those suburbs as well, again making it an inappropriate comparison group. Nevertheless, the pre-Say Yes trends in housing values in the suburban areas of Syracuse,

Buffalo, and Rochester are all similar to each other. We exploit this fact to implement an alternative estimation strategy discussed below.

We implement our difference-in-differences estimator in two different ways. First, we use all home sales in the treated and comparison districts for the eight years prior to the adoption of Say Yes and the three years following the adoption of Say Yes[16] to estimate a regression that controls for neighborhood fixed effects and trends as well as individual housing characteristics.[17] Specifically, we estimate the following regression:

$$\ln P_{int} = \alpha_0 + \alpha_1 D1_{nt} + \alpha_2 D2_{nt} + \alpha_3 D3_{nt} + X_{\text{int}}\Phi + \gamma_n + \varphi_n T_t + \eta_t + \varepsilon_{int}, \quad (4)$$

where the outcome variable is the log of the sales price for property *i* in census tract *n* in year *t*; the treatment variables are defined as they were in the enrollment analysis; ***X*** is a vector of housing characteristics; $\gamma_n + \varphi_n T_t$ are the intercept and slope of a neighborhood-specific trend,[18] and η_t is a year-specific effect. The strength of this first strategy is that it uses all housing sales in the district to estimate effects. However, it only provides adequate control for changes in the types of houses sold each year if—controlling for observed housing characteristics—homes sold within neighborhoods are sufficiently homogeneous.

The second strategy to control for changes in the types of homes sold is to limit the sample to homes that have sold multiple times, and estimate the following regression.

[16] For Syracuse, we use Rochester and Buffalo as the comparison districts, and for Buffalo, we use Rochester as the comparison. We also computed estimates using four years of pre-Say Yes observations, and the results were substantively very similar, although less precise. In contrast to the enrollment analysis, we do not expect the increases in charter schools in Syracuse that occurred four to eight years before the start of Say Yes to affect housing prices. We, therefore, report the results using eight years of pre-Say Yes data here. The results using four years of pre-Say Yes data are reported in Appendix Table A1.2.

[17] Housing characteristics included as controls are square feet of living area, square feet of garage and basement, overall condition, age of home, number of stories, number of rooms, number of bedrooms, number of full bathrooms, number of half bathrooms, whether or not there is a finished recreational room, whether or not the house has central air conditioning, and heat type.

[18] Inclusion of the neighborhood specific trend term is substantively important. Estimated effects of Say Yes on property values in both Syracuse and Buffalo are consistently and substantially more positive in models that exclude the control for neighborhood specific trends. See Appendix Table A1.1 for a comparison of coefficients.

$$\ln P_{int} = \alpha_0 + \alpha_1 D1_{nt} + \alpha_2 D2_{nt} + \alpha_3 D3_{nt} + \lambda_i + \varphi_n T_t + \eta_t + \nu_{int}. \quad (5)$$

In this regression, we replace the neighborhood fixed effect in equation (4) with an individual property fixed effect, which is possible because each home is observed multiple times. We continue to control for neighborhood trends. Because we only observe housing characteristics at a single point in time, their effect on housing prices cannot be estimated separately from the individual property fixed effect and thus drop out of this model. Including the individual housing fixed effect provides a more complete control for changes in the types of housing sold in different years. In this model, however, the effects of Say Yes are identified by changes in the price of homes sold multiple times in a relatively short period of time, which may be unrepresentative of changes in values across all homes that are sold.

Conducting proper inferences for estimates of the treatment effects in equations (4) and (5), α_i, is not straightforward. Although we observe thousands of individual home sales, these sales are clustered in three districts in the case of the estimated impact of Say Yes in Syracuse, and only two districts in the case of Say Yes in Buffalo, and in each case, there is only one treated cluster. In the presence of this type of clustering, standard error estimates that assume independent observations can be biased downward substantially. The standard solution to this problem—cluster robust standard errors—relies on having a large number of treated and comparison group clusters, which clearly is not the case here (Wooldridge, 2003, 2006; Donald and Lang, 2007; Conley and Taber, 2011; Cameron and Miller, 2015).

To conduct proper inferences, and specifically to obtain correct *p*-values, we estimate equations (4) and (5) using the two-step procedure suggested by Donald and Lang (2007). In the first step, the log of housing prices are regressed on all variables that vary at the individual or neighborhood level (namely, the individual property covariates, neighborhood or property fixed

effects, neighborhood trends), and a set of district-by-year fixed effects. In the second step, the estimated district-by-year fixed effects are regressed on variables that vary at the district level, namely the treatment variables, a district-specific time trend, and year fixed effects, weighting by the number of observations in each district-by-year. As demonstrated by Donald and Lang (2007), this two-step procedure is an efficient estimator and provides appropriate *p*-values in the case of a small number of clusters under relatively unrestrictive assumptions.

The difference-in-differences estimates effectively control for any factors that might have influenced property values in Syracuse, Buffalo, and Rochester similarly. However, the difference-in-differences estimates do not necessarily control for factors that had a unique influence on property values in a particular metropolitan area during the post-Say Yes period. Thus, as a robustness check, we again use a triple-differences estimator, which compares the difference between deviations from trends in the treated district and its surrounding suburbs to the similar differences between the central city and the suburban districts in the comparison metropolitan area(s). As in the enrollment analysis, this estimator has the advantage of controlling for metropolitan-specific shocks that might coincide with the adoption of Say Yes, and should be interpreted as an indicator of whether or not Say Yes may have contributed to a divergence (or convergence) in housing prices between the city where it was adopted and its surrounding suburbs.

Specifically, we use data on all home sales in the treated and comparison districts, and their surrounding suburbs to estimate equation (6).

$$\begin{aligned}\ln P_{inmt} = {} & \alpha_0 + \alpha_1 (D1 \times City \times Treated)_{nmt} + \alpha_2 (D2 \times City \times Treated)_{nmt} \\ & + \alpha_3 (D3 \times City \times Treated)_{nmt} + \beta_1 (D1 \times City)_{nmt} + \beta_2 (D2 \times City)_{nmt} \\ & + \beta_3 (D3 \times City)_{nmt} + X_{inmt}\Phi + \gamma_n + \varphi_n T_t + \eta_{mt} + \varepsilon_{int}.\end{aligned} \tag{6}$$

As in estimation of the difference-in-differences, we control for individual housing characteristics, neighborhood-specific fixed effects and trends, and in this case, metropolitan-by-year fixed effects, η_{mt}. As in the enrollment analysis, β_i is the difference in the deviation from pre-Say Yes trends in the central city district and districts in the comparison metropolitan area(s) during post-Say Yes year *i*, and α_i is the triple-differences estimate of the effect of Say Yes on the log of property values. We also estimate an equation similar to (6) in which we replace the individual property covariates and neighborhood fixed effects with an individual property fixed effect using properties with multiple sales. To ensure proper inferences, we estimate both the all sales and repeated sales regression using the two-step procedure that we used to implement the difference-in-differences estimator.

Results

Table 1.5 displays the results of our primary housing value analysis. For both the difference-in-differences and the triple-differences analyses, estimated changes in housing values associated with Say Yes are similar whether all sales or multiple sales are used. The triple-differences estimates tend to be larger in absolute value and less precise than the corresponding difference-in-differences estimates. Nonetheless, the results from the difference-in-differences and the triple-differences are qualitatively similar.

Both the difference-in-differences and triple-differences suggest that Syracuse experienced a larger increase in property values after Say Yes relative to pre-existing neighborhood trends than did Rochester and Buffalo. The results are statistically significant only in the case of the triple-differences estimates during the third year after the announcement of Say Yes, which show rather large increases of between 14 and 17 percent. Overall, the estimated

increases in property values associated with the adoption of Say Yes in Syracuse ranged between 6.5 percent and 16.9 percent depending on the sample and model. These results are similar to the estimates of 6 percent to 12 percent reported by LeGower and Walsh (2014) using their pooled sample and in contrast to early estimates in Kalamazoo that found no effect on housing prices (Miller, 2011; Miller-Adams, 2010).

The fact that the triple-differences estimates are larger than the difference-in-differences estimates for Syracuse suggests that the announcement of Say Yes is associated with decreases in property values in the surrounding suburbs as well as increases in property values in Syracuse. To test that hypothesis more directly, we computed difference-in-differences estimates of the effect of Say Yes on property values in the suburbs around Syracuse. To compute these difference-in-differences, we estimate equations (4) and (5) above using the sample of home sales in the Syracuse, Rochester, and Buffalo suburbs during the eight years preceding and three years following the announcement of Say Yes in Syracuse, and consider the homes sales in the Syracuse suburbs following the announcement as treated observations.

The results of this analysis of suburban property values is presented in Table 1.6. As implied by the results in Table 1.5, the changes in properties values in the Syracuse suburbs associated with the adoption of Say Yes are negative and large, and are also statistically significant. Three years after the adoption of Say Yes in Syracuse properties values in the suburbs decreased between 7 percent and 9 percent, relative to the projection of pre-existing trends and controlling for deviations from projected trends observed in the Rochester and Buffalo suburbs during the same time period. The results in Table 1.6 indicate that the announcement of Say Yes was associated with both increases in property values in the city and

decreases in property values in the surrounding suburbs, which is consistent with the idea that people who might otherwise have lived in the suburbs moved to or remained in the city. Returning to Table 1.5, the estimated changes in property values associated with Say Yes in Buffalo are all negative. The triple-differences estimates are slightly more negative, but also less precise, than the difference-in-differences estimates. The estimated changes in housing values are statistically significant only for the second year after the adoption of Say Yes, when all sales are used. The estimated second-year changes in the multiple sales sample, when more complete controls for housing characteristics are employed, are less precise and not statistically different from zero.

Although the results in the bottom panel of Table 1.5 suggest that a decline in property values in Buffalo relative to projections may have accompanied the adoption of Say Yes, Figure 1.2 suggests that the start of the decline preceded the start of the program. To test this possibility, we add to each of the models estimated in Table 1.5 variables indicating the first, second, and third year pre-Say Yes. If the estimated coefficients on the post-Say Yes variables move closer to zero (or change signs), or the coefficients on the pre-treatment variables are similar to the post treatment variables, it suggests that prices may have started to change before the start of Say Yes and the estimates in Table 1.5 may not reflect the causal impacts of the program.

The results of this "event history" analysis are presented in Tables 1.7 (Syracuse) and 1.8 (Buffalo). In Syracuse, the coefficients for the pre-Say Yes years are largely, though not always, positive and are not significant in any model. The post-Say Yes coefficients are substantially larger than the pre-Say Yes coefficients. The post-Say Yes coefficient are also slightly larger in this model than in the previous models, and still significant in the triple-differences model for the third year of Say Yes. Thus, the estimates do not provide any indication that the observed

housing market changes began before the start of Say Yes in Syracuse. Table 1.8 shows that the post-Say Yes coefficients are much smaller than the pre-Say Yes coefficient estimates and the estimates in Table 1.5, and no longer significant, suggesting that the price decreases in Buffalo were not the result of Say Yes.

We also used the synthetic control method to create counterfactuals for Syracuse and Buffalo housing prices (full results presented in the online-only appendix). For both Syracuse and Buffalo, Rochester receives the majority of weight in constructing the synthetic control, which validates our reliance on Rochester as a comparison city in the regression analyses. We find that the housing price increase in Syracuse after Say Yes was substantially larger than in its synthetic control, and larger than those found in the difference-in-differences analyses. As in the difference-in-differences models, the synthetic control analysis finds no evidence of housing price increases in Buffalo after the start of Say Yes. Thus, the synthetic control analyses suggest that the results are robust to the use of alternative methods.

We also estimated the changes in housing values associated with the adoption of Say Yes by neighborhood income level. Specifically, we divided the sample of treatment and control neighborhoods into thirds based on median housing income in the neighborhood. We then estimated our difference-in-differences models for each sample separately. The results suggest that changes in property values associated with the adoption of Say Yes were concentrated in low and middle-income neighborhoods in Syracuse and in low-income neighborhoods in Buffalo. However, the estimates of changes in housing values were generally quite noisy and thus, we are reluctant to draw any strong conclusions from this analysis.

VI. Summary and Discussion

The analyses presented above examine potential early indicators of urban revitalization—school district enrollments and housing prices—in Syracuse and Buffalo, New York in the wake of Say Yes to Education's start in each city. We find consistent evidence of enrollment increases in both Syracuse and Buffalo following the announcement of the program and that these increases occurred after years of largely declining enrollments. Moreover, the increases coincided with the start of the program and grew over time, though the program began in different years in each city. While the Syracuse increases were accompanied by large enrollment declines in surrounding suburban districts, the Buffalo increases coincided with large declines in private school enrollments in the area. The increases in both cities, and particularly in Syracuse, appear to be largely concentrated in the districts' highest performing schools. It must be noted, however, that the cities of Buffalo and Rochester also saw enrollment increases relative to projected trends following the announcement of Say Yes in Syracuse, suggesting that at least part of the increase in enrollments in Syracuse might be attributable to factors other than Say Yes.

Using difference-in-differences and triple-differences models, we find evidence of substantively meaningful increases in home prices in Syracuse after the program's announcement, as well as decreases in housing values in the suburbs surrounding Syracuse, both of which are consistent with the hypothesis that Say Yes helped to attract to the city people who would otherwise have located in the suburbs. We do not find evidence of similar housing price changes in Buffalo. The Syracuse results also suggest that much of the program's effect may be to shift locational decisions within, but not across, metropolitan areas.

These results, then, raise questions about why responses to the program would be different in Syracuse than in Buffalo. We have no definitive answers, but it is quite likely that the

different contexts and program benefits between the two cities may help to explain the findings. First, the Syracuse program is arguably more generous than the Buffalo program. Syracuse requires only three years of high school attendance for full scholarship eligibility, while Buffalo requires twelve years. Additionally, tuition at Syracuse University, listed at over $40,000 per year during this period, is available for all Syracuse Say Yes students but only for those from families with income under $100,000 in Buffalo. Given its location, Syracuse University is also likely to be a more attractive option for students from Syracuse than from Buffalo. The more generous benefits available in Syracuse may be more likely to induce families to move from the suburbs to the city, or to stay in the city, to take advantage of the program, thereby increasing demand for housing. In Buffalo, the program appears to have drawn students largely from private schools. If these families already lived in the city, the housing market effects would likely be smaller.

The contexts of Syracuse and Buffalo may also help to explain some of the differences in responses to Say Yes across the two cities. As described above, Buffalo's large Catholic school sector was undergoing consolidation and closures during the years before Say Yes began. While these events did not coincide with the start of Say Yes, the uncertainty surrounding private schools, combined with the programmatic and scholarship benefits of Say Yes, may have accelerated movement toward city public schools. In Syracuse, with a smaller private school sector, suburban schools may represent the more relevant alternative for many parents who do not want to send their children to public city schools. Ultimately, though, understanding the mechanisms underlying these differential effects is worthy of additional study in future research.

The results also provide some evidence on the potential for place-based scholarships to spur economic revitalization in distressed cities. Both Syracuse and Buffalo have suffered

through decades of economic decline and shrinking tax bases. Between 1950 and 2000, Buffalo lost half of its population, the fourth largest decline among large cities in the United States, while Syracuse lost one-third of its population (Office of the New York State Comptroller, 2004). Evidence from the Say Yes to Education program suggests that providing a substantial and highly visible amenity such as free college tuition may be effective at stemming these ongoing population losses and inducing some households to remain in central cities or to move from nearby suburbs, though the magnitude of the effect may be modest. From a metropolitan perspective, this growth may be a zero-sum game, with gains in cities offset by losses in neighboring communities. Additionally, providing free college to large numbers of students may be an expensive model if the gains are small, though the last-dollar nature of the scholarships reduces overall costs. Future work will be needed to determine whether these cities are able to maintain the enrollment gains and whether they are, in fact, leading indicators of broader economic development.

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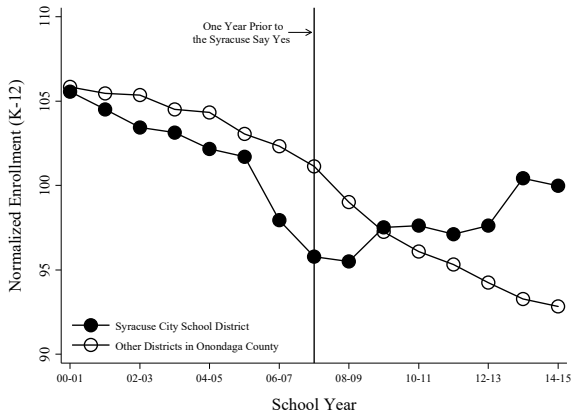
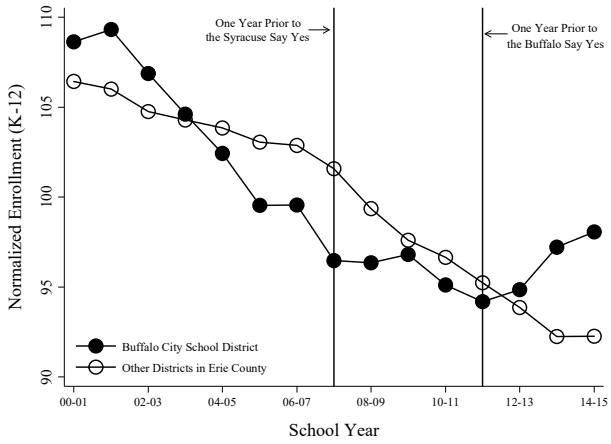
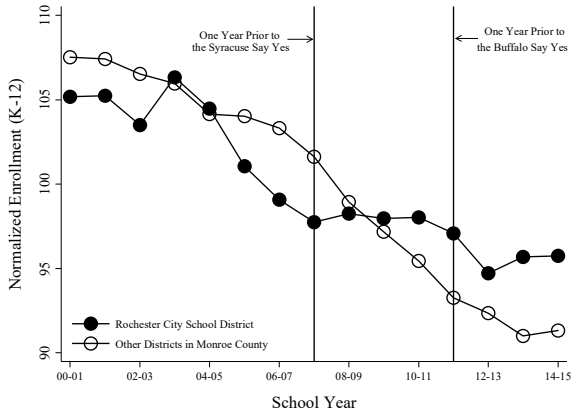


Panel A: Syracuse City School District vs. Other Districts in Onondaga County

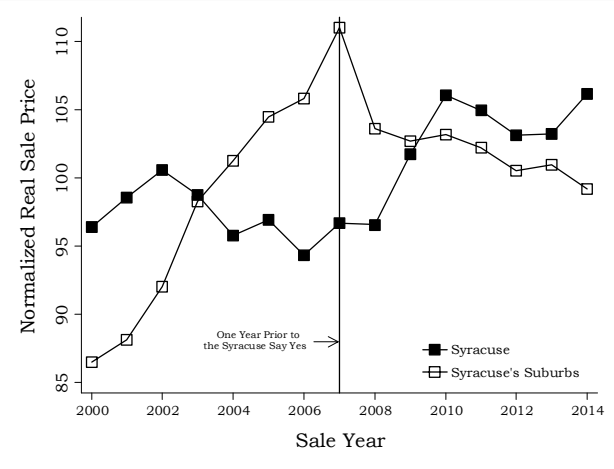


Panel B: Buffalo City School District vs. Other Districts in Erie County

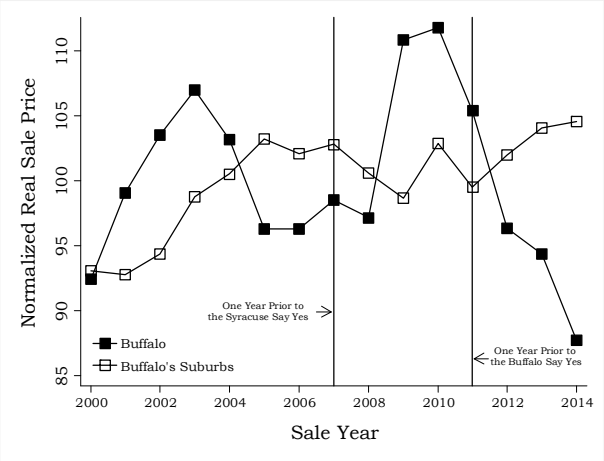


Panel C: Rochester City School District vs. Other Districts in Monroe County

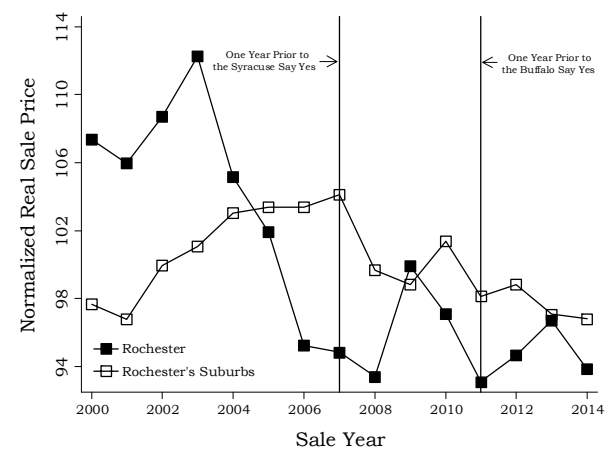
Figure 1.1. Enrollment Trends, Cities and Suburbs.



Panel A: Syracuse vs. Its Suburbs



Panel B: Buffalo vs. Its Suburbs



Panel C: Rochester vs. Its Suburb

Figure 1.2. Median Housing Values, Cities and Suburbs.

Table 1.1. Changes in Enrollment Trends Associated with Adoption of Say Yes

	Four-years pre-Say Yes included			Eight-years pre-Say Yes included		
Variable	Pre-post	Difference-in-differences	Triple-differences	Pre-post	Difference-in-differences	Triple-differences
			Panel A: Syracuse			
1 year post Say Yes	0.018	−0.004	−0.007	−0.006	−0.014	−0.011
	(0.016)	(0.019)	(0.018)	(0.015)	(0.017)	(0.017)
	[0.368]	[0.829]	[0.716]	[0.705]	[0.420]	[0.516]
2 year post Say Yes	0.062*	0.019	0.019	0.027	0.004	0.012
	(0.019)	(0.023)	(0.022)	(0.017)	(0.018)	(0.018)
	[0.083]	[0.425]	[0.404]	[0.126]	[0.811]	[0.515]
3 year post Say Yes	0.086*	0.032	0.030	0.041*	0.012	0.020
	(0.023)	(0.027)	(0.026)	(0.018)	(0.019)	(0.019)
	[0.064]	[0.277]	[0.284]	[0.060]	[0.539]	[0.315]
No. of observations	7	21	42	11	33	66
			Panel B: Buffalo			
1 year post Say Yes	0.013	0.037	0.041**	0.025	0.029*	0.032
	(0.011)	(0.012)	(0.011)	(0.013)	(0.013)	(0.033)
	[0.349]	[0.150]	[0.018]	[0.155]	[0.065]	[0.351]
2 year post Say Yes	0.047*	0.057*	0.064***	0.056***	0.046**	0.053
	(0.014)	(0.015)	(0.013)	(0.013)	(0.014)	(0.036)
	[0.075]	[0.060]	[0.008]	[0.006]	[0.016]	[0.163]
3 year post Say Yes	0.064*	0.069*	0.080***	0.075***	0.057***	0.065
	(0.016)	(0.017)	(0.015)	(0.014)	(0.015)	(0.038)
	[0.059]	[0.057]	[0.007]	[0.002]	[0.009]	[0.112]
No. of observations	7	14	28	11	22	44

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regression. The outcome variable in each regression is the natural log of enrollment. "Pre-post" correspond to equation (1), "Difference-in-differences" correspond to equation (2), and "Triple-differences" correspond to equation (3). Estimates include controls for pre-Say Yes enrollment trends. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 1.2. Changes in Enrollment Trends Associated with Adoption of Say Yes, by Type of District

Variable	Countywide	City public schools	Public schools in adjacent districts	Public schools in other districts in the county	Private schools
			Panel A: Syracuse		
1 year post Say Yes	115	394	−328**	−176	226
	(573)	(335)	(75)	(131)	(396)
2 year post Say Yes	523	1,323*	−544**	−550*	294
	(697)	(407)	(92)	(160)	(481)
3 year post Say Yes	776	1,839*	−585**	−856**	379
	(834)	(488)	(110)	(191)	(576)
No. of observations	7	7	7	7	7
			Panel B: Buffalo		
1 year post Say Yes	−851	518	−3	−16	−1,350*
	(546)	(430)	(104)	(175)	(341)
2 year post Say Yes	806	1,804*	−84	209	−1,122*
	(664)	(524)	(126)	(213)	(415)
No. of observations	6	6	6	6	6

Note. Each column of figures are coefficients, with associated standard errors in parentheses, from separate estimation of equation (1), each using four years of pre-Say Yes observations. Untransformed enrollment counts, rather than the natural log of enrollment counts, are used as outcome variables. Estimates include controls for pre-Say Yes enrollment trends. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 1.3. Change in Trends in Enrollment Associated with Adoption of Say Yes, by Race

	Difference-in-differences estimates		
Variable	*ln* white enrollment	*ln* non-white enrollment	Share white
		Panel A: Syracuse	
1 year post Say Yes	0.030	−0.006	0.008
	(0.030)	(0.031)	(0.006)
2 year post Say Yes	0.096**	0.008	0.019
	(0.037)	(0.038)	(0.007)
3 year post Say Yes	0.082	0.029	0.016
	(0.044)	(0.046)	(0.009)
No. of observations	21	21	21
		Panel B: Buffalo	
1 year post Say Yes	0.048**	0.042	0.002
	(0.004)	(0.020)	(0.004)
2 year post Say Yes	0.066**	0.095*	−0.002
	(0.005)	(0.024)	(0.005)
No. of observations	12	12	12

Note. Each column of figures are coefficients, with associated standard errors in parentheses, from separate estimates of equation (2). The outcome variable in each regression is the natural log of enrollment. Estimates include controls for pre-Say Yes enrollment trends estimated using four years of pre-Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05.

Table 1.4. Changes in Enrollment Trends Associated with Adoption of Say Yes, By School

Variable	Low-performing	Middle-performing	High-performing
		Panel A: Syracuse	
1 year post Say Yes	0.016	0.175***	0.178***
	(0.053)	(0.063)	(0.059)
2 year post Say Yes	0.025	0.101**	0.113**
	(0.045)	(0.048)	(0.047)
3 year post Say Yes	0.002	0.092**	0.057
	(0.036)	(0.045)	(0.038)
No. of observations	203	201	196
		Panel B: Buffalo	
1 year post Say Yes	0.050	0.116*	0.083
	(0.065)	(0.059)	(0.079)
2 year post Say Yes	−0.054	0.061	0.114*
	(0.054)	(0.049)	(0.065)
3 year post Say Yes	−0.016	0.010	0.002
	(0.044)	(0.040)	(0.054)
No. of observations	147	147	140

Note. Each column of figures are coefficients, with associated standard errors in parentheses, from separate regressions. Regression equation estimated in each case is similar to equation (2) except district-specific trends are replaced with school-specific trends. The outcome variable in each regression is the natural log of enrollment. All estimates are based on four-year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and ***indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 1.5. Changes in Housing Prices Associated with Adoption of Say Yes

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
		Panel A. Syracuse		
1 year post Say Yes	0.052	0.046	0.071	0.089
	(0.035)	(0.045)	(0.046)	(0.064)
	[0.159]	[0.317]	[0.136]	[0.174]
2 year post Say Yes	0.022	0.016	0.075	0.089
	(0.036)	(0.046)	(0.064)	(0.066)
	[0.544]	[0.731]	[0.119]	[0.189]
3 year post Say Yes	0.065	0.068	0.143**	0.169**
	(0.044)	(0.056)	(0.057)	(0.079)
	[0.159]	[0.243]	[0.020]	[0.044]
District-by-year obs.	33	33	66	66
Individual property sales	49,624	23,540	270,011	115,324
		Panel B. Buffalo		
1 year post Say Yes	−0.024	−0.028	−0.042	−0.060
	(0.035)	(0.044)	(0.048)	(0.067)
	[0.518]	[0.552]	[0.399]	[0.390]
2 year post Say Yes	−0.097**	−0.072	−0.111**	−0.107
	(0.036)	(0.044)	(0.049)	(0.069)
	[0.035]	[0.154]	[0.035]	[0.146]
District-by-year obs.	20	20	40	40
Individual property sales	39,112	18,989	186,554	80,530

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5), and "Triple-diff. all sales" correspond to equation (5). "Triple-diff. multiple sales" is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on eight-year pre-Say Yes and three-year post Say-Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 1.6. Changes in Housing Prices in the Syracuse Suburbs Associated with Adoption of Say Yes

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales
1 year post Say Yes	−0.012	−0.035**
	(0.013)	(0.016)
	[0.338]	[0.046]
2 year post Say Yes	−0.047***	−0.072***
	(0.013)	(0.016)
	[0.002]	[0.000]
3 year post Say Yes	−0.072***	−0.094***
	(0.015)	(0.019)
	[0.000]	[0.000]
District-by-year observations	33	33
Individual property sales	49,624	23,540

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5). All estimates are based on samples that include eight-year pre-Say Yes and three-year post Say-Yes observations of home sales in suburban areas around Syracuse, Buffalo, and Rochester. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 1.7. Changes in Housing Prices Associated with Adoption of Say Yes in Syracuse

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
3 year pre Say yes	0.036	0.039	0.046	0.027
	(0.044)	(0.056)	(0.036)	(0.041)
	[0.322]	[0.337]	[0.217]	[0.518]
2 year pre Say Yes	0.004	−0.025	−0.003	−0.065
	(0.036)	(0.046)	(0.043)	(0.049)
	[0.916]	[0.604]	[0.948]	[0.200]
1 year pre Say Yes	0.037	0.068	0.041	0.058
	(0.035)	(0.045)	(0.052)	(0.058)
	[0.477]	[0.253]	[0.435]	[0.335]
1 year post Say Yes	0.083	0.080	0.103	0.097
	(0.035)	(0.045)	(0.062)	(0.069)
	[0.192]	[0.255]	[0.117]	[0.179]
2 year post Say Yes	0.058	0.056	0.112	0.098
	(0.036)	(0.046)	(0.068)	(0.077)
	[0.400]	[0.471]	[0.124]	[0.221]
3 year post Say Yes	0.106	0.113	0.184**	0.180*
	(0.045)	(0.056)	(0.079)	(0.089)
	[0.200]	[0.219]	[0.036]	[0.062]
District-by-year observations	33	33	66	66
Individual property sales	49,624	23,540	270,011	115,324

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5), and "Triple-diff. all sales" correspond to equation (5). "Triple-diff. multiple sales" is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on eight- year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

Table 1.8. Changes in Housing Prices Associated with Adoption of Say Yes in Buffalo

Variable	Diff.-in-diff. all sales	Diff.-in-diff. multiple sales	Triple-diff. all sales	Triple-diff. multiple sales
3 year pre Say yes	0.026	0.037	0.011	0.031
	(0.035)	(0.039)	(0.044)	(0.067)
	[0.508]	[0.416]	[0.812]	[0.661]
2 year pre Say Yes	0.084	0.117*	0.065	0.103
	(0.043)	(0.048)	(0.055)	(0.083)
	[0.148]	[0.094]	[0.281]	[0.262]
1 year pre Say Yes	0.044	0.057	0.031	0.055
	(0.048)	(0.053)	(0.061)	(0.092)
	[0.430]	[0.362]	[0.632]	[0.566]
1 year post Say Yes	0.033	0.051	−0.003	0.011
	(0.053)	(0.059)	(0.067)	(0.102)
	[0.579]	[0.454]	[0.971]	[0.915]
2 year post Say Yes	−0.031	0.018	−0.072	−0.025
	(0.059)	(0.065)	(0.074)	(0.113)
	[0.629]	[0.803]	[0.373]	[0.833]
District-by-year observations	20	20	40	40
Individual property sales	39,112	18,989	186,554	80,530

Note. Each column of figures are coefficients, with associated standard errors and *p*-values in parentheses and brackets, respectively, from separate regressions. The outcome variable in each regression is the natural log of the home sales price. "Diff.-in-diff. all sales" correspond to equation (4), "Diff.-in-diff. multiple sales" correspond to equation (5), and "Triple-diff. all sales" correspond to equation (5). "Triple-diff. multiple sales" is based on equation similar to equation (5) with individual property covariates and neighborhood fixed effects replaced by individual property fixed effects. All estimates are based on eight- year pre-Say Yes and three-year post Say Yes observations. * indicates that the coefficient estimate is statistically distinguishable from zero at 0.10, ** indicates that the coefficient estimate is statistically distinguishable from zero at 0.05, and *** indicates that the coefficient estimate is statistically distinguishable from zero at 0.01.

PART 2: THE EFFECT OF SAY YES TO EDUCATION ON STUDENT TEST SCORES IN BUFFALO

I. Introduction

Improving student achievement is a critical component of the Say Yes to Education program's overarching goals in Buffalo and other partner school districts. It is well-known that families often select residential locations based, in part, on the quality of the schools in a given neighborhood or school district. While average standardized test scores have many shortcomings for measuring school quality, they are readily-available and commonly-used by families to select home locations. Therefore, improving district test scores can help to attract and retain families in the city and to the Buffalo Public Schools, as described in section 1 of this report. Moreover, improving student achievement and preparation in elementary and secondary school can improve access to, and success in, higher education institutions for graduates of the BPS.

In this section, we examine trends in student performance on New York State end-of-course examinations in math and English Language Arts (ELA) before and after the start of the Say Yes program in Buffalo. We begin by describing the data and analytic methods followed by statistical analysis that attempts to isolate the causal effects of the program.

II. Data and Methods

The analyses presented in this report use two data sources. The first is a student-level longitudinal database of students enrolled in grades 3–8 in the BPS for the years 2008–09 to 2014–15. The data were obtained directly from the BPS central administration. In addition to student background characteristics, these data include the student's school and grade, test scores,

and program eligibility for each year from 2006–07 to 2014–15. For each grade examined, the data span four cohorts of students that were enrolled in that grade prior to Say Yes and three cohorts that were enrolled in the grade during the post-Say Yes years. Figure 2.1 graphically displays the cohorts and grades at which Say Yes began for each cohort. Students in cohorts 5–9 are observed both before and after the start of the program, while earlier cohorts left middle school before the program began and later cohorts started 3rd grade after the start of Say Yes. Examining changes in test score performance of students in cohorts 5–9, who attended the BPS both before and after Say Yes, will allow us to measure the effects of Say Yes.

Each student record contains a unique identifier that allows us to track the student as he or she progresses through the Buffalo school system. The records also contain information on individual student characteristics such as gender, race, special education eligibility, and whether the student has repeated a grade or changed schools. Most important for analyses of student performance on state tests, the student-level data allow us to control for each student's performance in earlier grades.

While student-level data are generally preferable to more aggregated school- or district-level data for evaluating educational programs, the student-level data used here have two important shortcomings. First, we have access to student-level data for the BPS only. Therefore, while we can examine test score gains before and after the start of Say Yes in Buffalo, we cannot compare trends in performance for Buffalo students to those for students in other similar districts. Such a comparison group would strengthen the causal analysis by allowing us to control for events that may have affected Buffalo and other districts at the same time that the Say Yes program was implemented. Second, beginning in 2012–13 (the first year of Say Yes in Buffalo), New York State adopted the Common Core standards and new tests aligned to the

standards. Student test scores fell dramatically across the state in the first year of the test (Hernandez and Gebeloff, 2013), complicating trend analyses examining student performance.

To address these potential problems, we supplement the student-level data with district-level data available in the New York State Report Cards (NYSRC) maintained by the New York State Education Department. The NYSRC provides rich sets of school- and district- level information on student academic achievement. The report card data allow us to compare trends in Buffalo to those in Rochester and Syracuse, and facilitates analysis of effects of the testing instrument changes on student performance. The NYSRC also include means and standard deviations for each test which allow us to standardize scores relative to the state average.

Our primary analyses examine individual student outcomes in the four years prior to the start of Say Yes to the same outcomes during the first three years of the Say Yes program in Buffalo. In notation, we run the following regression model:

$$y_{ist} = \beta_0 + \beta_1 Post_t^1 + \beta_2 Post_t^2 + \beta_3 Post_t^3 + \gamma_i + \eta_s + \varepsilon_{ist},$$

where y_{ist} represents the test score of student *i* in school *s* in year *t*, $Post_t^1$ represents the first year after the start of Say Yes, $Post_t^2$ represents the second year after the start of Say Yes, $Post^3{}_t$ represents the third year of Say Yes, γ_i represents the student fixed effect, η_s is the school fixed effect and ε_{ist} is the error term. The student fixed effects control for all student characteristics that are fixed or unchanging over the period, such as aptitude, previous performance, family background, race and gender. Note that, because we include student fixed effects, we do not need to separately control for specific, fixed student characteristics, such as gender and race, though we do disaggregate the data by race to separately examine trends for different groups of students. The school fixed effects control for fixed characteristics of the schools these students attend, whether or not we can observe them in the data. Thus, for each student in cohorts 5-9 in

Figure 2.1, we compare his or her performance in each year after the start of Say Yes to his or her average performance prior to Say Yes in the same school

All test scores are constructed as standardized *z*-scores by subtracting the statewide mean for each test in each year from each student's score and dividing by the statewide standard deviation for that year. The z-scores allow us to compare performance across years to a standard metric, the statewide average, even when the testing instrument changes. Implicitly this standardization controls for any changes that effect test scores similarly across districts in the state.

We supplement the student-level Buffalo analyses with comparisons of district-level trends in Buffalo, Syracuse and Rochester based on NYSRC data. Because Rochester and Syracuse were subject to the same change in tests as was Buffalo, including Rochester schools in the analysis allows us to assess the likelihood that any changes observed in Buffalo in 2012–13 were simply artifacts of the change in tests rather than meaningful changes in student performance. While Syracuse does not provide a valid comparison group because Say Yes operates there, we would expect it to also be affected by the change in tests similarly to Buffalo.

III. Test Score Results

Figures 2.2 and 2.3 present trend lines of average districtwide math and ELA test scores in grades 3–8 for Buffalo, Syracuse and Rochester from 2005–06 to 2014–15. These figures represent simple average trends that do not control for other factors. As can be seen on the y-axis, average scores in each city in each year are well below the state average, typically by 0.5 to 1 standard deviation, as indicated by the negative scores.

For math (Figure 2.2) the pre-Say Yes trends in the three cities, and particularly in Rochester and Buffalo, track fairly closely, with general upward trends relative to the state

average between 2006–07 and 2009–10, followed by decreases from 2009–10 to 2011–12. Performance in Syracuse was largely flat in the earlier years but also declined in the later years. Starting in 2012–13, the first school year in which Say Yes was operating in Buffalo, the trends in the three cities diverge. Buffalo's scores rise sharply, and continue rising for three years, while Rochester and Syracuse decline or remain flat over the period. By 2014–15, Buffalo's average scores were closer to the state average than at any point in the time series.

As noted earlier, 2012–13 was also the first year that New York State administered tests aligned with the Common Core Standards. Thus, it is possible that the increase in Buffalo is solely or partly an artifact of the change in tests. While we cannot directly assess this possibility, two factors suggest this is not the case. First, alignment with the Common Core generally led to declines in test scores, but Buffalo saw a large increase. Second, if the change in tests differentially affected different types of districts, we would expect districts serving similar student populations to be affected in similar ways. Instead, we see increases in Buffalo that do not coincide with increases in Rochester and Syracuse.

The trends for ELA (Figure 2.3) are not as consistent as those for math. Buffalo's scores increased in each year between 2005–06 and 2009–10, then declined each year thereafter, though the downward trend was slower in the post-Say Yes years than in the years immediately preceding. Rochester and Syracuse had similar downward trends immediately preceding the start of Say Yes in Buffalo, though they had not experienced the increases from 2005–06 to 2009–10 that Buffalo did. Both cities also had generally declining scores in the post-Say Yes period, with no clear differences from Buffalo. Thus, we do not observe the same divergence between the three cities for ELA scores that is apparent for math scores.

The simple trends presented in Figures 2.2 and 2.3 do not control for other factors affecting the districts, such as changes in the student populations. By comparing each student's post-Say Yes scores to his or her pre-Say Yes scores using multiple regression analysis, we can more precisely measure how those students' scores changed over time relative to the state average, and whether those changes were statistically significant.

Table 2.1 presents the results for each post-Say yes year for the full sample and disaggregated by student race. The estimates in the upper panel for the full sample largely mirror the trends seen in Figures 2.2 and 2.3. Student test scores in Buffalo increased significantly in years 2 and 3 after the start of Say Yes relative to the pre-Say Yes period, with a gain of approximately one-tenth of a standard deviation in year 3. English Language Arts scores fell significantly in the first year of Say Yes but then showed no significant changes.

The remaining panels of Table 2.1 display test score changes disaggregated by student race. Math scores for Hispanic students did not change significantly over the period, while ELA scores fell slightly in the first two years post-Say Yes year. Math scores for African-American students rose significantly in years 2 and 3, though the increase was smaller than for the full sample. Scores in ELA fell significantly in each year, with a large decline of almost 0.14 of a standard deviation relative to the state average in year 3. Scores for White students were most similar to those for the overall sample, with an increase in math of 10 percent of a standard deviation in year 3 and small declines in ELA scores.

Overall, the test score analysis shows very different effects for math and English Language Arts relative to the state average in the years after Say Yes started in Buffalo. It is beyond the scope of this analysis to understand why the program might have such different effects on different tests, though this is a question worth further exploration. The results also

suggest that test score gaps between White and African American students may have widened over the post-Say Yes period. Math scores increased for both groups of students, with similar increases in the first two years, but exhibited a much larger increase for White students in year 3. Scores in ELA fell for both groups, but the declines were much larger for African-American students than for White students.

	2008-09	2009-10	2010-11	2011-12	2012-13	2013-14	2014-15
Cohort 1	8						
Cohort 2	7	8					
Cohort 3	6	7	8				
Cohort 4	5	6	7	8			
Cohort 5	4	5	6	7	8		
Cohort 6	3	4	5	6	7	8	
Cohort 7		3	4	5	6	7	8
Cohort 8			3	4	5	6	7
Cohort 9				3	4	5	6
Cohort 10					3	4	5
Cohort 11						3	4
Cohort 12							3

Figure 2.1. Sample Cohorts by Grade

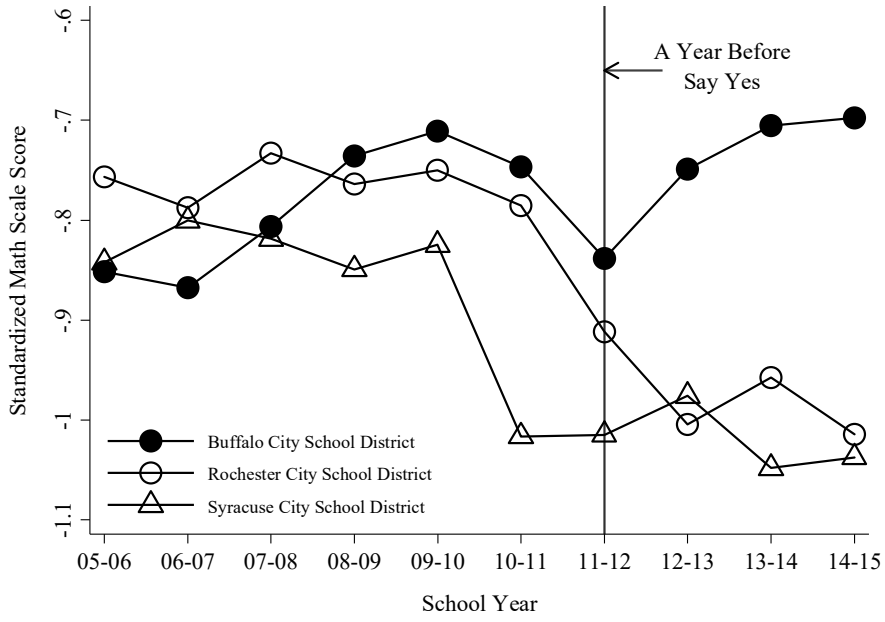


Figure 2.2. Trends in Average Math Scores, Buffalo, Syracuse, Rochester

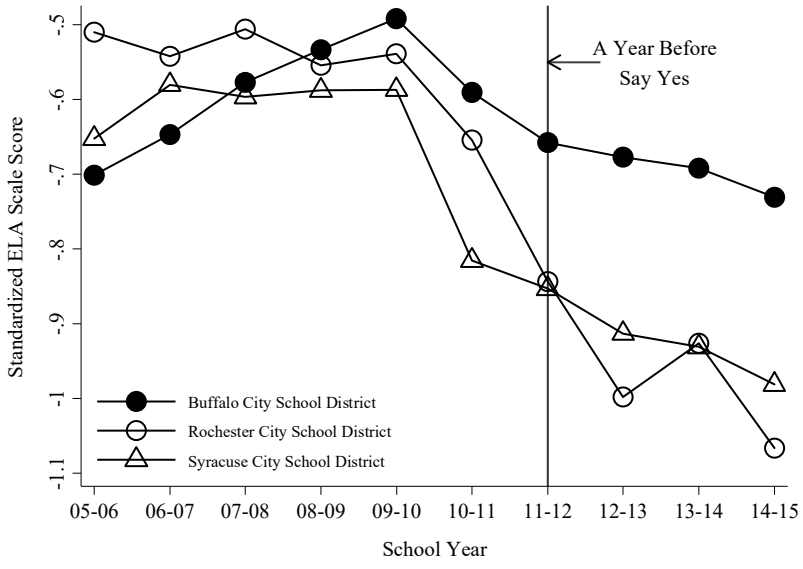


Figure 2.3. Trends in Average English Language Arts Scores, Buffalo, Syracuse, Rochester

Table 2.1. Effect Estimates, Three Years Post-Say Yes in Buffalo

Variable	Math	ELA
	Panel A: Full sample	
Post 1	0.031	−0.042***
	(0.022)	(0.014)
Post 2	0.079***	−0.026
	(0.027)	(0.021)
Post 3	0.105***	−0.049
	(0.031)	(0.031)
Sample size	98,503	97,402
	Panel B: Hispanic	
Post 1	−0.007	−0.058***
	(0.038)	(0.021)
Post 2	0.017	−0.061*
	(0.046)	(0.033)
Post 3	0.022	−0.051
	(0.056)	(0.043)
Sample size	14,886	14,886
	Panel C: African-American	
Post 1	0.013	−0.074***
	(0.019)	(0.015)
Post 2	0.053**	−0.066***
	(0.026)	(0.024)
Post 3	0.054*	−0.137***
	(0.031)	(0.033)
Sample size	55,210	55,473
	Panel D: White	
Post 1	0.025	−0.034*
	(0.029)	(0.020)
Post 2	0.053	−0.033
	(0.042)	(0.036)
Post 3	0.103***	−0.041
	(0.040)	(0.035)
Sample size	21,040	21,061

Note. Standard errors—clustered at the school-by-year level—in parentheses. The number of clusters ranges from 338 and 354. For the full sample, student scores are standardized using grade-by-year-specific statewide mean and standard deviations. For the sub-samples, student scores are standardized using race-by-grade-by-year-specific means.

PART 3: THE EFFECTS OF SAY YES TO EDUCATION ON HIGH SCHOOL GRADUATION, COLLEGE MATRICULATION AND COLLEGE PERSISTENCE

I. Introduction

Because the level of school supports Say Yes provides is generally broader than other "Promise-type" programs, and because the inclusion of private colleges makes the program more generous than most other merit-based and place-based scholarship programs, it provides a particularly interesting case study of the effects of guaranteed financial aid and school support services on high school graduation and college-going. There are at least three ways in which the program may affect college matriculation and persistence. First and most straightforward, the additional financial resources from the scholarships could make college attendance feasible for students who otherwise would not be able to afford it. Second, the transparent and comprehensive nature of the scholarships could provide important informational benefits, motivating students who otherwise would think they could not afford college to apply and enroll. Third, the scholarships and the other support services provided by the program could affect school culture and peer effects. We would expect the first two mechanisms to affect only those students eligible for the scholarships, while the third mechanism could affect all students enrolled in the BPS after the start of the program.

We find that high school graduation rates, college matriculation, and college persistence rates were all higher among cohorts eligible for Say Yes than for cohorts that graduate prior to Say Yes. For African-American students, graduation rates among twelfth graders, matriculation rates into both two and four year colleges among high school graduates, and persistence rates among matriculants into two-year colleges all increased for the post-Say Yes cohorts. Among

white students, effects were limited to increased matriculation into four year colleges among high school graduates. Our analyses also show that within the post-Say Yes cohorts, increases in college matriculation and persistence were limited to students who are eligible for Say Yes scholarships and were not seen among those ineligible for the scholarships.

The paper is organized as follows. Section II describes our data and methods, Section III presents estimates of impacts on high school graduation, college matriculation and persistence, overall and by race/ethnicity and Section IV compares the effects of Say Yes on eligible and ineligibles. Section V concludes by providing a summary of the results, discussion of potential implications, and a description of next steps in the evaluation.

II. Data and Empirical Strategy

Data

The BPS provides data on all high school graduates to the National Student Clearinghouse (NSC), which then matches the student-level records to NSC data for students enrolled in higher education institutions. The data provided to us includes both matches and non-matches from the NSC; that is, some records include a post-secondary institution and others do not. While some observations without post-secondary institutions may reflect students attending higher education institutions, who the NSC was unable to match (Dynarksi, Hemelt and Hyman, 2013), we assume that these students are high school graduates who did not enroll in a post-secondary institution.[19]

We then match the NSC data to student-level records from BPS for the eight classes of 12th graders from 2007–08 to 2014–15. Our match rate between the two data sets is very high,

[19] The Dynarski, Hemelt and Hyman paper reports match rates over 90% for New York State during the period covered in our analyses.

ranging from 98% to 100%. We define cohorts by 12th grade year. The first Say Yes scholarships were awarded in June of 2013, thus the Say Yes classes are defined as students who were in 12th grade in 2012–13, 2013–14, and 2014–15, while the classes from 2007–08 to 2011–12 are students who were not eligible for Say Yes initiatives and form our untreated group. We then follow students for two years post-high school graduation to observe whether they graduated from high school, as of August, following 12th grade enrollment, matriculated into college, and persisted into a second year of college.[20]

Empirical Methods

Our empirical strategy relies on cross-cohort comparisons to identify changes in high school graduation, college matriculation, and persistence rates into a second year of college. We supplement our primary analyses with subgroup analyses by race and scholarship eligibility status. The cross-cohort comparison is implemented by estimating the following regression:

$$y_{ist} = \alpha_0 + \alpha_1 D_t + \mathbf{X_{ist}}\Gamma + \alpha_2 T_t + \phi_s + \eta_{ist}. \quad (1)$$

Here, the outcome of interest (high school graduation, college matriculation, and persistence) is regressed on an indicator of whether or not the student is in the Say Yes cohorts, D_t; a set of student level covariates that include student gender, race, age, an indicator for limited English proficiency status and on time enrollment,[21] $\mathbf{X}_{ist}$; a set of high school fixed effects, ϕ_s; and time trends, T_t and η_{ist} is an error term. We estimate the model using the sample of all first-time 12th

[20] Whether a student persisted into a second year of college cannot be identified for the 2014–15 cohort as we do not have the NSC data for 2016.

[21] On time enrollee is equal to one if a student is enrolled by the BED day of 12th grade, which is typically the first Wednesday of October.

graders in each of the five years preceding Say Yes and the three years after the initiation of Say Yes, and the sample of all graduates from those same cohorts.

We are interested in the estimate ofα_1, which indicates the difference in an average of each outcome between the post-Say Yes cohorts and the five pre-Say Yes cohorts, controlling for individual student characteristics, time trends, and school fixed effects. Inclusion of school fixed effects ensures that estimates of the effect of Say Yes is based on comparison of cohorts exposed to Say Yes to earlier cohorts in the same school prior to Say Yes. Because the difference in treatment between treated and untreated cohorts is the entire Say Yes program, the effect estimates from this strategy reflects not only the impact of the Say Yes scholarship offer but also any effect of the additional services provided by the Say Yes program.

The key assumption required to interpret the effect estimates as the causal impact of the Say Yes program is that differences between nearby cohorts in the same grades and schools are essentially random. The assumption will be violated if the type of student selecting into Buffalo public schools is changing over time in ways that are not captured by the linear trend or non-linear changes in the observable covariates. This assumption is made more plausible by the fact that students are required to enroll in the district from grade 9 through 12 consecutively to qualify for the scholarships offer, and for all of the students in our sample decisions about where to enroll in ninth grade were made prior to the announcement of Say Yes. Thus, for the cohorts included in our sample, the Say Yes scholarship offer did not create an incentive to move into the district, and should not have affected the composition of the cohort.[22]

[22] Families entering the district to qualify younger sibling for the scholarship, however, could create changes in cohort composition. We do not observe siblings in our data and so cannot assess the extent to which this might have happened.

An important identifying assumption for the cross-cohort regression analyses is that the observed and unobserved characteristics of the 12th grade cohorts did not change systematically after the start of the program. We examine this issue in Table 3.1 which presents the results of covariate balancing tests across the cohorts. More specifically, we estimate a version of equation (1), dropping the covariates from the right-hand side, and using each one as the dependent variable in a series of separate regressions. The figures reported in the table are the estimated coefficients on the post-Say Yes variable and their standard errors. Statistically insignificant coefficient estimates indicate that there are no significant, observable differences between the post-Say Yes and pre-Say Yes cohorts. We include a control for trends and school fixed effects as we do in our subsequent analysis. Standard errors are clustered at the school-by-year level. The results show only small differences across the groups, none significantly different from zero. The lack of significant differences in observable student characteristics supports the identification strategy of cross-cohort comparisons.[23]

Analyzing the changes in outcomes by student subgroups is useful to help understand who might be benefitting the most from the Say Yes initiative. We therefore run the regression model separately for African-American, Hispanic, and white students. Note, however, that conducting the analysis separately by student ethnicity does not allow the statistical examination of the difference in effect estimates among subgroups. To examine whether there is a difference in effect estimates among subgroups, therefore, we estimate the following regression:

$$y_{ist} = \beta_0 + \beta_1 D_t + \beta_2 W_{ist} + \beta_3 (D_t \times W_{ist}) + \mathbf{X_{ist}}\Gamma + (W_{ist} \times \mathbf{X_{ist}}\Gamma) + \beta_4 T_t + \beta_5 (T_t \times W_{ist}) + \phi_s + (W_{ist} \times \phi_s) + \varepsilon_{ist},$$

[23] Data issues prevent us from including the percentage of students eligible for free and reduced price lunch as a proxy for poverty. While we would prefer to have these data, free lunch counts for older students are notoriously imprecise as many older students do not apply. Therefore, free lunch counts for 12th graders would likely underestimate poverty substantially and possibly be biased in unknown ways.

in which an indicator for white students, W_{ist} is interacted with all the variables in equation (1). In this setting, an estimate for coefficient β_3 allows us to test for the statistical significance of the difference in effect estimates between white and non-white students.

III. Results

Figure 3.1 presents high school graduation rates for 12th graders, college matriculation for all 12th graders in the previous year, and college matriculation among high school graduates in the previous year. Figure 3.2 displays trends for persistence into a second year of college for all 12 graders, all high school graduates, and all those who matriculated into college. The trends in Figure 3.1 suggest that high school graduation and college matriculation increased in the first year following the start of Say Yes and either maintained the increase in subsequent years or increased further. The trends are somewhat muddied, though, by a large drop in graduation and matriculation in 2011, the last before the program began. For high school graduation, the post Say Yes rates are similar to 2009 and 2010, before the program began. For college matriculation, whether conditional on high school graduation or not, the post-Say Yes rates are above those in any of the pre-Say Yes years. The persistence rates in Figure 3.2 show fewer pre-to-post differences than do the matriculation rates. The trends in persistence into a second year of college generally appear flat whether overall or conditional on high school graduation or college matriculation, and do not appear appreciably different after the start of the program.

Table 3.2 displays the estimated Say Yes effects from our basic models that include all students. All estimates are conditional on the observable characteristics shown in Table 3.1, school fixed-effects, and time trends. In column 1, which contains the results for the full sample, all post-Say Yes effect estimates are positive and, with the exception of high school graduation

are all significantly different from zero. Twelfth graders were 5.3 percentage points more likely to graduate at the end of the year after the start of Say Yes as compared to what is predicted based on students beginning 12th grade before the start of the program, though the estimate is not statistically significant. Twelfth graders were 8.6 percentage points more likely to matriculate in college. Average pre-Say Yes matriculation rates among 12th graders were approximately 41 percent, so the coefficient represents a 21 percent increase. High school graduates were 7.4 percentage points more likely to matriculate in college, an increase of 12.75 percent on a base of 58 percent.

Turning to college persistence, the full sample estimate in Table 2.2 indicates that 12th graders enrolled after Say Yes were 6.7 percentage points more likely to persist into a second year of college two years after starting 12th grade, an increase of 19.2 percent. Conditional on graduating high school, students graduating after Say Yes were 6.3 percentage points (12.75 percent) more likely to persist into a second year of college, and conditional on enrolling in college were 3.7 percentage points (5.27 percent) more likely to persist.

Taken together, these results suggest that, compared to predictions based on cohorts enrolled before the program began, Buffalo 12th graders were more likely to enroll in college following high school and to persist into a second year. The persistence result is particularly noteworthy. A common concern about scholarship programs is that they may induce enrollment among students who are unprepared or not sufficiently motivated to attend and graduate from college, thereby leading to higher college dropout rates. These results suggest that, while a higher percentage of students chose to enroll in college, a higher percentage of those students persisted. Thus, we do not see evidence that students at the margin enrolled but then dropped out of college.

The remaining columns in Table 3.2 examine the heterogeneity of effects by student race. We do not present results for Asian students because the sample sizes are small. The table shows largely significant positive effects for both African-American and white students. The estimates for Hispanic students are positive but not significantly different from zero. The estimates for African-American students, who make up about 56 percent of the sample, are generally similar to those for the full sample. The exception is high school graduation, where we find a significant increase of 8.7 percentage points as compared to only a 5.3 percentage point increase for the full sample. For white students, we find much more variable effects. While there is no effect on high school graduation among white 12th graders, there is a large 12.1 percentage point (26 percent) increase in college matriculation, and a 13.2 percentage point (22.5 percent) increase conditional on high school graduation.

As shown in the last column of Table 3.2, which compares the coefficients for white and non-white students, the high school graduation estimate is significantly higher for African-American students than for white students, while the college matriculation effects, conditional on graduating, are significantly higher for white students. These differences suggest that while the program appears to have raised college matriculation rates for both white and African-American students, the effects are largest at different points in the pipeline. The much larger effect on high school graduation for African-American students leads to a larger pool of potential college matriculants, and an increase in the share of that pool that does matriculate. For white students, the effect is largely to induce potential matriculants (high school graduates) to attend college, not to grow the pool of potential matriculants. The estimated matriculation effects for white students are quite large.

Turning to persistence, we find positive and significant effects of similar size for both African-American and white 12th graders and high school graduates. For example, African-American 12th graders are 7.7 percentage points (23.6 percent) more likely to be enrolled in college two years after starting 12th grade, and African-American high school graduates are 6.5 percentage points (13.6 percent) more likely to be enrolled in a second year of college. The corresponding estimates for white students are 7.5 percentage points (17.8 percent) and 8 percentage points (15 percent). Conditional on college matriculation, though, only the estimate for African-American students is statistically significant (6.8 percentage points). Thus, it appears that Say Yes increases the percentage of African-American students enrolled in a second year of college by increasing rates of progression at all stages of the pipeline: high school graduation, college matriculation and persistence. For white students, the program increased the percentage of students enrolled in a second year of college by increasing college matriculation rates, but not by raising the percentage of those matriculants who persist.

Table 3.3 examines college matriculation and persistence overall and by race, but this time separating the effects by two-year and four-year higher education institutions. The first column displays the full sample results and shows significant increases post-Say Yes in matriculation at both two-year and four-year institutions, using either 12th graders or high school graduates as the cohort. Estimates also indicate that among matriculants of two-year colleges, Say Yes increases persistence, although the estimated impact is only marginally statistically significant. Among matriculants of four-year institutions, there is no apparent effect on persistence.

Turning to the results disaggregated for African-American and white students, somewhat different patterns by race are apparent. For African-American students, the college matriculation

estimates for both two-year and four-year institutions, not conditional on high school graduation, are positive and significant, and of similar magnitude (approximately 4 percentage points). For white students, matriculation gains are concentrated in four-year institutions (approximately 10 percentage points), and are significantly larger than those for African-American students. Persistence increased significantly (9.8 percentage points) among African-American students who matriculated at two year institutions, but not among African-American who matriculated at four-year institutions or among white students at either type of institution.

The results in Tables 3.2 and 3.3 appear consistent with each other. For African-American students, Say Yes increased the likelihood of high school graduation (conditional on reaching 12th grade) and college matriculation at both two-year and four-year institutions among high school graduates. It is plausible that students on the margin of graduating high school would be more likely to attend two year institutions, while non-borderline graduates would attend four-year institutions. For white students, there appears to be no effect on high school graduation but a large effect on college matriculation among graduates; these non-borderline high school graduates may have been more likely to enroll in four-year institutions if they were going to attend college at all.

IV. Effects on Scholarship-Eligible and Scholarship-Ineligible Students

As described above, not all of the students in the post-Say Yes cohorts are eligible for Say Yes scholarships. Specifically, students who enter Buffalo public schools after ninth grade are not eligible for the scholarship even after Say-Yes was initiated. Other studies of placed-based scholarship programs with a similar eligibility limitation have relied on comparisons of changes in matriculation rates among eligible and ineligible students to compute difference-in-differences estimates of the scholarship offer's impact.

We believe such difference-in-differences estimates are unlikely to accurately reflect the effect of the Say Yes program for two reasons. First, the scholarship offer might influence the college going decisions of ineligible student as well as eligible students. This possibility is especially salient for the Say Yes program where ineligible students may have received some of the support services offered by the Say Yes program including college counseling. However, even for other placed-based scholarship programs ineligible students may be influenced by the decisions of their eligible classmates via peer effects. Bifulco, Fletcher and Ross (2011) show evidence that suggests that increases in the proportions of classmates choosing to attend college can influence the college enrollment decisions of individual students. Also, one of the goals of many placed-based scholarship is to foster a college going culture among students, teachers and parents, which can influence ineligible as well as eligible students.

Second, eligible students are likely to be considerably different than ineligible students, making it difficult to interpret changes in matriculation rates among ineligible students as an estimate of changes that would have been observed for eligible students in the absence of the scholarship offer.

Table 3.4, which compares observable characteristics of scholarship-eligible and ineligible students demonstrates that these students differ in several important ways. Notably, ineligible students are more likely to be Asian or Hispanic, to have limited English proficiency and to have enrolled late in their 12^{th} grade year. It is quite likely that many of these students are recent immigrants to the United States. As might be expected, they are also much less likely to graduate from high school and to enroll in college. The differences suggest that students not enrolled for the full four years of high school are different from scholarship-eligible students in important ways that make them unsuitable as a comparison group.

Nonetheless, comparison of the effect of Say Yes on eligible and ineligibles are informative, and we present them in Table 3.5.[24] The estimates suggest that the program effects are entirely concentrated among scholarship-eligible students. For eligible students, the estimates are large and significant for each outcome variable and, in each case, larger than the comparable estimates for the full sample. In contrast, there are no significant increases among non-eligible students. For non-eligible students, college matriculation among graduates has a negative coefficient, while the estimates are fairly small and not significantly different from zero for the remaining outcome variables. In the absence of an underlying causal model we would hesitate to characterize the negative matriculation estimate as a program effect, though it is possible that non-eligible students were crowded out at some institutions by the increase in scholarship students, or suffered a demoralization effect from not receiving the scholarships.

Table 3.6 disaggregates the matriculation analysis by type of post-secondary institution. Again, the estimates are positive and significant for scholarship-eligible students, at both two-year and four-year colleges, and the coefficients are similar across types of institutions. For ineligible students, the estimates are positive at two-year colleges and strongly negative for four-year institutions, though only significantly different from zero for four-year matriculation among all 12th graders (-10 percentage points). The sample sizes are small so we cannot draw any definitive conclusions, but the results provide some suggestive evidence that the Say Yes scholarships may have induced some ineligible students to enroll at two-year rather than four-year institutions, or to not enroll in post-secondary education.

[24] We treat all students who are continuously enrolled for grades 9-12, as well as those who graduate in three years and those attending the BPS for all but one high school grade as eligible. The program has an appeals process for students in special circumstances and these students would likely receive scholarships after appeal. Approximately 91% of 12th graders attend grades 9-12, with a slightly higher percentage in the pre-Say Yes period than in the post-Say Yes period (91% v. 89%).

Differences in effects across students eligible and ineligible for the Say Yes scholarship could reflect the fact that eligible and ineligible students tend to have different family and educational backgrounds, and the effect of the Say Yes interventions differ across different kinds of students. Alternatively, the results in Tables 3.5 and 3.6 may indicate that the multiplier effects generated through peer effects and changes in the college going cultural are minimal, at least in the early years of the Say Yes program. If so, that suggests that the effects of the Say Yes program on educational attainment are explained primarily by either reduction in the cost of attending college which are limited to those eligible for the scholarship or the increased clarity and simplicity of information about the availability of financial assistance for college created by the Say Yes scholarship offer.

V. Conclusions and Next Steps

The analyses presented in this paper examine high school graduation among 12th graders, college matriculation and second-year college persistence after the start of the Say Yes to Education Program in Buffalo. Using a cross-cohort estimation strategy, we find consistent evidence of increases in college matriculation. These increases are statistically significant and economically meaningful. The analyses suggest that increases in college matriculation among African-American students were driven both by increases in the percentage of 12th graders graduating from high school and increases in the percentage of those graduates enrolling in college. For white students, the increases appear to be entirely from a much higher percentage of high school graduates enrolling in four-year colleges.

We also see a higher percentage of students in the post-Say Yes cohorts persisting into a second year of college. Thus, it appears that Say Yes is not merely encouraging college matriculation among marginal students who are destined to drop out, but rather is attracting

students into college for sustained lengths of time. In some cases, such as for African- American students at two-year institutions, Say Yes appears to increase persistence among those who matriculate—suggesting additional benefits of a transparent offer of financial aid.

We also find that the positive effects are entirely concentrated among BPS students who are eligible for the scholarships. Not only does this strengthen the conclusion that increase in college matriculation and persistence can be attributed to Say Yes, but it also provides suggestive evidence about the likely mechanisms of the Say Yes effect. As described above, the effects on college-going and persistence likely occur through three primary mechanisms: better information on college costs, direct financial resources, and the school-wide effects of increased school supports and additional college-going peers. The comparison of scholarship-eligible and ineligible students suggests that information and financial resources are the primary factors related to improvements in post-secondary outcomes.

While the data do not allow us to directly disentangle the two hypothesized causal mechanisms of the scholarships, several pieces of information suggest that the informational effects may be stronger than the resource effects in the Say Yes to Education case. First, we find stronger effects on college matriculation than on college persistence. Matriculation effects are likely to be related to both information and resources. Persistence is likely to be affected only by the financial resources available through the scholarships, as college students would already be aware of the financial aid they received.

Second, as a last-dollar scholarship, it is possible that many lower-income students receive relatively little financial aid from the program. This is particularly true for two-year colleges, where Pell grants would cover the full-cost of tuition for most public institutions. In the analyses for African-American students we find significant increases in two-year college

attendance. While our data do not allow us to directly identify students eligible for Pell Grants or other need-based financial aid, 76 percent of the district's students were classified as economically disadvantaged in 2013–14. Thus, it is likely that many of the students attending two-year institutions receive little or no direct financial support from the program. For those attending four-year institutions, particularly private universities, both the information and financial resources are likely to be important.

The analyses also suggest areas for future research. We are in the process of addressing two data limitations in the current research by obtaining information on high school free and reduced price lunch eligibility and school withdrawal codes. The free and reduced price lunch eligibility will enable us to examine differential effects by student income, though these measures are likely to be imperfect for high school students. The withdrawal codes will allow us to identify which students drop out or transfer out of the district during high school, enabling us to measure student progress from 9th grade rather than 12th grade and better examine the program's potential effects on dropouts and high school graduation. We also hope to disaggregate matriculation and persistence by private and public university status, to help further understand how the program may be affecting students' post-secondary choices.

Perhaps most importantly, these analyses examine only the first two years of Say Yes recipients' college careers. While improving college access for underrepresented groups of students is an important policy goal, abundant evidence suggests that the largest returns to a college education accrue to those earning a degree, not simply attending college (see, for example, Jaeger and Page, 1996). Therefore, it is particularly important to continue to track BPS students through their college careers to evaluate the program's effects on college graduation

rates. We hope to continue this work over the next several years as students reach four-year and six-year college graduation markers.

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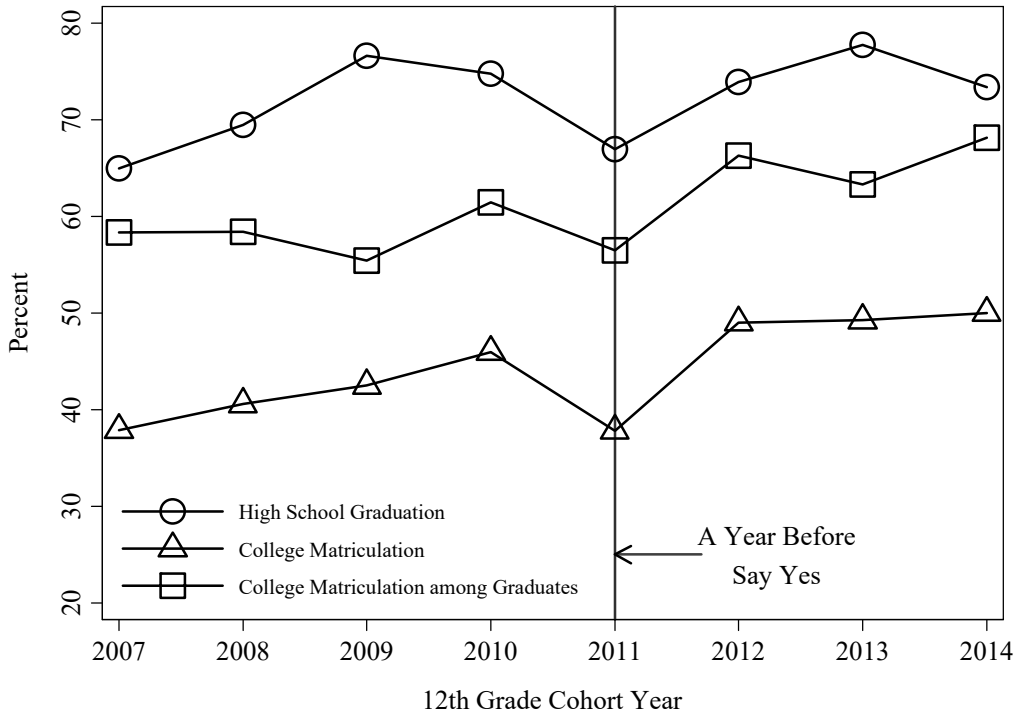

Figure 3.1. High School Graduation and College Matriculation, by Cohort

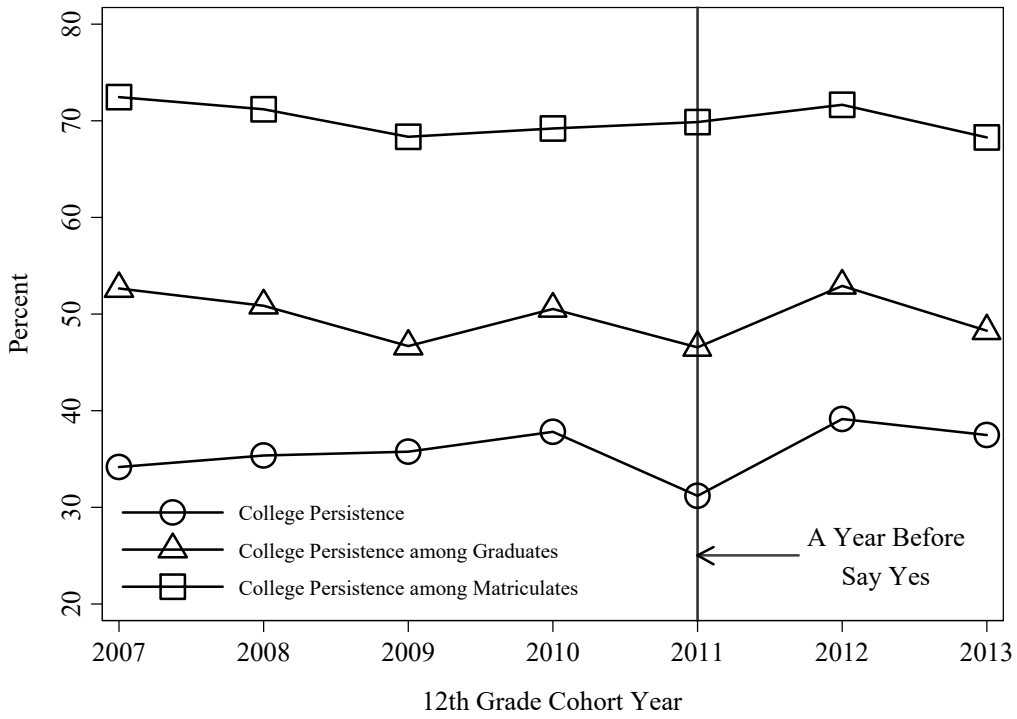

Figure 3.2. College Persistence, by Cohort

Table 3.1. Tests of Covariate Balance (Based on First-Time 12th Graders)

Outcome variable	Explanatory variable: Say Yes cohorts
Age (in years)	−0.028
	(0.035)
Female	0.004
	(0.014)
American Indian	−0.001
	(0.003)
Asian	0.018
	(0.011)
African American	−0.023
	(0.016)
Hispanic	0.008
	(0.012)
White	−0.002
	(0.012)
Limited English proficiency	−0.003
	(0.012)
On time enrollee	0.006
	(0.010)
Grade repeater	0.008
	(0.017)
School fixed effects	Yes
Controls for trends	Yes
Number of observations	16,163

Note. The Say Yes cohorts correspond to the 2012–13, 2013–14, and 2014–15 cohorts. The control groups are the 2007–08, 2008–09, 2009–10, 2010–11, and 2011–12 cohorts. The number of observations used for the balancing test analysis of "Grade repeater" is 18,197. "On time enrollee" is equal to one if a student is enrolled by the BEDS day, which is typically the first Wednesday of October. "Grade repeater" is equal to one if a student is not a first time 12th grader. Standard errors—clustered at the school-by-year level—are in parentheses. The number of clusters is 138. $*p \leq 0.10$; $**p \leq 0.05$; $***p \leq 0.01$.

Table 3.2. Outcome Analysis (Explanatory variable: An Indicator for the Say Yes cohorts)

	Sample				
Outcome variable	Full sample	African American	Hispanic	White	White vs non-white
High school graduation	0.053	0.087**	0.008	0.006	−0.067*
[*N* = 16,163]	(0.032)	(0.043)	(0.041)	(0.028)	(0.038)
College matriculation	0.086***	0.088***	0.023	0.121***	0.043
[*N* = 16,163]	(0.024)	(0.032)	(0.038)	(0.032)	(0.036)
College matriculation among graduates	0.074***	0.057**	0.023	0.132***	0.080**
[*N* = 11,650]	(0.020)	(0.026)	(0.047)	(0.033)	(0.037)
College persistence	0.067***	0.077***	0.035	0.075**	0.020
[*N* = 14,186]	(0.019)	(0.020)	(0.044)	(0.030)	(0.029)
College persistence among graduates	0.063***	0.065***	0.062	0.080**	0.080
[*N* = 10,199]	(0.019)	(0.020)	(0.060)	(0.031)	(0.031)
College persistence among matriculate	0.037*	0.068***	0.080	−0.001	−0.058
[*N* = 6,123]	(0.020)	(0.024)	(0.076)	(0.031)	(0.037)
School fixed effects			Yes		
Controls for trends			Yes		

Notes: The Say Yes cohorts correspond to the 2012–13, 2013–14, and 2014–15 cohorts. The control groups are the 2007–08, 2008–09, 2009–10, 2010–11 and 2011–12 cohorts. The number of observations (*N*) are in brackets. High school graduation as of August. College matriculation between August 15 and October 31 of the year following 12th grade. The 2014–15 cohort is not used for the analysis of the "college persistence" variables. All regressions are conditional on age, race, sex, limited English proficiency, and on time enrollment. Standard errors—clustered at the school-by-year level—are in parentheses. The number of clusters ranges from 89 to 138, depending on the outcome variable analyzed. *$p \leq 0.10$; **$p \leq 0.05$; ***$p \leq 0.01$.

Table 3.3. Effects on College Matriculation, by College Type

	Sample			
Outcome variable	Full sample	African American	White	White vs non-white
2-year college matriculation	0.041***	0.043**	0.027	−0.019
[*N* = 16,163]	(0.015)	(0.022)	(0.020)	(0.026)
2-year college matriculation among graduates	0.031**	0.021	0.029	−0.002
[*N* = 11,650]	(0.015)	(0.020)	(0.024)	(0.029)
4-year college matriculation	0.045***	0.046***	0.094***	0.061**
[*N* = 16,163]	(0.014)	(0.016)	(0.026)	(0.026)
4-year college matriculation among graduates	0.044***	0.038**	0.103***	0.080**
[*N* = 11,650]	(0.015)	(0.019)	(0.029)	(0.031)
College persistence among 2-year matriculate	0.064*	0.098**	0.008	−0.079
[*N* = 2,764]	(0.033)	(0.039)	(0.059)	(0.068)
College persistence among 4-year matriculate	0.002	0.024	−0.029	−0.049
[*N* = 3,357]	(0.021)	(0.026)	(0.028)	(0.035)
School fixed effects	Yes			
Controls for trends	Yes			

Note. The number of observations (*N*) are in brackets. All regressions are conditional on age, race, sex, limited English proficiency, and on time enrollment. Standard errors—clustered at the school-by-year level—are in parentheses. The number of clusters ranges from 88 to 138, depending on the outcome variable analyzed. $*p \leq 0.10$; $**p \leq 0.05$; $***p \leq 0.01$.

Table 3.4. Comparison of Scholarship-Eligible and Ineligible Students

	Pre-Say Yes cohorts		Post-Say Yes cohorts	
Variable	Eligible	Ineligible	Eligible	Ineligible
% graduate high school	0.734	0.555	0.769	0.600
% matriculate college	0.428	0.299	0.516	0.320
% persist to second year	0.359	0.247	0.267	0.161
Age (in years)	17.806	17.937	17.838	18.162
Female	0.525	0.524	0.521	0.496
Indian	0.015	0.014	0.012	0.013
Asian	0.021	0.082	0.062	0.185
African American	0.576	0.485	0.554	0.429
Hispanic	0.108	0.157	0.127	0.137
White	0.277	0.257	0.243	0.230
Limited English proficient	0.036	0.134	0.053	0.176
On-time enrollee	0.980	0.858	0.988	0.867
Grade Repeater	0.106	0.075	0.133	0.101
Number of observations	7,324	707	5,252	664

Note. Figures report are average within the five pre- or three post-Say Yes cohorts. For the "% persist to second year" variable, the figures are average within the four pre- and two post-Say Yes cohorts.

Table 3.5. *Outcome Analysis, by Scholarship Eligibility*

Variable	Explanatory Variable: Say Yes cohorts (1 = yes)
	Panel A: Eligible students
High school graduation (1 = yes) [*N* = 12,576]	0.083**
	(0.034)
College matriculation (1 = yes) [*N* = 12,576]	0.119***
	(0.026)
College matriculation among graduates (1 = yes) [*N* = 9,418]	0.094***
	(0.023)
College persistence (1 = yes) [*N* = 10,821]	0.093***
	(0.020)
College persistence among graduates (1 = yes) [*N* = 8,100]	0.077***
	(0.023)
College persistence among matriculates (1 = yes) [*N* = 4,939]	0.049**
	(0.023)
	Panel B: Ineligible students
High school graduation (1 = yes) [*N* = 1,371]	0.079
	(0.060)
College matriculation (1 = yes) [*N* = 1,371]	−0.052
	(0.043)
College matriculation among graduates (1 = yes) [*N* = 792]	−0.167**
	(0.066)
College persistence (1 = yes) [*N* = 1,149]	−0.030
	(0.039)
College persistence among graduates (1 = yes) [*N* = 659]	−0.088
	(0.062)
College persistence among matriculates (1 = yes) [*N* = 344]	0.015
	(0.090)
	Panel C: Tests of difference
High school graduation (1 = yes) [*N* = 13,947]	0.004
	(0.044)
College matriculation (1 = yes) [*N* = 13,947]	0.172***
	(0.036)
College matriculation among graduates (1 = yes) [*N* = 10,210]	0.261***
	(0.063)
College persistence (1 = yes) [*N* = 12,149]	0.124***
	(0.038)
College persistence among graduates (1 = yes) [*N* = 8,835]	0.165**
	(0.065)
College persistence among matriculates (1 = yes) [*N* = 5,352]	0.033
	(0.088)
School fixed effects	Yes
Time trends	Yes

Notes: The Say Yes cohorts correspond to the 12–13, 13–14, and 14–15 cohorts. The control groups are the 08–09, 09–10, 10–11 and 11–12 cohorts. The number of observations (*N*) are in brackets. High school graduation as of August. College matriculation between August 15 and October 31. The 14–15 cohort is not used for the analysis of the "college persistence" variables. All regressions are conditional on age, race, sex, limited English proficiency, and on time enrollment. Standard errors—clustered at the school-by-year level—are in parentheses. The number of clusters ranges from 92 to 121, depending on the outcome variable analyzed. *$p \leq 0.10$; **$p \leq 0.05$; ***$p \leq 0.01$.

Table 3.6. Outcome Analysis by College Type and Scholarship Eligibility

Outcome variable	Explanatory variable: Say Yes cohorts (1 = yes)
	Panel A: Eligible students
Two -year college matriculation (1 = yes)	0.055***
[*N* = 12,576]	(0.016)
Two -year college matriculation among high school graduates (1 = yes)	0.040**
[*N* = 9,418]	(0.018)
Four-year college matriculation (1 = yes)	0.065***
[*N* = 12,576]	(0.015)
Four -year college matriculation among high school graduates (1 = yes)	0.055***
[*N* = 9,418]	(0.017)
	Panel B: Ineligible students
Two -year college matriculation (1 = yes)	0.047
[*N* = 1,371]	(0.035)
Two -year college matriculation among high school graduates (1 = yes)	0.044
[*N* = 792]	(0.053)
Four-year college matriculation (1 = yes)	−0.100***
[*N* = 1,371]	(0.035)
Four -year college matriculation among high school graduates (1 = yes)	−0.212
[*N* = 792]	(0.066)
	Panel C: Tests of difference
Two -year college matriculation (1 = yes)	0.007
[*N* = 2,445]	(0.032)
Two -year college matriculation among high school graduates (1 = yes)	−0.004
[*N* = 10,210]	(0.052)
Four-year college matriculation (1 = yes)	0.165***
[*N* = 13,947]	(0.035)
Four -year college matriculation among high school graduates (1 = yes)	0.267***
[*N* = 10,210]	(0.066)
School fixed effects	Yes
Time trends	Yes

Note. The Say Yes cohorts correspond to the 12–13, 13–14, and 14–15 cohorts. The control groups are the 08–09, 09–10, 10–11 and 11–12 cohorts. The number of observations (*N*) are in brackets. All regressions are conditional on age, race, sex, limited English proficiency, and on time enrollment. Panel C tests for the difference in the effect estimates between eligible and ineligible students. Standard errors—clustered at the school-by-year level—are in parentheses. The number of clusters ranges from 86 to 121, depending on the outcome variable analyzed. *$p \leq 0.10$; **$p \leq 0.05$; ***$p \leq 0.01$.