# The Effect of Private School Vouchers on Political Participation: Experimental Evidence from New York City

Deven Carlson
University of Oklahoma

Matthew M. Chingos
Urban Institute

David E. Campbell
University of Notre Dame

October 2016

**Abstract:**
In 1997, the New York School Choice Scholarships Foundation Program (SCSF) randomly offered three-year scholarships to attend private schools to approximately 1,000 low-income families in New York City. In this paper we leverage exogenous variation generated by the SCSF to estimate the causal effect of the private school voucher offer—and the private school attendance it induced—on later-life voter registration and turnout outcomes. Our results demonstrate that the voucher intervention had no effect on registering to vote or voting in any of several elections for either the full sample or any of several demographic subgroups. Although unique aspects of the SCSF context impose limits on the scope of our results, they have clear implications for assessing the relative effectiveness of public and private schools in preparing democratic citizens. We close the paper by discussing the implications of the results for research and policy.

**Keywords:** Private school vouchers, political participation, experimental design

1

# Introduction

Education reform efforts of the past 20 years have focused significant attention on improving student achievement outcomes, particularly in reading and math. Although achievement outcomes are undoubtedly important—increased achievement has been linked to greater rates of college attendance, higher salaries, and lower rates of teen birth (Chetty, Friedman, and Rockoff 2014), among other outcomes—they are far from the only set of outcomes that education is thought or intended to shape. Indeed, education has been shown to affect a wide variety of outcomes across the economic, cultural, social, and political realms (see Haveman and Wolfe 1984 for an early catalog of the range of outcomes affected by education).

In the political realm, there is a longstanding belief that a society's education system is a primary means for creating the informed citizenry that a sustainable democratic society requires (Dewey 1916; Gutmann 1987). Empirical work has provided strong evidence that an individual's educational experiences—particularly attainment levels—affects his or her participation in the democratic process (e.g. Milligan, Moretti, and Oreopolous 2004; Dee 2004; Sondheimer and Green 2010), but there is less evidence as to whether particular aspects of a society's education system more broadly affect individuals' civic engagement (but see Dee 2005; Howell et al. 2006; Greene 1998). Perhaps the most fundamental aspect of a society's education system concerns the nature and extent of governmental involvement in the provision of education (Chubb and Moe 1990). In the United States, there is a well-established system of publicly financed and operated schools designed to provide universal primary and secondary education. There is also, however, a long history of private schooling in the U.S., and a nontrivial percentage of students are educated in these institutions. Private schools have historically received very little, if any, public financial support and have thus operated largely outside of the formal democratic process.

The differences in the democratic nature—and corresponding accountability—of public and private schools have served as a basis for scholars to theorize about potential differences in the relative effectiveness of the two schooling sectors in preparing democratic citizens. One line of reasoning holds that—at least in part because of their lack of democratic governance—private schools may focus on imparting a body of knowledge and skills that disproportionately generate private returns, at the expense of social benefits.

An opposing school of thought, however, contends that the democratic nature of public schools is overstated, primarily because the historical policy of assigning students to schools on the basis of the location of their residence simply results in schools reflecting segregated housing patterns. Adherents to this viewpoint often contend that the track record of public schools in preparing democratic citizens is quite mixed, citing low voter participation among many demographic groups as well as the absence of tolerance, political knowledge, and other democratic values among large swaths of the population.

In this paper, we take advantage of exogenous variation in the opportunity to attend a private school to provide experimental evidence on this issue. In particular, we leverage the variation generated by the New York School Choice Scholarships Foundation Program (SCSF), which in 1997 randomly offered three-year scholarships to attend private schools to as many as 1,000 low-income families in New York City, to estimate the causal effect of the voucher offer on later-life political participation outcomes. Although our results are specific to the particular context of the SCSF, which differs in important ways from many recent private school voucher proposals, they have clear implications for assessing the relative effectiveness of public and private schools in preparing democratic citizens. Our results demonstrate that being offered a voucher has no effect on registering to vote and voting in any of several elections. In addition,

our results provide no evidence of systematic effects of the voucher offer across several demographic subgroups.

We proceed by providing context for this study, describing the relationship between vouchers and schooling sector, reviewing relevant literature, and providing a more detailed overview of the SCSF. We then detail the data that underlie our analysis and outline our analytic approach before presenting our results. We close the paper by discussing the implications of the results for theory, future research, as well as policy.

## Political Participation, Education, and Private School Vouchers

Literature in political science has long demonstrated a strong positive relationship between educational attainment and political participation (e.g. Campbell et al. 1960; Campbell, Gurin, and Miller 1954; Key 1961; Verba and Nie 1972; Converse 1972; Wolfinger and Rosenstone 1980; Rosenstone and Hansen 1993; Verba, Schlozman, and Brady 1995) and recent work has leveraged plausibly exogenous variation to provide evidence that this relationship is likely causal in nature (e.g. Milligan, Moretti, and Oreopolous 2004; Dee 2004; Sondheimer and Green 2010). A separate line of work has explicitly examined how civic education practices and contexts shape individuals' engagement with the democratic process. For example, this set of studies illustrates that factors such as formal civics instruction (Niemi and Junn 1998; Conover and Searing 2000; Gimpel, Lay, and Schhuknecht 2003; Bachner 2010), a school's civic culture (Campbell 2006), and classroom diversity and climate (Campbell 2007; 2008) are related to a range of civic outcomes, including civic knowledge (Niemi and Junn 1998; Gimpel, Lay, and Schhuknecht 2003; Campbell 2007), political discussion (Gimpel, Lay, and Schhuknecht 2003; Campbell 2007), political efficacy (Gimpel, Lay, and Schhuknecht 2003), participatory

intentions (Campbell 2006; 2007; 2008), and voting in a future election (Bachner 2010; Campbell 2006).

Although a fair amount of research—situated primarily in political science—has analyzed how particular civic education policies and practices have shaped students' civic outcomes, there is substantially less work examining how more general features of the education system may affect democratic citizenship. As noted earlier, the nature and extent of government involvement in the provision of education is perhaps the most fundamental aspect of an education system. In the United States, there is a continuum of government involvement in the provision of education. At one end of this continuum is the well-established system of publicly financed and operated schools designed to provide universal primary and secondary education—the nation's public school system. At the other end of the continuum are private schools, which receive very little, if any, public financial support and are largely funded with student tuition dollars. Between these two poles are various programs that provide families with dollars—either public or private—that can be used to attend private schools. Among the most visible, and controversial, such programs are private school vouchers.

Private school voucher programs have taken several different forms since establishment of the first one in Milwaukee in the early 1990s. They have ranged from small, narrowly targeted programs funded with private dollars—early programs in New York City, Dayton, and Washington, D.C. are examples (Howell et al. 2006)—to publicly-funded, statewide policies with broad eligibility criteria that have recently been adopted in Louisiana, Wisconsin, Indiana, and other states. There is an extensive scholarly literature estimating the effects of private school vouchers. These studies have most commonly estimated the effects on student achievement outcomes, typically finding either null effects (Witte 2000; Metcalf et al. 2003) or small positive

effects (e.g. Peterson, Howell, and Greene 1999; Greene, Peterson, and Du 1998), if only for a single subgroup (Howell et al. 2006; Barnard et al. 2003) or subject (Wolf et al. 2013; Rouse 1998). However, two recent evaluations of the Louisiana Scholarship Program provided the first evidence of negative achievement effects of a private school voucher program (Abdulkadiroglu, Pathak, and Walters 2015; Mills and Wolf 2016).

A number of recent studies have moved beyond student achievement to estimate the effect of private school vouchers on educational attainment outcomes. Relative to studies estimating the achievement effects of voucher programs, these attainment-oriented studies have generally found more positive results (Wolf et al. 2013; Cowen et al. 2013; Chingos and Peterson 2015). Using data from Milwaukee, Cowen et al. (2013) provide evidence of a positive relationship between voucher use and postsecondary persistence. Chingos and Peterson (2015) draw on data from New York City and find no full-sample effects of a voucher offer on college enrollment or four-year degree attainment, but positive effects for minority students and the children of women born in the United States.

Why might we expect participation in a voucher program to affect political participation, and specifically voter turnout? One reason builds on the aforementioned studies that exploit exogenous variation in educational attainment, which have generally found evidence favoring a causal relationship between educational attainment and political involvement. Perhaps the most relevant study was done by Sondheimer and Green (2010) in which they examined the impact of randomized educational interventions and concluded that voter turnout is indeed higher among disadvantaged populations who participate in programs that lead to greater educational attainment. Given that conclusion, if—as described above—voucher programs increase the odds

of some participants completing higher education, then we might expect them to have higher levels of voter registration and turnout as well.

Beyond potential attainment-related effects of vouchers, however, there is another reason why they might lead to higher turnout. One motivation for political participation, especially voting, is a sense of civic duty (Riker and Ordeshook 1968; Blais 2000; Verba, Schlozman, and Brady 1995) and there is reason to think that some private schools may more successfully inculcate a sense of civic responsibility than their public counterparts. Campbell (2006) demonstrates that the civic ethos of a school, whether public or private, can have a long-term impact on individuals' sense of civic duty. Other scholars have suggested that private schools—especially religious schools—have an especially strong civic ethos (Coleman and Hoffer 1987; Bryk, Lee, and Holland 1993). In arguing for the democratic virtues of school choice, Moe lays out a theoretical explanation for why private schools are better able to foster a sense of community (i.e. civic ethos) than are public schools. In his words, school choice would "tend to promote the emergence of schools as true communities. As parents choose their own schools, they are more likely to identify with them, to share their values and missions, to trust one another, and to have respect for teachers and principals" (2000, 144-45).

Relative to their achievement and attainment impacts, the effects of private school vouchers on later-life political or civic behaviors have been examined with significantly less frequency. A small amount of older work examines the relationship between private schooling and contemporaneous civic outcomes—generally altruism, political tolerance, and volunteerism—and typically finds a private school advantage (e.g. Bettinger and Slonim 2006; Howell et al. 2006; Greene 1998; Campbell 2001). In addition, Dee (2005) estimates the effect of Catholic schooling on later-life voting behavior and finds a positive effect, but also presents

evidence that the analysis may not have adequately addressed selection concerns. More recently, studies have used data from Milwaukee (Fleming 2014; Fleming, Mitchell, and McNally 2014) and Louisiana (Mills et al. 2016) to examine the relationship between private school voucher receipt and civic outcomes. The work from Milwaukee finds voucher students to exhibit slightly higher levels of political tolerance, civic skills, future participation, and civic voluntarism than observably similar public school students (Fleming, Mitchell, and McNally 2014). Moreover, Fleming (2014) finds that voucher parents are more likely than public school parents to report that their experiences with their child's school has taught them how government works—voucher parents also report that their experience with the voucher program has increased their political activity. The results should be interpreted cautiously, however, given issues with either selection into the voucher program (Fleming 2014; Fleming, Mitchell, and McNally 2014), or low response rates (Mills et al. 2016).[1] Further, these recent studies rely on self-reported behavior—as opposed to administrative records—and typically do not analyze electoral turnout, which is arguably the civic outcome of greatest interest.

The relative scarcity of evidence surrounding the civic effects of private school voucher programs is perhaps unsurprising given the focus on student achievement that has accompanied the standards and accountability movement over the past 20 years. In addition, from a pure logistical standpoint, academic outcomes such as student achievement are much more proximate to the timing of voucher receipt than later-life participation in the political process. However, even if the relative lack of studies estimating the effects of private school vouchers on political and civic outcomes is understandable, it does not mean such studies are unimportant (see Levin 2002). Indeed, the differences in the democratic nature and corresponding accountability of

---

[1] Although Mills et al. (2016) take advantage of the admissions lottery for the Louisiana Scholarship Program, their results are based on a survey with an 11 percent response rate.

public and private schools—and the potential for private school voucher programs to shape enrollment in these two sectors—have served as a basis for debates over the relative effectiveness of the two schooling sectors in preparing democratic citizens.

In the following section we describe a program that provided over 1,000 families with a voucher that could be used to attend a private school. As will become apparent, these vouchers were provided in a manner that allow us to estimate the causal effect of the voucher offer—and the private school attendance it induces—on future political participation outcomes, specifically voter registration and turnout.

## The New York School Choice Scholarships Foundation Program and Accompanying Evaluation

The School Choice Scholarship Foundation was formed in January 1997 by several private philanthropists with the express goal of providing scholarships to students from low-income families in order to attend private schools. In the spring of 1997, the SCSF acted on this goal, offering three-year scholarships worth up to a maximum of $1,400 to as many as 1,000 low-income families with children who were enrolling in first grade or were enrolling in grades two through five and were currently attending a public school. Scholarship recipients could use the voucher to attend any New York City private school, either religious or secular. Chingos and Peterson (2015) report that the average tuition for Catholic schools—the city's single largest supplier of private schooling—in New York City in 1997 was about $1,700. Thus, the scholarship generally covered over 80 percent of the tuition at these schools, although tuition levels were undoubtedly higher at other private schools.

Demand for the scholarships far outstripped supply—over 20,000 students applied for fewer than 1,500 scholarships. This excess demand, coupled with the desire of the SCSF for a rigorous evaluation of the impact of the scholarship program on important student outcomes, led

to the scholarships being distributed via a lottery. Participation in the lottery required both students and parents to take specific actions. In particular, students in first grade and above were required to take a baseline standardized test—students in kindergarten at the time of the application were exempt from this requirement. Parents of applicant students—specifically the parent who brought the student to the testing session—were required to provide several pieces of information, including demographic characteristics, evidence of program eligibility, and identifying information for the student, such as name, sex, and date of birth.

Administration of the lottery was complex. Because the rules of the SCSF required that all eligible siblings of scholarship winners also receive a scholarship, families served as the unit of randomization. The initial plan for the lottery stated that families who applied to the program would be assigned to one of five sessions where students would undergo testing and parents would provide necessary information, as described above. This plan was followed for the first session, but was altered for the next four sessions because of the logistical difficulties inherent in collecting the testing and background information for each of the 20,000 applicants (Hill, Rubin, and Thomas 2000). Specifically, instead of assigning each of the remaining applicant families to an information collection session, only a sample of remaining applicant families was invited to attend one of the four remaining sessions.[2]

The rules governing administration of the scholarships specified a preference for students from low-achieving schools. In particular, the rules stated that 85 percent of the scholarships were to be awarded to students from schools with an average test score below the citywide

---

[2] The original evaluators increased the probability of receiving a scholarship for those cases selected to attend the second through fifth sessions, relative to the probability of receiving a scholarship for families who attended the first session. Such an adjustment was necessary to account for the reduced probability of being invited to an information collection session and thus keep the probability of receiving a scholarship equal across the five sessions (Hill, Rubin, and Thomas 2000).

median. However, only about 75 percent of the applicants came from such schools, which necessitated assigning these applicants a higher probability of winning the lottery. Our analyses account for the differential probabilities of winning the lottery by including the groups within which each family was randomized as fixed effects in the model—this approach is identical to that used by Chingos and Peterson (2015) in their analysis of the effect of private school vouchers on college enrollment and degree attainment.

In principle, the entire set of lottery winners and losers could have served as the evaluation sample. In practice, tracking all lottery participants over time was not possible, which led to the original evaluators selecting a subset of lottery participants for inclusion in the analytic sample. Nearly all lottery winners were included in the sample, but financial limitations did require some random subsampling to reach the target of 1,000 treatment families.[3] The manner in which the subset of lottery losers were selected for inclusion in the evaluation sample varied across the information collection sessions. In the first session, evaluators selected lottery losers for inclusion in the evaluation sample using matching techniques—the details of the matching approach are described in Hill, Rubin, and Neal (2000). In the second through fifth sessions, however, lottery losers were selected randomly after stratifying by the number of applicant children in the family (i.e. one child versus multiple children). The end result of this process was an evaluation sample that contained 2,000 families—1,000 families each in the treatment and control groups. Because about 35 percent of families in the evaluation sample had multiple children, there were a total of approximately 2,600 children in the evaluation sample.

The original evaluation focused primarily on the effect of the voucher offer on student achievement scores. Such a focus required tracking the children in the evaluation sample over

---

[3] The sampling was stratified by family size (single versus multiple children) and information collection session (first session versus second through fifth sessions).

time and testing them.  In an effort to maximize future participation, the evaluators informed

lottery winners that scholarship renewal was conditional upon their participation in the future

testing sessions. Lottery losers, as well as those families that won the lottery but elected to not

use the scholarship, were provided with financial compensation for the costs associated with

participation. Moreover, lottery losers were given additional chances to win a scholarship via

lottery—these subsequent lotteries were held prior selecting the evaluation sample so as to not

include scholarship winners in the control group.

Despite these efforts at sample retention, attrition rates ranged from 22 to 34 percent in

the three subsequent outcome years—1998, 1999, and 2000—analyzed in the original

evaluation.[4] Attrition is not an issue here because the information needed to link sample

members to political participation outcomes—information such as name, date of birth, and sex—

was collected for all sample members at the information collection sessions accompanying the

scholarship lottery. Noncompliance with treatment assignment, however, does have potential

implications for this study, although it does not threaten the internal validity of our estimates. We

address issues of noncompliance in a later section of the paper.

The original evaluation found the voucher offer to have no overall effect on reading or

math achievement in any of the three follow-up years, but a positive effect on the achievement of

black students (Howell et al. 2006; Mayer et al. 2002), although those estimates are somewhat

sensitive to alternative analytic decisions (Krueger and Zhu 2004). The voucher offer was

estimated to have a positive effect on parental satisfaction with their child's school, as well as on

---

[4] Mayer et al. (2002) report that 78 percent of the sample attended the first testing session in the Spring of 1998
while 66 percent and 67 percent of the sample attended the second and third sessions, which were held in the Spring
of 1999 and Spring of 2000, respectively. Although the scholarship was initially intended to be a maximum of three
years, the scholarship was renewed through the end of a student's 8th grade year if he or she remained continuously
enrolled in a private school.

parental reports of school quality (Howell et al. 2006; Mayer et al. 2002). More recent analysis, which took place when the youngest sample members were in their mid-twenties, found the voucher offer to have no overall effect on college enrollment or four-year degree attainment (Chingos and Peterson 2015). The analysis does return evidence, however, of positive effects for minority students and children of women born in the United States.

## Data and Analytic Approach

In this study, we draw on the data from the original SCSF evaluation, supplemented with voter registration and turnout histories of sample members. The data from the original SCSF evaluation contains demographic and background characteristics, as well as baseline test scores, recorded at the information collection sessions accompanying the scholarship lottery. Table 1 presents summary statistics on these demographic, background, and test score characteristics for the evaluation sample. It presents these statistics separately for the treatment and control groups and, for each characteristic, includes the *p*-value resulting from a difference-of-means test for the two groups. The summary statistics presented in Table 1 serve two major purposes. First, they demonstrate that the analytic sample is quite disadvantaged along several dimensions—on average, these students scored in the bottom quartile on the Iowa Test of Basic Skills in the baseline year, less than 20 percent of students had a parent with a bachelor's degree, nearly half lived in a household with income less than $12,000 (in 2014 dollars), and well over 30 percent lived in a household where the father was absent. Second, Table 1 demonstrates the validity of the experimental design. There is remarkable balance across the treatment and control groups on the set of observable characteristics presented in Table 1. To more formally test the balance, we combine the separate tests into a single test statistic by estimating a Seemingly Unrelated Regression and conducting a Chi-squared test of the hypothesis that the estimated coefficients

for the treatment indicator across all of the regressions are jointly equal to zero. The test was unable to reject the hypothesis (*p*-value=0.37). This provides confidence in the validity of the experimental design and thus the causal nature of our estimates.

[Insert Table 1 about here]

To obtain political participation measures of sample members, we commissioned Catalist—a leading political data firm—to match the individuals in the original evaluation sample to their database containing, among other information, records on all registered voters across the U.S. Catalist compiles this information from voter registration files they obtain from the secretaries of state in each of the 50 states, as well as the District of Columbia. Catalist continuously updates this database with new information on voter registration and turnout, and has done so for several years. Consequently, Catalist was able to provide us with turnout records for our sample members for the 2008, 2010, and 2012 general elections. The 2008 election was the first election for which a majority of the evaluation sample would be old enough to be eligible to vote—the 2010 election was the first election for which all sample members were at least 18 years old.[5] Catalist also provided us with records indicating whether sample members were ever registered to vote during this time period.

Catalist used a fuzzy matching procedure to link the participation records in their database to the members of the original evaluation sample on the basis of name, date of birth, and sex. Using this procedure, Catalist found an exact match on each of the three fields noted above—name, sex, and date of birth—for 1,205 cases, which translates to 45.7 percent of observations. An additional 912 cases (34.6 percent of observations) had no match on any of the

---

[5] Because of state laws in California and Illinois, Catalist was unable to provide us with the participation records of individuals who matched to data from these states. However, only three sample members matched to these states, rendering this issue largely moot.

fields and have thus never appeared in any state's voter registration file. Together, these two sets

of observations—those with a perfect match and those that clearly have no match—account for

approximately 80 percent of the evaluation sample. The match accuracy for the remaining 20

percent of observations is less clear, a fact attributable to fuzzy matching procedure. For these

cases there is typically a perfect match on some fields, but no perfect match on others. Although

these cases are unlikely to threaten the validity of our estimates—there is no reason to think

match accuracy is correlated with treatment assignment—we assess the sensitivity of our

estimates to various ways of handling these cases. As the first step in doing so we manually

classified each of these cases into one of five categories:

1. Individual matched on name and sex, but the Catalist database did not contain a birthdate against which to compare the birthdate from the original evaluation data (223 cases—8.5% of observations).
2. Case does not match on one field, but lack of match could be attributable to a common typographical error (48 cases—1.8% of observations).
3. Individuals match on fields except birthdates, but the two nonmatching birthdates are within five years of one another (96 cases—3.6% of observations).
4. Individuals do not match on sex—may or may not match of birthdate, but nonmatching birthdates are within five years of one another (54 cases—2.1% of observations).
5. Individuals do not match on birthdate and nonmatching birthdates are not within five years of one another—may or may not match on sex (100 cases—3.8% of observations).

In our primary analyses we include all observations and consider those that were imperfect

matches—the 20 percent of observations classified into one of the five groups above—as

nonmatches. That is, we code the participation outcomes for the imperfect matches as zero,

which we believe to be the most conservative treatment of these observations. However, our

results are remarkably robust to treating the observations in the five above categories in different

manners, such as assuming them all to be accurate matches, assuming different combinations of

the groups to be accurate matches or nonmatches, or excluding one or more of the five groups

from the analytic sample. Indeed, in addition to our primary results, we present results from two

additional analysis—one where we assume all fuzzy matches to be accurate and a second where the analytic sample includes the perfect matches as well as the imperfect matches from groups 1 and 2, which we believe are likely to be accurate matches. It is unsurprising that treating these observations in alternative ways analytically does not meaningfully alter the estimated treatment effects; as we noted above, there is no *a priori* reason to expect that match accuracy would be correlated with treatment assignment.

In order to appropriately account for the group from within which each family was randomized into the treatment or control group, we estimate the effect of the voucher offer on later-life political participation using weighted least squared (WLS) regression techniques. The weights we use in the model were constructed by the original evaluators and are intended to ensure that the results can be generalized to the full group of scholarship applicants. Chingos and Peterson (2015) note that the weights also account for the fact that many more families lost the lottery than won, even though the two groups are approximately equal in size in the evaluation sample. The first model we estimate can be written as:

$$Y_{ig} = \alpha + \gamma T_{ig} + \delta_g + \varepsilon_{ig} \tag{1}$$

where the political participation outcome, *Y*, for student *i* in randomization group *g* is modeled as a function of an overall intercept, $\alpha$, an indicator for being randomly assigned to the treatment group, *T*, a vector of randomization group fixed effects, $\delta_g$, and an error term, $\varepsilon_{ig}$.[6] We cluster standard errors by family, which were the unit of randomization. We note that all results are substantively similar if a probit model is estimated in place of the linear probability model presented in equation (1). We estimate this model separately for five participation outcomes: 1)

---

[6] The vector of randomization group dummies uniquely identify the combination of sample members' family size (single child or multiple children), information collection session (first through fifth), and school quality (below or above citywide median).

Ever being registered to vote, 2) Voting in the 2008 general election, 3) Voting in the 2010 general election, 4) Voting in the 2012 general election, 5) Voting in any of those three elections. In addition, we estimate the model for the full sample as well as several demographic subgroups.

In this model, $\gamma$ is the parameter of interest as it represents the estimated effect of the voucher offer on the later-life political participation outcome specified in the model. As we estimate the effect of the scholarship offer—as opposed to the effect of scholarship use—$\gamma$ represents the intention-to-treat (ITT) parameter, which we believe to be the parameter most relevant to policy decisions. That said, the effect of using the scholarship to attend a private school—the treatment-on-the-treated (TOT) parameter—is also of interest to both researchers and policymakers, and in a later section of this paper we estimate this parameter..

In an effort to increase the precision of the estimated effect of the voucher offer we estimate a second model that contains student background characteristics. The experimental design renders these characteristics orthogonal to treatment status—meaning the estimates of $\gamma$ should not meaningfully change—but to the extent that the characteristics are predictive of later-life participation outcomes they may decrease the standard error of the estimate of $\gamma$. This second model can be written as:

$$Y_{ig} = \alpha + \gamma T_{ig} + \delta_g + X_{ig}\beta + \varepsilon_{ig} \qquad (2)$$

where *X* represents a vector of student background characteristics and the remaining contents of the model were described above. We again cluster standard errors by family. We estimate two specifications of the model presented in equation (2). The first only includes baseline measures of students' reading and math scores in the vector of student background characteristics. The second specification includes baseline test scores as well as measures of family income,

race/ethnicity, parental education, whether the family primarily speaks English in the home, maternal nativity, maternal employment, and paternal presence in the home.[7]

## Results

Table 2 presents the results of estimating the three model specifications described in the previous section over the full sample—assuming imperfect matches to be nonmatches—for each of the five following participation outcomes:[8]

- Ever being registered to vote;
- Voting in the 2008 general election;
- Voting in the 2010 general election;
- Voting in the 2012 general election; and
- Voting in either the 2008, 2010, or 2012 general elections.

The top panel of the table presents results from the model containing no baseline student characteristics—it contains just the treatment indicator and fixed effects for randomization blocks—while the middle and bottom panels present results from the models containing baseline test scores and baseline test scores plus additional background characteristics, respectively. For context, the table also presents the control group mean against which the estimated marginal effect operates. Full model results are presented in Table A1 in the appendix.

[Insert Table 2 about here]

The results in Table 2 make clear that the voucher offer had no significant effect on any of the five participation outcomes we analyze. For each outcome, the estimated marginal effects

---

[7] Students who do not have a baseline test score are assigned a score of zero and a dummy variable indicating these students is included in the model. Family income and parental education are included as a series of dummy variables, with one of the dummies indicating students were missing data on that measure. All control variables are interacted with the dummy for missing baseline test scores in order to recognize potential heterogeneity in the predictive power of background characteristics for students with and without baseline test scores.

[8] In referring to the full sample we mean we are not restricting the sample by subgroup. The analysis includes all observations and assumes imperfect matches to be nonmatches—their participation outcomes are coded as zero. As noted above, however, our results are robust to treating these cases in different ways.

are substantively small—all estimates are less than 2 percentage points—and statistically insignificant. As expected, given the experimental design, the estimated effects are consistent across the three model specifications. More surprisingly, however, the standard errors also exhibit little change across specifications—including student background characteristics in the model does not meaningfully increase the precision of the estimated effect of the voucher offer. Considered together, the results in Table 2 provide evidence that the voucher offer did not significantly affect students' later-life participation, at least through the 2012 general election.

We note, however, that finding no significant effect is not equivalent to finding that the effect of voucher receipt on later-life political participation is zero. For example, the uncertainty associated with our point estimates on the measure of voting in any of the three elections we analyze—2008, 2010, or 2012—results in the 95% confidence interval containing values as large as 3.5 percentage points and as small as -3.5 percentage points. Similarly, for the measure of voting in the 2012 election we cannot reject a positive effect as large as 5 percentage points.

The descriptive statistics in Table 2—the control group means—illustrate that the sample exhibits very low levels of political participation. Less than half of control group members were ever registered to vote through 2012 and less than a fifth voted in the 2008 general election—turnout levels for the 2012 general election were only slightly higher. For purposes of comparison, data from the United States Election Project indicate that national turnout levels for the 2008 and 2012 general elections were approximately 60 percent—2008 turnout levels were slightly above 60 percent and 2012 turnout was slightly below. Finally, Table 2 demonstrates that the turnout rate for control group members in the 2010 midterm election was less than 8 percent—national turnout for the midterm was about 40 percent—and less than one-third of the control group voted in the 2008, 2010, or 2012 elections.

Although Table 2 makes clear that there were no significant overall effects of the voucher offer on any of the five participation outcomes, it is possible that voucher offer may have heterogeneous effects across student subgroups. To examine this possibility we estimate separately the model containing the full suite of student background characteristics separately for each of several student demographic subgroups, specifically those characterized by parental education level, maternal nativity, maternal race/ethnicity, the primary language spoken at home, and student gender.[9] We also estimate the model separately for students who were entering grades K-2 and those entering grades 3-5 at the time of the lottery. This analysis is motivated by the prospect of voting being more proximate for older students than for younger ones, which could lead to heterogeneity in the effects of the voucher offer.

[Insert Table 3 about here]

The results of the subgroup analyses are presented in Table 3. Specifically, the table presents the estimated effect of the voucher offer, as well as the accompanying standard error, across the five participation outcomes we analyze for each of the subgroups detailed above. The results in Table 3 largely mirror the full sample results presented in Table 2, providing little evidence of the voucher offer having any significant effect on political participation outcomes. Across the 60 separate subgroup-outcome combinations presented in the table, only in one case is the estimated effect of the voucher offer statistically different from zero at $p<0.10$ and for no subgroup is the estimated effect significant at $p<0.05$. Table A2 in the appendix presents the sample size for each subgroup along with the control group mean from which each estimated marginal effect operates.

---

[9] In addition to the African American and Hispanic racial/ethnic subgroups included in Table 3 we also estimated effects for an "Other race" category and no estimates were significant. We do not include the "Other race category" in Table 3 for both reasons of space and very small sample sizes. We also examined the possibility of heterogeneity by baseline achievement scores and—consistent with all other subgroups—found no significant effects.

As described earlier, our primary results are based on an analytic sample in which we assume all of the imperfect matches to be nonmatches—the participation outcomes for these observations are coded as zero. To demonstrate that our results are not sensitive to our treatment of observations with questionable match accuracy we conduct two additional analyses. The first of these analyses assumes that all of the potential matches provided by Catalist were accurate, regardless of how implausible the match may seem. The second analysis includes the perfect matches as well as the imperfect matches in categories 1 and 2—those for which there is no second birthdate that can be used as a basis for matching and those for which a common typo could explain the mismatch—in the analytic sample. The nonmatches that we classified into categories 3-6 are excluded from the sample. Using these two alternative analytic samples, we estimate the specification of equation (2) containing the full set of student background characteristics. The results from estimating these models are presented Table 4 and, consistent with our primary results, demonstrate that the voucher offer has no significant effect on later-life political participation. This is true for each of the two additional analyses across each of the five outcomes we analyze. The insensitivity of our estimates to different treatment of observations with questionable match accuracy is unsurprising because, as we noted earlier, there is no *a priori* reason to think that match accuracy would be correlated with treatment status.

[Insert Table 4 about here]

Together, the results in Tables 2-4 provide unambiguous evidence that being offered a scholarship to attend a private school had no significant effect on multiple measures of students' later-life political participation, particularly registering to vote and voting.

## Private School Attendance and Treatment-on-the-Treated Estimates

The results in the previous section represent the estimated effect of the voucher offer on later-life registration and voting—they are ITT estimates. These estimates have clear policy implications as they provide evidence on the effects of making vouchers available. They provide less information, however, on the effects of using the voucher to attend private school—the TOT parameter—which is also of clear interest to the policymaking and research communities.

Estimating the TOT parameter requires information on compliance with treatment assignment for both the treatment and control groups. In our context, it requires information on private school attendance for each group. Data from the original SCSF evaluation include private school attendance status for each member of the treatment group separately for each of the first three years following the voucher offer. The data do not contain any record of private school attendance in subsequent years, however. For the control group, data from the original evaluation only record annual private school attendance status for students who attended the follow-up session for a given year. Table 5 provides information on private school attendance for both the treatment group and the portion of the control group with non-missing records. It also provides information on the percentage of the control group with private school attendance records.

[Insert Table 5 about here]

Table 5 illustrates that about 75 percent of the treatment group attended private school in the first year following the voucher offer. That number declined to 65 percent and 55 percent, however, in the second and third years following the voucher offer, respectively. Over the three years following the voucher offer, about 77 percent of treatment cases attended a private school at some point, and just over half of treatment cases attended private school over the full period.

Data for the control group reveal that about 80 percent of students have non-missing private school attendance records in the first year following the voucher lottery. This number fell

22

to about 72 percent in the second and third years following the lottery. About 90 percent of control group students have non-missing private school attendance records in at least one of the first three post-lottery years, but only about 58 percent have non-missing records in each of those years. Among those control group students with non-missing records in the first post-lottery year, about 7 percent attended a private school. The analogous numbers for the second and third years are 8 percent and 11 percent, respectively. Data indicate that about 12 percent of control group participants attended a private school at some point over the three years following the voucher lottery, but only about 4 percent of students attended private school in each of those years.

We use these records on private school attendance to estimate the effects of voucher use—the TOT parameter—on voter registration and voting. We estimate this parameter using the instrumental variables (IV) approach proposed by Angrist, Imbens, and Rubin (1996) that has been in several prior lottery-based studies of school choice (e.g. Abdulkadiroglu et al. 2011; Cullen, Jacob, and Levitt 2006; Deming et al. 2014; Rouse 1998).[10] In this approach, we use the randomized voucher offer as an instrument for private school attendance. We implement this IV approach in a two-stage least squares (2SLS) framework where the first stage predicts private school attendance using the following model:

$$P_{ig} = \alpha + \gamma T_{ig} + \delta_g + X_{ig}\beta + \omega_{ig} \tag{3}$$

where *P* is an indicator of private school attendance for student *i* in randomization group *g*, *T* is an indicator for the randomized voucher offer, $\delta_g$ is a vector of randomization group fixed effects, *X* is the full suite of student background characteristics described earlier, and $\omega$ is an

---

[10] Technically, the IV approach we employ estimates the Local Average Treatment Effect (LATE), which is equivalent to the TOT if we assume that the average treatment effect for students who would attend private school regardless of the outcome of the voucher lottery is the same as the average treatment effect for those students whose private school attendance status is determined by the lottery outcome (Gennetian et al. 2005.

error term. Standard errors are clustered by family. The predicted values of $P$ resulting from estimation of equation (3)—denoted as $\hat{P}$ below—are then inserted into the second-stage equation, taking the place of the indicator for winning the voucher lottery in equation (2) above. The second-stage model can be written as:

$$Y_{ig} = \alpha + \lambda \widehat{P_{ig}} + \delta_g + X_{ig}\beta + \varepsilon_{ig} \tag{4}$$

In this analysis we define private school attendance as enrollment in private school in all three years following the voucher lottery, although our results are robust to other definitions. We demonstrate that our substantive conclusions are insensitive to several different assumptions about the nature of missing private school attendance records for control group students.

[Insert Table 6 about here]

Table 6 presents results from the 2SLS approach we use to estimate the effect of private school attendance on voter registration and voting. In each panel, the top row presents the estimated effect of the voucher offer on private school attendance, which is represented by $\gamma$ in equation (3) above. The bottom row in each panel presents the estimated effect of private school attendance on each of the five participation outcomes—represented by $\lambda$ in equation (4) above.

As described above, annual private school attendance records are missing for control group students who did not attend the follow-up session in that year. These missing records present a potential issue for determining treatment compliance among control groups students. However, over 90 percent of control group students attended at least one of the follow-up sessions and, as Table 5 makes clear, the vast majority of students did not attend private school. Consequently, there are only 159 of the 1,279 control group students whose private school attendance status across the full period cannot be definitively classified—these students either did not attend any follow-up sessions or attended one or two follow-up sessions and were

24

recorded as attending private school in every session they attended. Table 6 presents the estimated effects of private school attendance under four different assumptions about the private school attendance of these 159 students. The top panel of the table presents results under the assumption that none of these students attended private school in each of the three years. The second panel assumes that students who were recorded as attending private schools in two of the three years, but are missing private school attendance records for the third year, attended private school in the third year as well. The third panel assumes that all of the 159 students attended private school throughout the full period. Finally, the fourth panel presents results when these cases are dropped from the analysis. Chingos and Peterson (2015) provide evidence of selective attrition, which indicates that dropping these cases is likely to be problematic. However, we include these results to allow readers to assess the consistency of estimates under various assumptions and analytic decisions.

The results in each panel of Table 6 demonstrate that private school attendance has no significant effect on any of the five registration or voting outcomes we analyze. The estimates are relatively consistent across the four approaches to handling the missing control group cases. Together, the results in Table 6 provide strong evidence that, at least in this context, private school attendance had no detectable effect on the later-life voting outcomes we examine.[11] The statistical insignificance of the estimates in Table 6 is perhaps unsurprising given the

---

[11] Table A3 in the appendix presents the estimated effect of private school attendance when it is defined as attending private school in any of the three follow-up years. This definition results in many more control group students missing the private school attendance measure, as less than 60 percent of control group students attended all three follow-up sessions. Table A3 presents results under five different approaches to handling these cases: 1) Missing control group cases never attended private school; 2) Control group cases missing two follow-up sessions attended private school at some point; 3) Control group cases missing one follow-up session attended private school in that year; 4) All missing control group cases attended private school; 5) Drop control group cases that missed any follow-up session. Table A3 illustrates that private school attendance had no significant effect on any of the five later-life registration or voting outcomes we analyze, providing further evidence of the null effects of the voucher intervention on political participation.

insignificance of the ITT results in the prior section (Gennetian et al. 2005). Indeed, although the TOT point estimates are larger in magnitude than the analogous ITT estimates, the standard errors of the TOT estimates are larger than the standard errors of the ITT estimates.

## Discussion and Conclusion

Owing largely to differences in the democratic nature—and corresponding accountability—of public and private schools, there are longstanding questions about the relative effectiveness of schools in these respective sectors in preparing democratic citizens. In this paper, we leverage exogenous variation generated by the New York School Choice Scholarships Foundation Program (SCSF), which in 1997 randomly offered three-year scholarships to attend private schools to as many as 1,000 low-income families in New York City, to estimate the causal effect of the voucher offer on later-life political participation outcomes. Our results demonstrate that both the voucher offer and private school attendance had no significant effect on any of five separate participation outcomes we analyze. We show that these conclusions are robust across subgroups and to a variety of alternative analytical decisions, including those regarding model specification, the treatment of cases with questionable match accuracy, and assumptions about private school attendance for control group cases with missing records. These results have several implications for both research and policy.

At a basic level, our results provide what we believe to be the first experimental evidence on the effect of private school vouchers on later-life political participation. The fact that the voucher intervention had, on average, no effect on voter registration or voting in multiple elections suggests that—at least for the context and outcomes we analyze—public and private schools were equally (in)effective in preparing citizens to participate in democratic society. We again stress, however, that finding no significant effect is not equivalent to determining that the

effect of the voucher intervention on political participation is zero—the uncertainty associated with our point estimates leaves us unable to reject effects of four or even five percentage points, either positive or negative. Potential effects of this magnitude may not seem especially large from an absolute standpoint, but they are more meaningful relative to the low participation rates of the control group. Indeed, an effect as small as three percentage points would represent an increase in voter participation in excess of ten percent. Although increased statistical power would clearly be desirable, we believe the analysis has sufficient power to provide useful evidence on the effects of both the voucher offer and private school attendance on voter registration and voting outcomes.

In suggesting that the effects of public and private schools on students' later-life participation were not detectably different, our results also indicate that the theoretical arguments contending that schools in one sector will more effectively prepare democratic citizens than schools in the other are not borne out empirically. Or, at least, the theoretical mechanisms through which each sector may exert positive, or negative, effects on participation operate in a manner such that they balance each other out, resulting in no net effect on political participation. So while there are myriad reasons to support or oppose either private school vouchers or traditional public education, the argument that one sector will generate citizens better prepared to participate in our democracy is not corroborated by our data.

Of course, all of our results, and corresponding implications, are subject to multiple caveats. We highlight three here. First, we identify the effect of the voucher offer for students who were in elementary and middle school grades at the time they received the offer, and it is possible that a voucher offer could have a very different effect—either positive or negative—for students in high school grades. Indeed, much of the existing work analyzing the relationship

between education and civic outcomes focuses on students' high school years (e.g. Niemi and

Junn 1998; Conover and Searing 2000; Gimpel, Lay, and Schuknecht 2003; Campbell 2006;

2007; 2008; Carlson 2012). It is unclear whether this focus on students' secondary education has

a firm theoretical basis, or whether it is primarily a product of data availability. Regardless of the

reasons ultimately underlying this focus, our data contain no information on the high schools that

students attended, which prevents us from exploring this issue empirically. Future work in both

the theoretical and empirical realms would do well to consider how different factors at each level

of the education system—elementary, secondary, and even postsecondary—might shape

students' future engagement with the political process. Moreover, such work could analyze how

different policies or contexts might operate differently at each level of the education system with

respect to development of students' civic skills. As an example, future work could consider

whether the effects of vouchers—or private schools more generally—might differ across the

elementary, middle, and high school levels.

Second, the context and design of the SCSF program and original evaluation impose

limits on the scope of the results. Our analysis clearly demonstrates that the randomized offer of

a partial voucher to attend private school through 8th grade induced substantial differences in

private school attendance between the treatment and control group over the three years following

the voucher lottery. It is further clear that neither the voucher offer nor private school attendance

induced by the voucher offer resulted in significant effects on any of five voter registration or

voter outcomes. However, we have no information on private school attendance for either the

treatment or control group beyond those initial three years, including for the potentially

important high school years, which were not covered by the partial voucher. As such, the nature

of the intervention—particularly the partial nature of the voucher and the time constraints on its'

use—is not perfectly aligned with most current private school voucher proposals, which generally provide more consistent access to private schooling. Even recognizing the limited scope of our results, though, they still provide evidence relevant to debates over the civic effects of private school vouchers and attendance.

Third, we note that we estimated the effects of the voucher offer on political participation using data from a sample of individuals that were quite disadvantaged along several dimensions, including income, parental education levels, and perhaps most notably, student achievement levels. It is possible that the effect of a voucher offer—and the related private school attendance—might have been different for a sample with different characteristics. In particular, with a low-achieving sample like the one we analyze, it is easy to imagine a situation where schools—either public or private—are so focused on improving the academic outcomes of their students that they devote relatively few resources to cultivating civic skills. Put differently, it may be the case that schools'—again, either public or private—ability to develop students' citizenship skills may more clearly manifest itself in a more advantaged student population, where the school does not have to place such a heavy focus on developing basic academic skills.

Perhaps inseparable from the disadvantaged nature of our sample are the strikingly low participation rates. Only about half of sample members have ever registered to vote and less than a quarter would turn out to vote in a given presidential election—for the 2010 midterm election the turnout rate of sample members was below ten percent. These participation rates are very low, even for individuals in their 20s and early 30s who generally have notoriously low participation rates. This lack of engagement with the political process suggest that—independent of whether private school vouchers or any other educational intervention—schools and other educational institutions, as well as policymakers, may wish to place a greater focus on generating

a civic awareness and cultivating civic skills. Indeed, rhetoric surrounding education is rife with references to the achievement gaps that exist between different groups. When considered in tandem with information on political participation rates of the broader population, our data suggest the presence of a participation gap, both between individuals with different levels of socioeconomic advantage as well as between younger and older individuals. It is beyond the scope of this paper to consider the potential consequences of any participation gap, but our nation's education system could serve as an institution in which such gaps are recognized and possibly even addressed via policy or practice.

# References

Abdulkadiroğlu, A., Angrist, J. D., Dynarski, S. M., Kane, T. J., & Pathak, P. A. (2011). Accountability and flexibility in public schools: Evidence from Boston's charters and pilots. *The Quarterly Journal of Economics*, *126*(2), 699-748.

Abdulkadiroglu, A., Pathak, P. A., & Walters, C. R. (2015). *School Vouchers and Student Achievement: First-Year Evidence from the Louisiana Scholarship Program* (No. w21839). National Bureau of Economic Research.

Barnard, J., Frangakis, C. E., Hill, J. L., & Rubin, D. B. (2003). Principal Stratification Approach to Broken Randomized Experiments: A Case Study of School Choice Vouchers in New York City. *Journal of the American Statistical Association* 98 (462), 299-311.

Bettinger, E., & Slonim, R. (2006). Using experimental economics to measure the effects of a natural educational experiment on altruism. *Journal of Public Economics*, *90*(8), 1625-1648.

Blais, A. 2000. *To Vote or Not to Vote? The Merits and Limits of Rational Choice Theory.* Pittsburgh, PA: University of Pittsburgh Press.

Bryk, A.S., Lee, V.E., & Holland, P.B. 1993. *Catholic Schools and the Common Good.* Cambridge, MA: Harvard University Press.

Campbell, D.E. (2001). Making Democratic Education Work. In *Charters, Vouchers, and Public Education*, eds. Paul E. Peterson and David E. Campbell, pp. 241-267. Washington, DC: Brookings Institution Press.

Campbell, D.E. (2006). *Why We Vote: How Schools and Communities Shape our Civic Life.* Princeton, NJ: Princeton University Press.

Campbell, D.E. (2007). Sticking Together: Classroom Diversity and Civic Education. *American Politics Research* 35 (1):57.

Campbell, D.E. (2008). Voice in the Classroom: How an Open Classroom Climate Fosters Political Engagement Among Adolescents. *Political Behavior* 30 (4):437-454.

Campbell, A., Converse, P.E., Miller, W.E., & Stokes, D.E. (1960). *The American Voter.* New York: Wiley.

Campbell, A., Gurin, G., & Miller, W.E. (1971). *The Voter Decides.* Westport, CT: Greenwood Press.

Carlson, D.E. (2012). Out of the Classroom and into the Voting Booth? Analyzing the Effects of Education on Political Participation. Unpublished doctoral dissertation. Madison, WI: University of Wisconsin-Madison.

Chingos, M.M. & Peterson, P.E. (2015). Experimentally Estimated Impacts of School Vouchers on College Enrollment and Degree Attainment. *Journal of Public Economics* 122 (1): 1-12.

Chubb, J.E. & Moe, T.M. (1990). *Politics, Markets, and America's Schools*. Washington, DC: Brookings Institution Press.

Coleman, J.S. & Hoffer, T.B. 1987. *Public and Private High Schools: The Impact of Communities*. New York, NY: Basic Books.

Conover, P.J. & Searing, D.D. (2000). A Political Socialization Perspective. In *Rediscovering the Democratic Purposes of Education*, edited by L. M. McDonnell, P. M. Timpane and R. Benjamin. Lawrence, KS: University Press Of Kansas.

Converse, P.E. (1972). Change in the American Electorate. In *The Human Meaning of Social Change*, edited by A. Campbell and P. E. Converse. New York: Russell Sage Foundation.

Cowen, J.M. (2012). Interpreting School Choice Effects: Do Voucher Experiments Estimate the Impact of Private Schooling? *Journal of Research on Educational Effectiveness* 5(4): 384-400.

Cowen, J.M., Fleming, D.J., Witte, J.F., Wolf, P.J. & Kisida, B. (2013). School Vouchers and Student Attainment: Evidence from a State-Mandated Study of Milwaukee's Parental Choice Program. *Policy Studies Journal* 41(1): 147-168.

Cullen, J. B., Jacob, B. A., & Levitt, S. (2006). The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica*, *74*(5), 1191-1230.

Dee, T.S. (2004). Are There Civic Returns to Education? *Journal of Public Economics* 88 (9-10):1697-1720.

Dee, T.S. (2005). The Effects of Catholic Schooling on Civic Participation. *International Tax and Public Finance* 12(5): 605-625.

Deming, D. J., Hastings, J. S., Kane, T. J., & Staiger, D. O. (2014). School choice, school quality, and postsecondary attainment. *American Economic Review*, *104*(3), 991-1013.

Dewey, J. (1916). *Democracy and Education: An Introduction to the Philosophy of Education*. New York: MacMillan Company.

Fleming, D. J. (2014). Learning from schools: School choice, political learning, and policy feedback. *Policy Studies Journal*, *42*(1), 55-78.

Fleming, D. J., Mitchell, W., & McNally, M. (2014). Can Markets Make Citizens? School Vouchers, Political Tolerance, and Civic Engagement. *Journal of School Choice*, *8*(2), 213-236.

Gennetian, L. A., Morris, P. A., Bos, J. M., & Bloom, H. S. (2005). Constructing Instrumental Variables from Experimental Data to Explore How Treatments Produce Effects. In *Learning More From Social Experiments: Evolving Analytic Approaches*, edited by Howard S. Bloom. New York: Russell Sage Foundation.

Gimpel, J.G., Lay, J.C., & Schuknecht, J.E. (2003). *Cultivating Democracy : Civic Environments and Political Socialization in America*. Washington, D.C.: Brookings Institution Press.

Greene, J.P. (1998). Civic Values in Public and Private Schools. In *Learning from School Choice*, edited by Paul E. Peterson and Bryan C. Hassel. Washington, DC: Brookings, pp. 83-106.

Greene, J.P., Peterson, P.E., & Du, J. (1998). Effectiveness of School Choice: The Milwaukee Voucher Experiment. *Education and Urban Society* 31(2): 190-213.

Gutman, A. (1987). *Democratic Education*. Princeton, NJ: Princeton University Press.

Hill, J.L., Rubin, D.B., & Thomas, N. (2000). The Design of the New York School Choice Scholarships Program Evaluation. In *Research Design: Donald Campbell's Legacy, Volume 2*, edited by Leonard Bickman. Thousand Oaks, CA: Sage Publications

Howell, W.G., Peterson, P.E., Wolf, P.J., & Campbell, D.E. (2006). *The Education Gap: Vouchers and Urban Schools* (2nd ed.). Washington, DC: Brookings Institution Press.

Key, V. O. (1961). *Public Opinion and American Democracy*. New York: Knopf.

Krueger, A.B., & Zhu, P. (2004). Another Look at the New York City School Voucher Experiment. *American Behavioral Scientist* 47(5): 658-698.

Levin, H.M. (2002). A Comprehensive Framework for Evaluating Educational Vouchers. *Educational Evaluation and Policy Analysis* 24(3): 159-174.

Mayer, D.P., Peterson, P.E., Meyers, D.E., Tuttle, C.C., & Howell, W.G. (2002). School Choice in New York City after Three Years: An Evaluation of the School Choice Scholarships Program: Final Report. Washington, DC: Mathematica Policy Research, Inc.

Metcalf, K. K., West, S. D., Legan, N. A., Paul, K. M., & Boone, W. J. (2003). *Evaluation of the Cleveland Scholarship and Tutoring Program: Summary Report 1998-2002.* Bloomington, IN: Indiana University.

Milligan, K., Moretti, E. & Oreopoulos, P. (2004). Does Education Improve Citizenship? Evidence From the United States and the United Kingdom. *Journal of Public Economics* 88 (9-10):1667-1695.

Mills, J.N., & Wolf, P.J. (2016). *The Effects of the Louisiana Scholarship Program on Student Achievement After Two Year*s. New Orleans, LA: Education Research Alliance for New Orleans.

Mills, J.N., Cheng, A., Hitt, C.E., Wolf, P.J., Greene, J.P. (2016). *Measures of Student Non-Cognitive Skills and Political Tolerance After Two Years of the Louisiana Scholarhip Program.* New Orleans, LA: Education Research Alliance for New Orleans.

Moe, T.M. 2000. Two Democratic Purposes of Education. In *Rediscovering the Democratic Purposes of Education*, ed. McDonnell, L.M., Timpane, P.M. & Benjamin, R., pp. 127-47. Lawrence, KS: University Press of Kansas.

Niemi, R.G., & Junn, J. (1998). *Civic Education: What Makes Students Learn*. New Haven, CT: Yale University Press.

Peterson, P. E., Howell, W. G., & Greene, J. P. (1999). *An Evaluation of the Cleveland Voucher Program after Two Years.* Harvard University Program on Education Policy and Governance.

Riker, W. & Ordeshook, P.C. 1968. A Theory of the Calculus of Voting. *American Political Science Review* 22 (1): 96-111.

Rosenstone, S.J., & Hansen, J.M. (1993). *Mobilization, Participation, and Democracy in America*. New York, NY: Macmillan

Rouse, C.E. (1998). Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program. *Quarterly Journal of Economics* 113(2), 553-602.

Sondheimer, R.M., & Green, D.P. (2010). Using Experiments to Estimate the Effects of Education on Voter Turnout. *American Journal of Political Science* 54 (1):174-189.

Verba, S., & Nie, N.H. (1972). *Participation in America: Political Democracy and Social Equality*. New York: Harper & Row.

Verba, S., Schlozman, K.L., & Brady, H.E. (1995). *Voice and Equality: Civic Voluntarism in American Politics*. Cambridge, MA: Harvard University Press.

Witte, J.F. (2000). *The Market Approach to Education*. Princeton, NJ: Princeton University Press.

Wolf, P.J., Kisida, B., Gutmann, B., Puma, M., Eissa, N., & Rizzo, L. (2013). School Vouchers and Student Outcomes: Experimental Evidence from Washington, DC. *Journal of Policy Analysis and Management* 32(2): 246-270.

Wolfinger, R.E., & Rosenstone, S.J. (1980). *Who Votes?* New Haven, CT: Yale University Press.

# Tables and Figures

**Table 1. Summary Statistics**

| Characteristic | Treatment | Control | *p*-value |
|---|---|---|---|
| **Student gender** | | | |
| Female | 48.6% | 50.5% | 0.381 |
| Gender Missing | 3.2% | 2.0% | 0.097 |
| **Baseline test score** | | | |
| Math score (percentile) | 17.1 | 17.1 | 0.885 |
| Reading score (percentile) | 22.9 | 24.6 | 0.242 |
| No baseline test scores | 29.1% | 30.4% | 0.504 |
| **Parents' education** | | | |
| Some high school | 16.0% | 15.9% | 0.931 |
| High school grad or GED | 23.8% | 27.7% | 0.075 |
| Some college | 41.1% | 40.2% | 0.722 |
| BA degree or higher | 14.9% | 13.1% | 0.306 |
| Missing | 4.2% | 3.1% | 0.242 |
| **Family income (2014 dollars)** | | | |
| Less than $7,425 | 25.1% | 27.0% | 0.385 |
| $7,425-$11,879 | 22.0% | 20.3% | 0.417 |
| $11,880-$16,334 | 11.9% | 13.7% | 0.282 |
| $16,335-$22,274 | 12.9% | 12.6% | 0.817 |
| $22,275-$29,699 | 10.2% | 10.0% | 0.918 |
| $29,700 or more | 9.7% | 8.5% | 0.476 |
| Missing | 8.2% | 7.9% | 0.797 |
| **Mother race/ethnicity** | | | |
| African American | 42.1% | 41.1% | 0.679 |
| Hispanic | 41.9% | 47.4% | 0.025 |
| White/Asian | 6.0% | 5.1% | 0.442 |
| Other or missing | 10.1% | 6.5% | 0.011 |
| **Other parental characteristics** | | | |
| Mother born in U.S. | 57.8% | 61.1% | 0.178 |
| Mother birthplace missing | 1.4% | 2.7% | 0.063 |
| Mother is employed | 34.8% | 34.3% | 0.799 |
| Father absent from home | 36.0% | 35.1% | 0.674 |
| English primary language spoken in home | 71.5% | 71.2% | 0.926 |
| **Unweighted *N*** | 1,359 | 1,279 | NA |

**NOTE:** Statistics calculated using baseline weights created by original evaluation team. The *p*-values in the table are for group differences after adjusting for randomization group.

**Table 2. Coefficient estimate and standard error for effect of voucher offer, as well as control group mean, by model specification**

| | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| | | | *No Background Characteristics* | | |
| Effect of voucher offer | -0.011 | 0.001 | -0.013 | 0.014 | 0.003 |
| | (0.022) | (0.017) | (0.011) | (0.019) | (0.020) |
| Control group mean | 0.457 | 0.185 | 0.062 | 0.213 | 0.298 |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| | | | *Baseline test scores* | | |
| Effect of voucher offer | -0.009 | 0.001 | -0.012 | 0.015 | 0.004 |
| | (0.022) | (0.017) | (0.011) | (0.019) | (0.020) |
| Control group mean | 0.457 | 0.185 | 0.062 | 0.213 | 0.298 |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| | | | *Baseline test scores & demographic characteristics* | | |
| Effect of voucher offer | -0.006 | -0.001 | -0.013 | 0.018 | 0.003 |
| | (0.022) | (0.017) | (0.011) | (0.018) | (0.020) |
| Control group mean | 0.457 | 0.185 | 0.062 | 0.213 | 0.298 |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |

**NOTE:** Robust standard error clustered by family in parentheses below coefficient estimate for effect of voucher offer. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

**Table 3. Coefficient estimate and standard error for effect of voucher offer, as well as control group mean, by subgroup**

| | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| **Parents' education** | | | | | |
| HS Grad or less | 0.002 | -0.018 | -0.012 | -0.016 | -0.021 |
| | (0.033) | (0.025) | (0.016) | (0.028) | (0.031) |
| Some college or more | -0.020 | 0.005 | -0.014 | 0.044* | 0.020 |
| | (0.031) | (0.025) | (0.015) | (0.026) | (0.030) |
| **Mother's birthplace** | | | | | |
| U.S. | 0.002 | 0.010 | -0.007 | 0.023 | 0.014 |
| | (0.028) | (0.022) | (0.013) | (0.023) | (0.026) |
| Outside U.S. | -0.029 | -0.012 | -0.021 | 0.001 | -0.018 |
| | (0.035) | (0.027) | (0.018) | (0.029) | (0.033) |
| **Race/Ethnicity of Mother** | | | | | |
| African American | 0.013 | -0.019 | -0.011 | 0.018 | 0.007 |
| | (0.034) | (0.028) | (0.018) | (0.030) | (0.032) |
| Hispanic | -0.030 | -0.001 | -0.010 | 0.004 | -0.006 |
| | (0.032) | (0.023) | (0.014) | (0.027) | (0.029) |
| **Primary language** | | | | | |
| English | -0.008 | -0.003 | -0.012 | 0.012 | -0.003 |
| | (0.026) | (0.021) | (0.014) | (0.023) | (0.025) |
| Not English | 0.004 | 0.004 | -0.009 | 0.033 | 0.015 |
| | (0.041) | (0.030) | (0.015) | (0.033) | (0.036) |
| **Gender** | | | | | |
| Female | -0.032 | -0.004 | -0.019 | 0.001 | -0.012 |
| | (0.032) | (0.025) | (0.016) | (0.029) | (0.031) |
| Male | 0.022 | -0.011 | -0.013 | 0.027 | 0.007 |
| | (0.030) | (0.022) | (0.015) | (0.023) | (0.026) |
| **Grade** | | | | | |
| Grade 2 or lower | 0.023 | 0.016 | -0.017 | 0.014 | 0.015 |
| | (0.028) | (0.019) | (0.013) | (0.023) | (0.025) |
| Grade 3 or higher | -0.048 | -0.034 | -0.006 | 0.033 | -0.014 |
| | (0.034) | (0.030) | (0.016) | (0.027) | (0.032) |

**NOTE:** Robust standard error clustered by family in parentheses below coefficient estimate for effect of voucher offer. Models used to estimate effects contain full set of student background characteristics. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

**Table 4. Coefficient estimate and standard error for effect of voucher offer, as well as control group mean, by assumptions about match accuracy**

| | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| *Assuming All Accurate Matches* | | | | | |
| Effect of voucher offer | -0.003 | -0.005 | -0.009 | 0.019 | 0.004 |
| | (0.022) | (0.018) | (0.012) | (0.019) | (0.021) |
| Control group mean | 0.561 | 0.229 | 0.074 | 0.256 | 0.358 |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Sample Includes Perfect Matches and Imperfect Matches in Groups 1 and 2* | | | | | |
| Effect of voucher offer | -0.005 | -0.002 | -0.017 | 0.023 | 0.003 |
| | (0.023) | (0.019) | (0.012) | (0.020) | (0.022) |
| Control group mean | 0.525 | 0.215 | 0.074 | 0.246 | 0.343 |
| *N* | 2,388 | 2,388 | 2,388 | 2,388 | 2,388 |

**NOTE:** Robust standard error clustered by family in parentheses below coefficient estimate for effect of voucher offer. * p<0.10; ** p<0.05; *** p<0.01.

**Table 5. Private School Attendance for Treatment and Control Groups, and Percent of Control Group with Non-Missing Private School Attendance Records**

|  | All Students |
|---|---|
| *Percent of Treatment Group Attending Private School* | |
| Year 1-1997-98 | 74.5 |
| Year 2-1998-99 | 64.8 |
| Year 3-1999-00 | 55.2 |
| Any Year | 77.3 |
| All Three Years | 52.8 |
| | |
| *Percent of Control Group Attending Private School-Among Those With Non-Missing Records* | |
| Year 1-1997-98 | 6.5 |
| Year 2-1998-99 | 8.1 |
| Year 3-1999-00 | 10.6 |
| Any Year | 12.3 |
| All Three Years | 2.4 |
| | |
| *Percent of Control Group With Non-Missing Private School Attendance Records* | |
| Year 1-1997-98 | 79.8 |
| Year 2-1998-99 | 71.6 |
| Year 3-1999-00 | 72.2 |
| Any Year | 90.4 |
| All Three Years | 57.9 |

**Table 6. Coefficient estimate and standard error for effect of private school attendance, by model specification**

| | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| *Assume No Missing Control Group Cases Attended Private School* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.502*** (0.018) | 0.502*** (0.018) | 0.502*** (0.018) | 0.502*** (0.018) | 0.502*** (0.018) |
| 2nd stage: Effect of private school attendance on participation | -0.012 (0.043) | -0.002 (0.034) | -0.026 (0.021) | 0.035 (0.036) | 0.006 (0.040) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Assume Control Group Cases With Two Years of Private School Attendance Attended in Third Year* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.490*** (0.018) | 0.490*** (0.018) | 0.490*** (0.018) | 0.490*** (0.018) | 0.490*** (0.018) |
| 2nd stage: Effect of private school attendance on participation | -0.012 (0.044) | -0.002 (0.034) | -0.027 (0.022) | 0.036 (0.037) | 0.006 (0.041) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Assume All Missing Control Group Cases Attended Private School* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.383*** (0.056) | 0.383*** (0.056) | 0.383*** (0.056) | 0.383*** (0.056) | 0.383*** (0.056) |
| 2nd stage: Effect of private school attendance on participation | -0.016 (0.056) | -0.002 (0.044) | -0.034 (0.028) | 0.046 (0.047) | 0.008 (0.053) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Drop Missing Control Group Cases* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.499*** (0.018) | 0.499*** (0.018) | 0.499*** (0.018) | 0.499*** (0.018) | 0.499*** (0.018) |
| 2nd stage: Effect of private school attendance on participation | -0.026 (0.044) | -0.011 (0.035) | -0.028 (0.022) | 0.030 (0.037) | 0.0003 (0.041) |
| *N* | 2,479 | 2,479 | 2,479 | 2,479 | 2,479 |

**NOTE:** Robust standard error clustered by family in parentheses below coefficient estimate for effect of private school attendance. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

# Appendix Tables

**Table A1. Coefficients and standard errors from OLS regression predicting political participation, by participation outcome**

| Variable | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| Treatment | -0.006 | -0.001 | -0.013 | 0.018 | 0.003 |
| | (0.022) | (0.017) | (0.011) | (0.018) | (0.020) |
| Math score (percentile) | 0.00003 | -0.001 | -0.0005 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Reading score (percentile) | 0.003*** | 0.003*** | 0.001 | 0.001 | 0.002*** |
| | (0.001) | (0.001) | (0.0004) | (0.001) | (0.001) |
| Score missing | -0.135 | -0.119 | -0.178** | 0.051 | -0.062 |
| | (0.152) | (0.088) | (0.076) | (0.118) | (0.127) |
| Parents' education (relative to some high school) | | | | | |
| HS Grad or GED | 0.091** | -0.012 | 0.006 | 0.028 | 0.008 |
| | (0.046) | (0.043) | (0.023) | (0.041) | (0.046) |
| Some college | 0.068 | -0.032 | 0.019 | -0.005 | -0.0001 |
| | (0.045) | (0.043) | (0.024) | (0.040) | (0.046) |
| BA or higher | 0.063 | -0.038 | -0.012 | -0.058 | -0.056 |
| | (0.056) | (0.052) | (0.028) | (0.048) | (0.055) |
| Missing | 0.085 | -0.123** | -0.013 | -0.006 | -0.024 |
| | (0.101) | (0.060) | (0.044) | (0.083) | (0.091) |
| Family income (relative to less than $7,425) | | | | | |
| $7,425-$11,879 | -0.013 | 0.020 | -0.006 | 0.013 | 0.023 |
| | (0.040) | (0.038) | (0.023) | (0.035) | (0.040) |
| $11,880-$16,334 | -0.041 | -0.014 | -0.063*** | 0.073 | 0.035 |
| | (0.054) | (0.050) | (0.023) | (0.050) | (0.056) |
| $16,335-$22,274 | -0.030 | -0.027 | -0.021 | 0.041 | 0.003 |
| | (0.052) | (0.043) | (0.024) | (0.043) | (0.049) |
| $22,275-$29,699 | 0.004 | -0.008 | 0.012 | -0.007 | 0.009 |
| | (0.061) | (0.054) | (0.033) | (0.049) | (0.058) |
| $29,700 or more | -0.063 | -0.092* | -0.007 | -0.003 | -0.047 |
| | (0.060) | (0.049) | (0.032) | (0.051) | (0.058) |
| Missing | 0.047 | -0.009 | 0.061 | 0.057 | 0.023 |
| | (0.059) | (0.054) | (0.044) | (0.053) | (0.060) |
| Mother born in U.S. | -0.053 | -0.077** | -0.081*** | -0.059* | -0.097** |
| | (0.038) | (0.035) | (0.024) | (0.035) | (0.038) |
| Mother's birth country missing | -0.247 | -0.144* | -0.127*** | -0.059 | -0.139 |
| | (0.151) | (0.080) | (0.048) | (0.137) | (0.145) |
| Mother's race/ethnicity (relative to white or Asian) | | | | | |
| African American | 0.108 | 0.133** | -0.009 | 0.134** | 0.145** |
| | (0.075) | (0.064) | (0.051) | (0.055) | (0.070) |
| Hispanic | 0.080 | 0.069 | -0.036 | 0.110** | 0.101 |
| | (0.074) | (0.063) | (0.051) | (0.055) | (0.070) |
| Other/missing | 0.170* | 0.136* | 0.012 | 0.157** | 0.192** |
| | (0.091) | (0.082) | (0.065) | (0.073) | (0.088) |
| Mother employed | -0.053 | 0.0002 | -0.015 | 0.011 | -0.019 |
| | (0.036) | (0.031) | (0.019) | (0.030) | (0.034) |

**Table A1. Coefficients and standard errors from OLS regression predicting political participation, by participation outcome**

| Variable | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| Father absent | -0.018 | -0.013 | -0.005 | -0.018 | -0.015 |
| | (0.031) | (0.028) | (0.016) | (0.026) | (0.031) |
| English at home | 0.024 | 0.029 | 0.038 | 0.038 | 0.045 |
| | (0.043) | (0.038) | (0.024) | (0.039) | (0.043) |
| Female | 0.145*** | 0.059** | 0.017 | 0.122*** | 0.106*** |
| | (0.028) | (0.025) | (0.015) | (0.024) | (0.027) |
| Gender missing | -0.211* | -0.033 | -0.043** | -0.037 | -0.122 |
| | (0.118) | (0.070) | (0.022) | (0.121) | (0.117) |
| N | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| R-squared | 0.0843 | 0.0999 | 0.0671 | 0.0697 | 0.0724 |

NOTE: Robust standard error clustered by family in parentheses below coefficient estimate. Coefficients and standard errors for interactions of control variables and indicator for missing baseline test score not presented but are available upon request. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

**Table A2. Number of observations and control group means, by participation outcome and subgroup**

| Subgroup | N | Control Group Mean | | | | |
|---|---|---|---|---|---|---|
| | | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
| **Parents' education** | | | | | | |
| HS Grad or less | 1,132 | 0.446 | 0.196 | 0.059 | 0.237 | 0.311 |
| Some college or more | 1,400 | 0.473 | 0.186 | 0.067 | 0.197 | 0.295 |
| | | | | | | |
| **Mother's birthplace** | | | | | | |
| U.S. | 1,578 | 0.452 | 0.178 | 0.055 | 0.206 | 0.286 |
| Outside U.S. | 1,060 | 0.465 | 0.197 | 0.074 | 0.225 | 0.316 |
| | | | | | | |
| **Race/Ethnicity of Mother** | | | | | | |
| African American | 1,098 | 0.451 | 0.206 | 0.069 | 0.226 | 0.310 |
| Hispanic | 1,218 | 0.468 | 0.174 | 0.052 | 0.202 | 0.287 |
| | | | | | | |
| **Primary language** | | | | | | |
| English | 1,857 | 0.463 | 0.189 | 0.068 | 0.224 | 0.307 |
| Not English | 781 | 0.443 | 0.176 | 0.048 | 0.188 | 0.275 |
| | | | | | | |
| **Gender** | | | | | | |
| Female | 1,306 | 0.535 | 0.208 | 0.069 | 0.277 | 0.356 |
| Male | 1,332 | 0.377 | 0.162 | 0.055 | 0.148 | 0.238 |
| | | | | | | |
| **Grade** | | | | | | |
| Grade 2 or lower | 1,587 | 0.422 | 0.148 | 0.064 | 0.216 | 0.284 |
| Grade 3 or higher | 1,051 | 0.513 | 0.244 | 0.058 | 0.208 | 0.321 |

**Table A3. Coefficient estimate and standard error for effect of private school attendance from two-stage least squares model, by model specification**

| | Ever Registered to Vote | Voted in 2008 General Election | Voted in 2010 General Election | Voted in 2012 General Election | Voted in 2008, 2010, or 2012 General Elections |
|---|---|---|---|---|---|
| *Missing Control Group Cases Never Attended Private School* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.654*** | 0.654*** | 0.654*** | 0.654*** | 0.654*** |
| | (0.018) | (0.018) | (0.018) | (0.018) | (0.018) |
| 2nd stage: Effect of private school attendance on participation | -0.009 | -0.001 | -0.02 | 0.027 | 0.005 |
| | (0.033) | (0.026) | (0.016) | (0.028) | (0.031) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Control Group Cases With Two Missing Observations Attended Private School* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.565*** | 0.565*** | 0.565*** | 0.565*** | 0.565*** |
| | (0.020) | (0.020) | (0.020) | (0.020) | (0.020) |
| 2nd stage: Effect of private school attendance on participation | -0.011 | -0.001 | -0.023 | 0.031 | 0.005 |
| | (0.038) | (0.030) | (0.019) | (0.032) | (0.036) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Control Group Cases With One Missing Observation Attended Private School* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.430*** | 0.430*** | 0.430*** | 0.430*** | 0.430*** |
| | (0.022) | (0.022) | (0.022) | (0.022) | (0.022) |
| 2nd stage: Effect of private school attendance on participation | -0.014 | -0.002 | -0.03 | 0.041 | 0.007 |
| | (0.050) | (0.039) | (0.025) | (0.042) | (0.047) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *All Missing Control Group Cases Attended Private School* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.280*** | 0.280*** | 0.280*** | 0.280*** | 0.280*** |
| | (0.023) | (0.023) | (0.023) | (0.023) | (0.023) |
| 2nd stage: Effect of private school attendance on participation | -0.022 | -0.003 | -0.047 | 0.063 | 0.011 |
| | (0.077) | (0.060) | (0.038) | (0.065) | (0.072) |
| *N* | 2,638 | 2,638 | 2,638 | 2,638 | 2,638 |
| *Drop Missing Control Group Cases* | | | | | |
| 1st stage: Effect of voucher offer on private school attendance | 0.589*** | 0.589*** | 0.589*** | 0.589*** | 0.589*** |
| | (0.021) | (0.021) | (0.021) | (0.021) | (0.021) |
| 2nd stage: Effect of private school attendance on participation | -0.047 | -0.037 | -0.038 | -0.004 | -0.034 |
| | (0.041) | (0.033) | (0.022) | (0.036) | (0.039) |
| *N* | 2,154 | 2,154 | 2,154 | 2,154 | 2,154 |

**NOTE:** Robust standard error clustered by family in parentheses below coefficient estimate for effect of private school attendance. * p<0.10; ** p<0.05; *** p<0.01.