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Private School Choice and Character: More Evidence from Milwaukee

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Abstract
We examine the effects of Milwaukee's school voucher program on adult criminal activity and paternity suits. Using matched student-level data, we find that exposure to the program in eighth or ninth grade predicts lower rates of conviction for criminal activity and lower rates of paternity suits by ages twenty-five to twenty-eight. Specifically, exposure to the MPCP is associated with a reduction of around 53 percent in drug convictions, 86 percent in property damage convictions, and 38 percent in paternity suits. The program effects tend to be largest for males and students with lower levels of academic achievement at baseline.

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Keywords: school vouchers, school choice, educational policy, program evaluation, private education, educational reform, economics of education, character skills, crime

I. Introduction

Private school choice programs are government initiatives that directly or indirectly provide financial support for parents to enroll their child in a private school. These programs use government-financed school vouchers, tax-credit-financed scholarships, or K–12 education savings accounts to fund access to private schooling largely for students with low incomes or disabilities. Fifty-six private school choice programs are operating in twenty-nine states plus the District of Columbia, enrolling over 482,000 students in 2018–19 (EdChoice 2019).[1]

[1] These totals include "town-tuitioning" voucher programs in the rural areas of Maine, New Hampshire, and Vermont but exclude nine tax provisions that merely provide deductions or partial credits for a parent's personal private school

Most evaluations of private school choice programs have examined their effects on standardized test scores. A recent meta-analysis of nineteen experimental studies of eleven different programs around the world finds that private school vouchers have null or small positive effects on student achievement (Shakeel, Anderson, and Wolf 2016). Test-score outcomes vary significantly across evaluations, however, based on each individual study's research methodology, academic subject, and age. The achievement effects of vouchers tilt positively in studies that are experimental, focused on reading, and published prior to 2012. They tilt negatively in studies that are quasi-experimental, focused on math, and published after 2012 (Wolf and Egalite 2018).

Recent experimental evaluations report negative effects on math scores in the first two years of the DC Opportunity Scholarship Program that fade to null by the third year (Webber et al. 2019) and negative effects on both math and reading scores after four years of the Louisiana Scholarship Program (Mills and Wolf 2017; Mills and Wolf 2019). Two recent quasi-experimental evaluations find mostly negative effects of voucher programs on student test scores in Ohio and Indiana over four-year periods (Figlio and Karbownik 2016; Waddington and Berends 2018).

Standardized test scores, however, do not fully capture society's goals for education (Macedo and Wolf 2004; Zimmer et al. 2009). Tests measure the effects of schools and teachers on the cognitive performance of students. Schools are also social institutions that aim to improve noncognitive skills such as grit, persistence, conscientiousness, and social functioning (Arthur, Davison, and Stow 2000; DeAngelis 2019b; Duckworth et al. 2007; Egalite, Mills, and Greene 2016; Hitt, Trivitt, and Cheng 2016). While some studies find a link between teachers' effects on standardized tests and their effects on long-term outcomes (Chetty, Friedman, and Rockoff 2014), two recent reviews of the literature find that the effects of school choice interventions, specific schools of choice, and teachers on student test scores do not consistently predict the effects of those factors on long-term outcomes such as high school graduation, college enrollment, employment, and health (DeAngelis 2019a; Hitt, Wolf, and McShane 2018). Improving student noncognitive character skills can lead to higher levels of educational attainment and better life

expenses. We exclude personal deductions and credits because they are not "programs" per se.

outcomes as measured by lifetime earnings, employment, and citizenship (Reynolds, Temple, and Ou 2010).

Do private school choice programs affect students' character skills? In theory, such programs could improve character skill development through market pressure, strong-culture organizations, and exposure to peers who discourage risky behaviors. In this study, we use student-level data to estimate the effects of exposure to the longest-standing modern-day voucher program in the United States—the Milwaukee Parental Choice Program (MPCP)—on adult criminal activity and paternity suits. We find that exposure to the program in eighth or ninth grade is associated with lower rates of conviction for criminal activity and lower rates of paternity suits by the time students are twenty-five to twenty-eight years old. The benefits associated with program participation tend to be largest for males and students with lower levels of academic achievement at baseline.

II. Theory

Schools should teach people to be responsible citizens, increase social cohesion, and boost democratic participation (Mann 1855; Dewey 1916; Tooley 2000). A key element of responsible citizenship is obeying just laws crafted by legitimate government institutions and procedures (Locke 1690; West 1965). Studies consistently show a positive economic return to more years of education (Mincer 1974; Ferrer and Riddell 2002). People who earn more have a greater stake in society and therefore are less tempted by crime (Rouse 2005). Society has a strong incentive to prevent crime through the effective education of children. On average, each case of vandalism inflicts $5,457 in 2016 US dollars in societal costs and each robbery sets the community back $47,500 (McCollister, French, and Fang 2010). More effective schooling experienced for additional years could dissuade young adults from engaging in risky behavior.

Greater access to private schooling might improve character skills. Many parents expect schools to enhance the character of their children (Zeehandelaar and Winkler 2013; Stewart and Wolf 2014; Erickson 2019). When families choose their children's school, competitive pressure from the potential loss of a dissatisfied student to a competing school may provide an additional incentive for schools to develop the noncognitive skills of students that parental customers desire (Chubb and Moe 1988; Friedman 1997; DeAngelis and Holmes Erickson 2018).

What specific mechanisms might schools of choice use to enhance character skills? Students attend chosen schools voluntarily, in contrast to assignment to a district-run public school by their residential address. People who associate voluntarily often share similar values and expectations, making it easier to establish a strong educational culture and generate social capital (Coleman and Hoffer 1987; Hill, Foster, and Gendler 1990; Brandl 2010). Such "voluntary associations not only generate 'social capital' . . . they *presuppose* it" (Berkowitz 1996, p. 47). Sustained exposure to a voluntary, and therefore value-intensive, educational environment should increase student levels of personal responsibility and conscientiousness.

Similarly, when allowed to be autonomous, schools of choice tend to be more distinctive than traditional public schools (Fox and Buchanan 2014; DeAngelis and Burke 2017). Students interested in the distinctive mission of their school and its curriculum may be less likely to engage in risky behaviors out of boredom (Wurmser 1974).

Schools with religious affiliations tend to emphasize the importance of shaping student character traits and moral behavior (Bellah et al. 1985; Johnson 2011; Jeynes 2012). Although sectarian private schools participating in choice programs tend to be funded at lower levels than neighboring public schools (Wolf and McShane 2013; Egalite 2015; Lueken 2018; Trivitt and DeAngelis 2020), "sectarian schools are communities generating and dispensing inspiration and nurture that accomplishes much more" than money can buy (Brandl 2006, p. 32). Since most schools participating in choice programs are sectarian (e.g., DeAngelis 2020; Sude, DeAngelis, and Wolf 2018), and these religious schools teach students that God always and everywhere is watching and evaluating what they do, private schools of choice might be expected to improve the subsequent behavior of their charges.

Finally, because private schools are typically located in more-affluent and lower-crime areas, access to private school choice could decrease risky behaviors by separating vulnerable children from peers who would pressure them to join criminal enterprises (DeAngelis and Dills 2018). Peer pressure at more-advantaged schools may discourage the negative activities of students (Akerlof and Kranton 2002). Law enforcement officials may be more familiar with rebellious students in public schools, and therefore more likely to arrest them, because police officers often are stationed there (Shakeel & DeAngelis 2018).

For the above reasons, we expect that access to a private school through the MPCP improves character skills, leading to fewer risky behaviors that result in criminal convictions and paternity suits. It is possible that private school choice programs have differential effects on a student's character skills depending on the student's gender or initial achievement level. Young male adults are more likely to engage in criminal activity than young female adults. Since males are more at risk of negative behavioral outcomes, we hypothesize that exposure to private schooling would have a larger effect on criminal outcomes for males. Since every paternity suit in our sample includes a male and a female, we expect no difference in the effect of the MPCP on paternity suits by gender. Finally, lower-achieving students are less likely to feel optimistic regarding their prospects for success in legitimate pursuits and, therefore, a life of crime is more tempting to them. We hypothesize that exposure to the MPCP will have a greater effect on this more vulnerable subgroup of lower achievers.

Specifically, we hypothesize:

1. Exposure to the MPCP reduces adult criminal convictions and paternity suits.
2. The effects of the MPCP are largest for the most at-risk subpopulations of students:
   a. Males experience a larger reduction in criminal outcomes than females;
   b. Students with lower levels of academic achievement at baseline experience a larger reduction in both criminal and paternity suit outcomes than students with higher levels of academic achievement at baseline; and,
   c. Male lower performers demonstrate the largest programmatic effects of any student subgroup.

III. Literature Review

The research on the topics of both school choice and crime is extensive. Unfortunately, the intersection between those two robust literatures is minimal.

Test scores are the focal outcome of most evaluations of private school choice programs. Studies of the non-test-score outcomes of school choice have been rare until recently, yet still represent an undeveloped literature. Six of eight evaluations of the attainment effects of private school choice find that choice boosts the likelihood of students graduating from high school, enrolling in college, or obtaining a college degree (Cowen et al. 2013; Wolf et al. 2013;

Cheng, Chingos, and Peterson 2019; Chingos and Peterson 2015; Chingos, Monarrez, and Kuehn 2019; Chingos 2018; Holmes Erickson, Mills, and Wolf 2019; Wolf, Witte, and Kisida 2019).

DeAngelis (2017) reviews the findings from eleven studies of the effects of private school choice on civic outcomes. He finds that choice programs have effects ranging from zero to significantly positive on political participation, volunteering, and charitable giving (e.g., Bettinger and Slonim 2006; Campbell 2008; Fleming 2014; DeAngelis and Wolf 2019b). In a more expansive review of twenty-one studies of the civic effects of various forms of school choice, Wolf (2007) reports generally positive effects, with some null findings and only a few results suggesting that public schooling better promotes citizenship traits. Eight studies of the effect of private school choice programs on the racial integration of schools all reveal null or positive effects (Swanson 2017).

Avoiding the criminal justice system is critical to the life success of low-income urban youth. Criminal records have negative effects on subsequent employment opportunities, especially for young Black men (Freeman 1987; Pager 2003; Apel and Sweeten 2010). Pager, Western, and Sugie (2009) randomly assigned criminal records and races to otherwise equivalent resumes in New York City. After sending these resumes to employers, the authors find that criminal records significantly reduce the likelihood that job-seekers are interviewed. The negative effects are larger for Black applicants. Agan and Starr (2017) performed a similar field experiment and find that employer callback rates are 5 percentage points (about 38 percent) lower for resumes that were randomly assigned a felony conviction. Waldfogel (1994) reports that first-time convictions reduce the likelihood of employment by 5 percentage points and reduce lifetime income by up to 30 percent.

The research literature on schooling and crime focuses on the effects of general schooling laws and dropping out of school on criminal activity, largely ignoring school choice (Luallen 2006; Lochner 2010; Anderson 2014). Several studies analyze the effects of school desegregation on crime outcomes (Weiner, Lutz, and Ludwig 2009; Billings, Deming, and Rockoff 2014), or whether educational degrees influence criminal activity (Machin, Marie, and Vujić 2011). None of these studies of educational effects on crime investigates the effects of school sector. School choice studies rarely focus on crime as an outcome and crime studies rarely focus on school choice as a cause.

Six studies have broken the mold and investigated the relationship between school choice and crime. Students who won public school admission lotteries in Charlotte-Mecklenburg County, North Carolina, had significantly lower crime rates as young adults than the control group members who lost lotteries (Deming 2011). Specifically, winning a lottery to attend a public school of choice reduced crime by about 50 percent for high-risk male students. Nationally, an increase of one standard deviation in the availability of school choice by residential relocation is correlated with a drop of 40 percent in juvenile crime (Dills and Hernández-Julián 2011).

Dobbie and Fryer (2015) report that winning a lottery to attend a public charter school in the Harlem Children's Zone drives the likelihood of a male student going to prison down to zero while decreasing female teen pregnancy rates by 59 percent. A decrease in private school choice in a community, due to the closing of a Catholic school, tends to generate an increase in crime (Brinig and Garnett 2014). Recently, McEachin et al. (2020) found that access to public charter schools in North Carolina is associated with a 36 percent reduction in the likelihood of committing a felony and a 38 percent reduction in the likelihood of committing a misdemeanor. The six existing studies of various forms of school choice all conclude that choice has a positive relationship with crime reduction.

Only one study exists of the effect of a private school choice *program* on the criminal behavior of young adults (DeAngelis and Wolf 2019a). Using student-level data from a longitudinal evaluation of the MPCP, the authors find that sustained participation in the MPCP reduces the likelihood of a student engaging in criminal activity by the time he or she reaches the age of twenty-two to twenty-five. Because most significant effects in that analysis are dependent on students' persistence in the choice program, and that persistence might be driven by unmeasured student and family characteristics correlated with the likelihood of committing crimes, the researchers cannot conclusively rule out post-match selection bias as the reason for their results.

We build on that previous study in at least five important ways: (1) we look up the cumulative record of risky behaviors three years later than the original study—in the fall of 2018—when the students were twenty-five to twenty-eight years old; (2) we use "exposure to the program in 2006" as our variable of interest in an intent-to-treat analysis that is free of post-match selection bias; (3) instead of simply examining the changes in probabilities of being convicted of any

crimes, we track the counts of each type of criminal behavior to produce a more holistic crime measure with more analytic power; (4) we include additional categories of outcomes such as the total amount of criminal fees students were assessed by the state and the total number of paternity suits the students experienced by the fall of 2018; and (5) we examine heterogeneous effects of the school voucher program on outcomes based on gender and initial academic ability.

IV. Description of the Program

The Milwaukee Parental Choice Program (MPCP) is a government-run private school initiative piloted in 1990. It represented the first US test of the idea that private school vouchers might improve outcomes for low-income urban students. Enrollment in the program was capped at 1.5 percent of Milwaukee Public Schools (MPS) enrollment, which equated to about 500 students. Religious schools were excluded from the program, leaving only seven secular private schools as school choice destinations for students (Witte et al. 2008).

The MPCP evolved over time. The enrollment cap increased gradually starting in 1996 and was eliminated completely in 2012. The program allowed religious schools to serve students with vouchers starting in 1998. These steps to ease restrictions on both the demand and supply sides of this market-based education reform resulted in the MPCP claiming a 25 percent K–12 market share in Milwaukee by 2014–15. Public school choice options, including public charter schools and enrollment options outside of a child's residentially assigned school, expanded in the city during this period.

Evaluations indicated that competitive pressures from widespread school choice in Milwaukee produced achievement gains for students who remained in traditional public schools (Greene 2001; Greene and Forster 2002; Hoxby 2003; Carnoy et al. 2007; Chakrabarti 2008; Greene and Marsh 2009). The positive competitive effects of school choice in Milwaukee likely contributed to the findings from the most recent evaluation of the MPCP that the test score gains for actual voucher participants were limited (Witte et al. 2014).

Participants in the MPCP must be entering grades K–12 and live within the geographic boundaries of Milwaukee. From 1990 through 2011, only students with family incomes of 175 percent of the poverty level or less were eligible. In 2012, the program's income ceiling was increased to 300 percent of the poverty line. Students apply for the voucher just as college students apply for financial aid:

after having enrolled in a chosen private school. Most private school choice programs reverse the sequence of those actions, with students first receiving vouchers and then enrolling in a participating private school.

The MPCP voucher was worth a maximum of $6,501 in 2006. That amount was about 40 percent less than the per pupil spending in MPS that year (Costrell 2008). The maximum value of the voucher had dropped to $6,442 by 2011, or 57 percent less than per pupil spending in MPS (McShane et al. 2012). Participating private schools reported subsidizing their students on vouchers by amounts averaging $962 per student in 2006 (Kisida, Jensen, and Wolf 2009) and $1,250 in 2011 (McShane et al. 2012).

Private schools participating in the MPCP generally must accept the voucher as the full cost of educating the child. The one exception is that, since 2012, parents of students in grades nine through twelve with an income greater than 220 percent of the federal poverty level can be charged a fee on top of the voucher amount. Participating private schools must administer state standardized tests, be accredited by the state within three years of program participation, let students opt out of religious activities, require all teachers and administrators to have a teaching license or a bachelor's degree, and admit voucher-eligible students on a random basis (EdChoice 2019).

V. Data and Matching Procedure

Participating private schools must admit students by random lottery when they have more applicants than seats. Since religious schools were allowed into the program in 1998, few admissions lotteries have been held, as schools tend to stop recruiting students once their capacity is filled (Cowen et al. 2013). The dearth of randomly assigned MPCP students has prevented researchers from conducting an experimental evaluation of the program since 1998.

We used a hybrid of exact and propensity-score matching to produce an MPS comparison group to our MPCP treatment group. We exactly matched each voucher student to the set of MPS students in their grade, neighborhood, and within 2.5 percentiles above or below their 2006 baseline test scores. From that set of exact-matched students, the specific MPS student selected as a comparison to each voucher student was the one with the highest propensity score based on race, gender, English language learner (ELL) status, and math and reading baseline test scores (Witte et al. 2008). All 801 MPCP students in ninth grade and 290 randomly selected MPCP eighth

graders in 2006 were combined into the programmatic treatment sample of 1,091. All but two of those treatment students were successfully matched to an MPS comparison student, producing a total analytic sample of 2,178.

The voucher and comparison groups are similar on most factors. Table 1 suggests that the students participating in the MPCP had higher baseline reading scores and had more highly educated parents than their counterparts; however, the MPCP students also had lower baseline math test scores and came from households with lower income levels than their matched MPS peers. Put differently, the direction of selection bias, if any exists after the match, is unclear. All observable differences are controlled for in our model estimations.

Table 1. Statistics on key covariates of matched groups

	MPS in 2006	MPCP in 2006	*N*
Female	0.53	0.57*	2,178
Black	0.70	0.70	2,178
Hispanic	0.18	0.19	2,178
Asian	0.04	0.03	2,178
White	0.07	0.07	2,178
Grade in 2006	8.73	8.74	2,178
Math in 2006	0.04*	-0.03	2,178
Reading in 2006	0.02	0.13***	2,178
Parent completed college	0.12	0.16**	1,506
Parent some college	0.31	0.35	1,506
Income over 50k	0.17***	0.05	1,401
Income under 25k	0.54***	0.59	1,401

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. MPS is Milwaukee Public Schools. MPCP is Milwaukee Parental Choice Program.

A series of within-study comparisons has tested the ability of matching approaches to approximate the findings from random-assignment education studies. Those replication studies consistently find that matching students on "geography" (i.e., neighborhood) is essential to generating an appropriate nonexperimental comparison group (Heckman, Ichimura, and Todd 1997; Cook, Shadish, and Wong 2008; Bifulco 2012). Census tracts in Milwaukee match actual neighborhood boundaries. Families tend to cluster in neighborhoods where everyone has similar moral values, motivation, and educational aspirations for their children. Matching voucher and MPS students on neighborhood therefore effectively controls for such unmeasured factors that otherwise tend to bias nonexperimental evaluations of school choice effects.

Our first effort at original data collection involved surveying the parents of matched students regarding their income, education levels, and marital status (Witte et al. 2008). Our survey response rate was 69 percent, which is high for a telephone survey. A total of 73 percent of the MPCP parents responded compared to 66 percent of the MPS parents. We use standard weighting techniques to adjust for any differences in baseline characteristics between the MPCP treatment and matched comparison groups due to differential survey response rates.

The Wisconsin Court System's Circuit Court Access case search website provided the outcome variables for this study. By law, every criminal charge and conviction in Wisconsin is entered into this searchable public database. Researchers searched for records using student first name, last name, and date of birth, not knowing if a given student was in the voucher or comparison group. We used ten different categories for dependent variables. Each judicial record matched to a student in our sample generated a count of 1 for each category for which it documented a conviction for a felony, misdemeanor, drug-related offense, property damage, disorderly conduct, battery, theft, or traffic-related offense. The record generated a count of 1 for a paternity suit if that was the subject of the court case.

These categories are not all mutually exclusive. Misdemeanors are mutually exclusive of felonies, while traffic crimes are generally mutually exclusive of both. Drug and theft crimes, however, could be felonies or misdemeanors, depending on their severity. Thus, a single judicial record could produce (1) multiple codings of 1 across the various behavioral indicators, (2) a single 1, or (3) all zeroes (e.g., if it represented a charge but not a conviction). Multiple judicial records for a given student in the study could produce multiple counts of convictions for a single outcome category or multiple counts of "1" across different categories. We also noted all fines (in current dollars) that were assessed. By law, the data were restricted to outcomes for adults age eighteen or older. Because we searched the database during the fall of 2018, the students were twenty-five to twenty-eight years old when we looked up their records, and thus experienced seven to ten years of adulthood in which they might have been convicted of one or more crimes or might have been a party to one or more paternity suits.

Table 2 summarizes our sample of 2,178 students and their characteristics. On average, each person in our dataset was convicted

of 0.19 felonies, 0.27 misdemeanors, 0.11 drug-related offenses, 0.01 damages to property, 0.07 disorderly conduct offenses, 0.03 batteries, 0.05 thefts, 0.73 traffic-related offenses, and had 0.11 paternity suits. The students were assessed an average of $526 in crime-related fees. Our analysis is challenged by the limited variation in most of our outcome variables. All else being equal, it is more difficult to identify statistically significant relationships between a program treatment and outcome variables when the outcome variables hardly vary. Thus, to minimize the risk of type II "false negative" errors in our analysis, we use $p < .10$ as a minimal level of statistical significance for any group differences, using a two-tailed test.

Table 2. Descriptive statistics of all variables used in analysis

Variable	*N*	Mean	SD	Min	Max
Student					
MPCP 2006	2,178	0.50	0.50	0	1
Black	2,178	0.70	0.46	0	1
Hispanic	2,178	0.18	0.39	0	1
Asian	2,178	0.04	0.19	0	1
White	2,178	0.07	0.26	0	1
Female	2,178	0.55	0.50	0	1
Grade in 2006	2,178	8.74	0.44	8	9
Math *Z*-score	2,178	0.00	0.87	-3.13	3.00
Reading *Z*-score	2,178	0.07	0.90	-2.97	2.54
Parent					
Income over 50k	1,401	0.11	0.31	0	1
35k < Income < 50k	1,401	0.14	0.35	0	1
25k < Income < 35k	1,401	0.18	0.39	0	1
Income under 25k	1,404	0.31	0.46	0	1
Parent high school grad	1,506	0.29	0.45	0	1
Some college	1,506	0.33	0.47	0	1
Full college	1,506	0.15	0.35	0	1
Both in household	1,502	0.34	0.47	0	1
Churchgoer	1,500	0.58	0.49	0	1
Outcomes					
Felonies	2,178	0.19	0.79	0	16
Misdemeanors	2,178	0.27	0.96	0	17
Drug crime	2,178	0.11	0.54	0	12
Property damage	2,178	0.01	0.13	0	3
Disorderly conduct	2,178	0.07	0.34	0	4
Batteries	2,178	0.03	0.21	0	3
Thefts	2,178	0.05	0.35	0	7
Traffic	2,178	0.73	1.80	0	21
Fines ($)	2,178	526	1,844	0	37,718
Paternity disputes	2,178	0.11	0.37	0	3

Notes: SD is standard deviation. MPS is Milwaukee Public Schools. MPCP is Milwaukee Parental Choice Program.

VI. Methods

Our basic model 1 uses an ordinary least squares regression approach of the form:

$$Outcome_i = \beta_0 + \delta_1 MPCP06_i + \varepsilon_{it} \qquad (1)$$

where for each outcome of interest (conviction for felonies, misdemeanors, drug-related offenses, property damage, disorderly conduct, batteries, thefts, traffic-related offenses, total fines in current dollars, and paternity suits), δ_1 is the difference associated with exposure to MPCP (enrolled in the MPCP in 2006). Each

observation is coded as nonnegative integer values for each outcome category because the data are counts of cases, except for "total fines." The category for total fines (in constant dollars) is also nonnegative but is a continuous variable rather than a count. We obtain robust standard errors of ε_{it} by clustering the individual errors *i* by census tract *t* because students within the same geographic region tend to be similar on unobservable characteristics that otherwise might generate spatially autocorrelated error terms. As our sample of 2,178 students comes from only 194 different census tracts, clustering the errors increases their size, thereby leading to more conservative significance tests.

We use an intent-to-treat (ITT) approach. All of the students in the MPCP group are coded "1" for *MPCP* regardless of how long they persisted in the program. Our analysis estimates the effect of mere "exposure" to the MPCP (for whatever duration of time starting in the fall of 2006) on subsequent criminal behavior, further making our estimates conservative. We also use this ITT approach in our analysis because nonrandom self-sorting of students across sectors took place after the 2006 baseline match year (Cowen et al. 2012) that otherwise might bias our estimates of the program's effect.

Our preferred model 2 adds student controls to the estimation. Since we have complete data on all the student control variables, adding those variables does not reduce our analytic sample. Our preferred model takes the form:

$$Outcome_i = \beta_0 + \delta_1 MPCP06_i + \beta_1 X_i + \beta_2 test_{2006} + \varepsilon_{it} \quad (2)$$

where the outcome and MPCP exposure variables, as well as the error term, are the same as described for model 1. Added in this equation are vector *X* of student race, gender, and baseline grade (eighth or ninth) indicators; and $test_{2006}$, a vector of student math and reading test scores in 2006, standardized to have a mean of zero and a standard deviation of one. Because we control for student 2006 test scores, any effect that the MPCP has on improving character skills by boosting student test scores prior to that date would be captured by that control variable, making our independent estimate of the effect of the MPCP on character skills conservative.

Model 2 is our preferred vehicle for estimating the effects of the MPCP on student risky behaviors because it controls for student characteristics known to be predictive of irresponsible behavior (e.g., academic ability, gender, age) while preserving all of the observations in our analytic sample. More extensive statistical models can control for family background variables that also might predict criminal

activity, but they bring with them the disadvantage of reducing the size of the analytic sample by more than one-third, thereby decreasing our study power. Since the nearly 800 observations excluded by adding family variables is likely a nonrandom subgroup of our sample, adding those variables also risks introducing survey nonresponse bias into our analysis.

With those cautions in mind, we also estimate model 3 as a robustness test of our analytically preferred model 2. Model 3 takes on the same functional form as model 2 but restricts the sample to the 1,385 students with all parental controls available because their parents responded to the survey. Model 3 does not actually add those parental variables. We include a model 4 with all parental controls included. Including both estimations allows us to see how much of the change in the model 4 estimates, relative to our preferred model 2 estimates, is due to its limited sample and how much is due to its added control variables. Model 4 takes the form:

$$Outcome_i = \beta_0 + \delta_1 MPCP06_i + \beta_1 X_i + \beta_2 test_{2006} + \beta_3 Z_i + \varepsilon_{it} \quad (4)$$

where for each outcome of interest, $\delta_1$is the difference associated with exposure to MPCP after accounting for the same vector X of student characteristics and $test_{2006}$ described above, but adding vector Z of parent income levels, education levels, churchgoing activity, and whether or not both parents lived at home. Because of the count nature of our data, we also use Poisson regression and negative binomial regression as robustness checks for all results.

VII. General Results

Tables 3 and 4 present the results for four different statistical models:

1. the MPCP indicator variable with no control variables for the full sample
2. the MPCP indicator with student controls for the full sample
3. the MPCP indicator with student controls for the smaller sample of students with all parental controls available
4. the MPCP indicator with both student and parental controls for the smaller sample

A negative coefficient represents a reduction in criminal convictions or paternity disputes and therefore signals a beneficial effect of exposure to the private school choice program. Exposure to the MPCP is correlated with a reduction in nine of the ten negative behavior measures. The only effect estimates that are positively

signed are for theft, and none of those four estimates is even close to statistically significant.

The effects of the MPCP on reducing negative behavioral outcomes do vary in statistical significance across the indicators. Exposure to the MPCP has a highly statistically significant effect on reducing the number of drug convictions across all four statistical models. Specifically, for the model with all controls, exposure to the MPCP is associated with a reduction of about 0.1 drug-related offenses. This result is equivalent to around a 90 percent reduction relative to the mean and around an 18 percent of a standard deviation reduction in drug-related offenses.

The program also has a statistically significant effect on reducing property-damage convictions, at least in the uncontrolled model and the model with student controls, which is our preferred model. The effects of the MPCP on property damage convictions are not statistically significant in the two models with the smaller sample, even though the size of the regression coefficient for the MPCP effect is larger in those estimations. That pattern suggests that lower analytic power, and not stronger control variables, likely explains the loss of statistical significance. The beneficial effect of the MPCP on lowering rates of paternity suits is statistically significant in models 1 through 3 but not in model 4. That difference appears partly due to a coefficient that shrinks by about one-sixth when parental controls are introduced and partly due to the standard error of the coefficient increasing dramatically due to the loss of 800 observations.

Our preferred model 2 includes the complete sample with student controls. Three of the ten results are statistically significant at the $p < 0.05$ level or better. Specifically, exposure to the MPCP is associated with a reduction of around 0.06 drug-related offenses, 0.01 property-damage offenses, and 0.04 paternity suits. These estimates are equivalent to a 53 percent reduction in drug convictions, an 86 percent reduction in property damage convictions, and a 38 percent reduction in paternity suits.

In terms of generalizable programmatic effect sizes, the estimates are equivalent to an 11 percent of a standard deviation reduction in drug-related offenses, an 8 percent of a standard deviation reduction in property-damage offenses, and an 11 percent of a standard deviation reduction in paternity suits. Each of these results is robust to Poisson regression and negative binomial regression as alternative functional forms. Because type I errors are expected to occur 5 percent of the time at the $p < 0.05$ threshold, we do not expect that

the three statistically significant results from our preferred model are merely type I errors. Because we employed four analytic models with ten outcomes each, we can be reasonably confident that six of the eight statistically significant results found in tables 3 and 4 are not type I errors. Our hypothesis 1, that exposure to private schooling through a choice program reduces subsequent risky behavior, is confirmed for three of our ten outcome measures. For the other seven measures, we are left with uncertainty regarding whether MPCP exposure had an effect.

Table 3. Effects of the MPCP on character, four different statistical models, part 1 of 2

	(1) Felonies	(2) Misdem	(3) Drugs	(4) Property	(5) Disorder
1. MPCP	-0.023	-0.042	-0.065***	-0.011**	-0.007
	(0.524)	(0.376)	(0.007)	(0.041)	(0.641)
R-squared	0.0002	0.0005	0.0036	0.0017	0.0001
N	2,178	2,178	2,178	2,178	2,178
2. MPCP	-0.007	-0.032	-0.058***	-0.011**	-0.005
	(0.846)	(0.473)	(0.009)	(0.039)	(0.735)
R-squared	0.0629	0.0666	0.0560	0.0152	0.0207
N	2,178	2,178	2,178	2,178	2,178
3. MPCP	-0.022	-0.058	-0.096***	-0.007	-0.008
	(0.614)	(0.250)	(0.001)	(0.136)	(0.624)
R-squared	0.0637	0.0824	0.0596	0.0142	0.0364
N	1,385	1,385	1,385	1,385	1,385
4. MPCP	-0.024	-0.042	-0.099***	-0.007	-0.009
	(0.589)	(0.417)	(0.001)	(0.235)	(0.613)
R-squared	0.0744	0.0966	0.0652	0.0174	0.0485
N	1,385	1,385	1,385	1,385	1,385

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects. All models use ordinary least squares regression with robust standard errors clustered by census tract. Model 1 does not include control variables. Models 2 and 3 include student controls for race, gender, grade, and baseline math and reading test scores. Model 4 includes student controls and parental income, parental education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables are available from the authors by request. Statistically significant results are robust to Poisson regression and negative binomial regression functional forms. MPCP is Milwaukee Parental Choice Program.

Table 4. Effects of the MPCP on character, four different statistical models, part 2 of 2

	(1) Battery	(2) Thefts	(3) Traffic	(4) Fines	(5) Paternity
1. MPCP	-0.009	0.004	-0.112	-45.737	-0.042**
	(0.324)	(0.822)	(0.218)	(0.602)	(0.016)
R-squared	0.0005	0.0000	0.0010	0.0002	0.0031
N	2,178	2,178	2,178	2,178	2,178
2. MPCP	-0.010	0.007	-0.094	-8.599	-0.042**
	(0.306)	(0.695)	(0.292)	(0.920)	(0.015)
R-squared	0.0104	0.0112	0.0278	0.0472	0.0127
N	2,178	2,178	2,178	2,178	2,178
3. MPCP	-0.005	0.016	-0.133	-23.182	-0.042*
	(0.686)	(0.417)	(0.131)	(0.840)	(0.053)
R-squared	0.0068	0.0110	0.0301	0.0451	0.0116
N	1,385	1,385	1,385	1,385	1,385
4. MPCP	-0.008	0.017	-0.093	-27.918	-0.034
	(0.561)	(0.391)	(0.283)	(0.830)	(0.138)
R-squared	0.0124	0.0196	0.0397	0.0556	0.0183
N	1,385	1,385	1,385	1,385	1,385

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects. All models use ordinary least squares regression with robust standard errors clustered by census tract. Model 1 does not include control variables. Models 2 and 3 include student controls for race, gender, grade, and baseline math and reading test scores. Model 4 includes student controls and parental income, parental education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables are available from the authors by request. Statistically significant results are robust to Poisson regression and negative binomial regression functional forms. MPCP is Milwaukee Parental Choice Program.

Statistically significant control variables behave as expected. Females are less likely to commit all types of crimes and are assessed less in fines than males. The number of paternity suits, however, does not vary by gender. Blacks are more likely to commit crimes but do not receive higher fines and do not differ in their frequency of paternity suits than whites, on average. Asians are generally less likely to commit crimes than whites and are less likely to face paternity suits. Students with higher baseline achievement commit fewer crimes and are assessed less in fees but are similar to students with lower achievement levels in paternity suit frequency.

The statistically significant parent-level control variables indicate that more-advantaged students are less likely to engage in risky behaviors. Students from families with higher incomes, higher levels of parental education, and two-parent households are less likely to commit various crimes but do not differ on their number of paternity suits, all else being equal.

VIII. Heterogeneous Effects

We now explore the possibility of heterogeneous effects of the MPCP by initial student characteristics. We interact indicator variables for membership in various student subgroups with the indicator variable for exposure to the MPCP. Doing so allows us to calculate the effect of MPCP exposure on crime and paternity outcomes for specific subgroups of students while simultaneously signaling which of those subgroup effects, if any, are truly heterogeneous. For example, the effect of the MPCP on a specific crime outcome might be statistically significant for the subgroup of males and not for the subgroup of females, but the difference in those two subgroup effects itself might not be statistically significant. In such cases, we can say with confidence that the program significantly reduced the criminal outcome for males, but we cannot say with confidence that the effect of the program was different for males than for females.

Generally, gender and initial math ability appear to be stronger sources of heterogeneity in the effects of the MPCP on risky behavior than initial reading levels. In our preferred model with student-level controls (tables 5–8), males exposed to the MPCP commit 0.12 fewer drug-related offenses, 0.02 fewer property-damage offenses, and are listed in 0.05 fewer paternity suits than their MPS male peers. The difference between the effect of the MPCP on males and its effect on females is statistically significant for drug and property damage convictions largely because females experienced little to no reduction in convictions for those crimes due to MPCP exposure, while the effects for males were substantial. Females exposed to the MPCP experience 0.04 fewer paternity suits than their MPS peers, a 34 percent reduction from the mean.

This finding is consistent with, but somewhat smaller than, the experimental finding that females who won a charter school lottery in the Harlem Children's Zone were 59 percent less likely to experience a teen pregnancy than females who lost the lottery (Dobbie and Fryer 2015). As expected, the reductions in paternity suits for males and

females due to the MPCP are not statistically different from one another. Students exposed to the MPCP experience about the same decline in paternity disputes regardless of their gender. Finally, the effect of exposure to the MPCP on misdemeanor convictions is different for males compared to females, but we cannot say with confidence that the MPCP reduced misdemeanors for either of those gender subgroups when compared to their subgroup peers.

Both the higher and lower baseline achievement subgroups demonstrate statistically significant effects of the MPCP on reducing negative behavioral outcomes compared to their MPS subgroup peers—four based on initial reading levels and four based on baseline math levels. For reading, however, in only one case was the effect of the MPCP on crime reduction significantly different due to student baseline achievement subgroup. Students in the higher reading subgroup at baseline experienced a large reduction in total criminal fines, averaging nearly $200, compared to their similarly higher-reading MPS peers. That subgroup effect of the MPCP was significantly different from the program's effect on total fines for the lower reading subgroup, which was positive (an average increase of $111) but not statistically significant.

Initial math ability was as strong a source of heterogeneity in MPCP effects as was gender. For two outcomes, thefts and traffic offenses, exposure to the MPCP had a significantly greater effect on reducing negative behavioral outcomes for the higher-performing baseline math subgroup than for the lower-performing one. Only in the case of paternity disputes did the subgroup results based on initial math performance play out as we hypothesized, as students with lower initial math ability experienced a significantly larger reduction in paternity suits due to the MPCP than did students with higher initial math ability. When we combine gender interactions with initial ability interactions, we see that gender differences appear to drive the results. The subgroups become individually very small, as the total sample is divided into four subgroups; however, males with lower baseline math scores clearly experienced a larger reduction in drug offenses due to exposure to the MPCP than did females with higher baseline math scores. Conversely, males with higher baseline math achievement realized a significantly larger reduction in drug crimes

due to the MPCP than females with lower baseline math achievement.[2]

Table 5. Heterogeneous effects, student controls, full sample (part 1 of 2)

	(1) Felonies	(2) Misdem	(3) Drugs	(4) Property	(5) Disorder
Male	-0.029	-0.071	-0.124***	-0.024**	0.003
	(0.690)	(0.440)	(0.010)	(0.030)	(0.921)
Female	0.012	-0.000	-0.003	0.000	-0.012
	(0.421)	(0.996)	(0.629)	(0.981)	(0.344)
Difference	-0.041	-0.071*	-0.121**	-0.024**	0.015
	(0.580)	(0.093)	(0.011)	(0.032)	(0.660)
Low read	-0.022	-0.060	-0.082**	-0.014	-0.019
	(0.705)	(0.395)	(0.041)	(0.118)	(0.448)
High read	-0.015	-0.024	-0.045**	-0.008	0.003
	(0.671)	(0.551)	(0.045)	(0.209)	(0.813)
Difference	-0.007	-0.036	-0.037	-0.007	-0.022
	(0.920)	(0.650)	(0.429)	(0.538)	(0.439)
Low math	-0.025	-0.036	-0.097***	-0.017*	0.003
	(0.626)	(0.562)	(0.009)	(0.076)	(0.881)
High math	-0.010	-0.053	-0.024	-0.004	-0.025
	(0.800)	(0.229)	(0.312)	(0.407)	(0.144)
Difference	-0.015	0.017	-0.073	-0.013	0.028
	(0.822)	(0.808)	(0.102)	(0.246)	(0.327)
N	2,178	2,178	2,178	2,178	2,178

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects for the defined subgroup. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. Coefficients for control variables are available from the authors by request. "Low reading" and "low math" refer to students with baseline test scores at or below the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. Statistically significant results are robust to Poisson regression and negative binomial regression. The null result for property damage crime reduction for students with low reading scores is statistically significant at the $p < 0.10$ level when negative binomial regression is used.

[2] The pattern of effect heterogeneity by gender, reading, and math ability is substantively similar to these model 2 results when that model with student controls is estimated on the smaller sample (i.e., our model 3). Those results are available from the authors by request.

Table 6. Heterogeneous effects, student controls, full sample, part 2 of 2

	(1) Battery	(2) Thefts	(3) Traffic	(4) Fines	(5) Paternity
Male	-0.017	-0.002	-0.126	-67.074	-0.045**
	(0.272)	(0.950)	(0.348)	(0.698)	(0.026)
Female	-0.004	0.014	-0.067	39.586	-0.040*
	(0.710)	(0.329)	(0.488)	(0.475)	(0.078)
Difference	-0.013	-0.016	-0.059	-106.66	-0.005
	(0.469)	(0.656)	(0.689)	(0.549)	(0.875)
Low read	-0.008	0.004	-0.090	111.601	-0.056***
	(0.563)	(0.861)	(0.501)	(0.371)	(0.007)
High read	-0.012	0.001	-0.131	-197.450**	-0.034
	(0.322)	(0.971)	(0.217)	(0.038)	(0.201)
Difference	0.004	0.004	0.041	309.10**	-0.022
	(0.828)	(0.893)	(0.812)	(0.042)	(0.478)
Low math	-0.020	0.025	0.021	68.686	-0.071***
	(0.131)	(0.305)	(0.860)	(0.517)	(0.002)
High math	0.004	-0.026	-0.279**	-164.142	-0.013
	(0.734)	(0.111)	(0.020)	(0.158)	(0.597)
Difference	-0.024	0.051*	0.300*	232.83	-0.059*
	(0.170)	(0.064)	(0.063)	(0.121)	(0.059)
N	2,178	2,178	2,178	2,178	2,178

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects for the defined subgroup. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. Coefficients for control variables are available from the authors by request. "Low reading" and "low math" refer to students with baseline test scores at or below the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. Statistically significant results are robust to Poisson regression and negative binomial regression.

Table 7. Heterogeneous effects, student controls, full sample, math level and gender, part 1 of 2

	(1) Felonies	(2) Misdem	(3) Drugs	(4) Property	(5) Disorder
LM male	-0.073	-0.078	-0.208***	-0.035*	0.015
	(0.498)	(0.507)	(0.009)	(0.074)	(0.736)
HM female	-0.001	0.007	-0.004	0.004	-0.019
	(0.957)	(0.815)	(0.655)	(0.431)	(0.225)
Difference	-0.072	-0.085	-0.204*	-0.039	0.033
	(0.609)	(0.717)	(0.075)	(0.497)	(0.572)
LM female	0.027	0.013	-0.008	0.000	-0.003
	(0.234)	(0.717)	(0.193)	(0.857)	(0.889)
HM male	-0.027	-0.135	-0.052	-0.015	-0.034
	(0.761)	(0.170)	(0.274)	(0.183)	(0.307)
Difference	0.054	0.148	0.060*	0.015	0.031
	(0.609)	(0.717)	(0.075)	(0.497)	(0.572)
N	2,178	2,178	2,178	2,178	2,178

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects for the defined subgroup. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. Coefficients for control variables are available from the authors by request. "LM" is "low math" and "HM" is "high math." "Low math" refers to students with baseline test scores at or below the fiftieth percentile. "High math" refers to students with baseline test scores above the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. Statistically significant results are robust to Poisson regression and negative binomial regression.

Table 8. Heterogeneous effects, student controls, full sample, math level and gender, part 2 of 2

	(1) Battery	(2) Thefts	(3) Traffic	(4) Fines	(5) Paternity
LM male	-0.041*	0.025	0.085	37.702	-0.069**
	(0.066)	(0.557)	(0.681)	(0.859)	(0.015)
HM female	-0.003	-0.006	-0.137	-59.891	-0.005
	(0.854)	(0.608)	(0.279)	(0.275)	(0.866)
Difference	-0.039	0.032	0.222	97.593	-0.063
	(0.104)	(0.401)	(0.218)	(0.603)	(0.752)
LM female	-0.001	0.025	-0.034	106.151	-0.075**
	(0.958)	(0.318)	(0.764)	(0.176)	(0.044)
HM male	0.013	-0.052	-0.461**	-302.485	-0.021
	(0.486)	(0.101)	(0.025)	(0.222)	(0.487)
Difference	-0.013	0.077	0.427	408.636	-0.054
	(0.104)	(0.401)	(0.218)	(0.603)	(0.752)
N	2,178	2,178	2,178	2,178	2,178

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects for the defined subgroup. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. Coefficients for control variables are available from the authors by request. "LM" is "low math" and "HM" is "high math." "Low math" refers to students with baseline test scores at or below the fiftieth percentile. "High math" refers to students with baseline test scores above the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. Statistically significant results are robust to Poisson regression and negative binomial regression. The null result for fine reduction for males with high math scores is statistically significant at the $p < 0.10$ level when negative binomial regression is used.

The pattern of heterogeneity in our results based on gender and initial achievement is similar when parent-level controls are added to the statistical model (tables 9–12). Where there are differences in the crime-suppressing effects of exposure to the MPCP based on gender, males realize a greater benefit than females. The only difference in the effects of the MPCP based on initial reading ability is a bigger reduction in total criminal fines experienced by the higher baseline reading group than by the lower one. Lower math achievers at baseline experience a larger reduction in drug crimes and paternity suites due to exposure to the MPCP than do higher math achievers at

baseline. When gender and initial math ability indicators are used to parse the sample into four different subgroups, being male more consistently leads to a bigger reduction in negative behavioral outcomes from experiencing the MPCP than does being a lower math performer at baseline. All statistically significant results are robust to Poisson regression and negative binomial regression except one: the result for property-damage crimes is only robust to ordinary least squares regression.

Table 9. Heterogeneous effects, all controls, part 1 of 2

	(1) Felonies	(2) Misdem	(3) Drugs	(4) Property	(5) Disorder
Male	-0.062	-0.074	-0.211***	-0.019*	0.001
	(0.508)	(0.472)	(0.002)	(0.068)	(0.985)
Female	0.007	-0.016	-0.011	0.003	-0.016
	(0.739)	(0.637)	(0.253)	(0.394)	(0.249)
Difference	-0.069	-0.058	-0.199***	-0.022**	0.017
	(0.466)	(0.579)	(0.003)	(0.028)	(0.665)
Low read	-0.040	-0.034	-0.120**	-0.003	-0.022
	(0.576)	(0.708)	(0.029)	(0.741)	(0.447)
High read	-0.032	-0.073*	-0.096***	-0.012*	0.000
	(0.511)	(0.056)	(0.001)	(0.057)	(0.989)
Difference	-0.007	0.040	-0.024	0.010	-0.022
	(0.932)	(0.684)	(0.694)	(0.329)	(0.483)
Low math	-0.068	-0.072	-0.155***	-0.012*	-0.012
	(0.318)	(0.374)	(0.005)	(0.081)	(0.668)
High math	0.005	-0.028	-0.048*	-0.001	-0.011
	(0.924)	(0.514)	(0.075)	(0.888)	(0.456)
Difference	-0.073	-0.044	-0.107*	-0.011	0.000
	(0.383)	(0.623)	(0.084)	(0.303)	(0.989)

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. All models also control for parental income, education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables are available from the authors by request. "Low reading" and "low math" refer to students with baseline test scores at or below the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. All statistically significant results are robust to Poisson regression and negative binomial regression except one: the result for misdemeanors for low-math males is only robust to ordinary least squares regression.

Table 10. Heterogeneous effects, all controls, part 2 of 2

	(6) Batteries	(7) Thefts	(8) Traffic	(9) Fines	(10) Paternity
Male	-0.008	0.021	-0.162	-133.197	-0.045
	(0.684)	(0.566)	(0.256)	(0.610)	(0.123)
Female	-0.008	0.015	-0.038	54.535	-0.024
	(0.637)	(0.425)	(0.708)	(0.549)	(0.434)
Difference	0.001	0.006	-0.124	-187.73	-0.021
	(0.970)	(0.878)	(0.470)	(0.481)	(0.611)
Low read	0.006	0.022	-0.092	163.641	-0.056*
	(0.750)	(0.542)	(0.519)	(0.357)	(0.059)
High read	-0.024	0.002	-0.108	-298.418*	-0.015
	(0.145)	(0.827)	(0.345)	(0.063)	(0.639)
Difference	0.031	0.019	0.017	462.08**	-0.041
	(0.214)	(0.610)	(0.931)	(0.043)	(0.300)
Low math	-0.012	0.031	0.028	50.479	-0.070**
	(0.489)	(0.335)	(0.839)	(0.741)	(0.039)
High math	-0.004	-0.011	-0.264**	-194.072	0.007
	(0.838)	(0.378)	(0.020)	(0.289)	(0.817)
Difference	-0.008	0.042	0.293	244.55	-0.076*
	(0.746)	(0.196)	(0.126)	(0.273)	(0.081)

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. All models also control for parental income, education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables are available from the authors by request. "Low reading" and "low math" refer to students with baseline test scores at or below the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. All statistically significant results are robust to Poisson regression and negative binomial regression except one: the result for misdemeanors for low math males is only robust to ordinary least squares regression.

Table 11. Heterogeneous effects, all controls, math level and gender, part 1 of 2

	(1) Felonies	(2) Misdem	(3) Drugs	(4) Property	(5) Disorder
LM male	-0.153	-0.108	-0.316***	-0.025*	0.014
	(0.279)	(0.502)	(0.005)	(0.071)	(0.808)
HM female	-0.004	0.009	-0.010	0.007	0.004
	(0.881)	(0.823)	(0.503)	(0.345)	(0.774)
Difference	-0.148	-0.117	-0.306*	-0.032	0.011
	(0.271)	(0.970)	(0.087)	(0.798)	(0.289)
LM female	0.022	-0.020	-0.003	0.000	-0.028
	(0.408)	(0.696)	(0.760)	(0.881)	(0.202)
HM male	0.010	-0.086	-0.108**	-0.013	-0.035
	(0.927)	(0.282)	(0.045)	(0.405)	(0.234)
Difference	0.012	0.066	0.105*	0.013	0.006
	(0.271)	(0.970)	(0.087)	(0.798)	(0.289)
N	1,385	1,385	1,385	1,385	1,385

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. All models also control for parental income, education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables are available from the authors by request. LM is low math and HM is high math. "Low math" refers to students with baseline test scores at or below the fiftieth percentile. "High math" refers to students with baseline test scores above the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. All statistically significant results are robust to Poisson regression and negative binomial regression except one: the result for misdemeanors for low-math males is only robust to ordinary least squares regression.

Table 12. Heterogeneous effects, all controls, math level and gender, part 2 of 2

	(6) Batteries	(7) Thefts	(8) Traffic	(9) Fines	(10) Paternity
LM male	-0.026	0.025	0.148	-11.038	-0.080*
	(0.357)	(0.686)	(0.558)	(0.970)	(0.065)
HM female	-0.019	-0.021	-0.027	-67.114	0.015
	(0.480)	(0.142)	(0.825)	(0.437)	(0.718)
Difference	-0.007	0.045	0.175*	56.077	-0.094
	(0.148)	(0.625)	(0.055)	(0.719)	(0.996)
LM female	0.001	0.041	-0.080	116.249	-0.063
	(0.949)	(0.148)	(0.582)	(0.318)	(0.219)
HM male	0.016	0.000	-0.585***	-373.787	-0.003
	(0.430)	(0.983)	(0.003)	(0.354)	(0.934)
Difference	-0.015	0.041	0.505*	490.035	-0.060
	(0.148)	(0.625)	(0.055)	(0.719)	(0.996)
N	1,385	1,385	1,385	1,385	1,385

Notes: *P*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Results are average marginal effects. All models use ordinary least squares regression with robust standard errors clustered by census tract. All models control for student race, gender, grade, and baseline math and reading test scores. All models also control for parental income, education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables are available from the authors by request. LM is low math and HM is high math. "Low math" refers to students with baseline test scores at or below the fiftieth percentile. "High math" refers to students with baseline test scores above the fiftieth percentile. "Difference" indicates the difference between the coefficients located in the two preceding rows. Subgroup effects and differences are shaded in gray if the subgroup effects themselves are significantly different from each other. All statistically significant results are robust to Poisson regression and negative binomial regression except one: the result for misdemeanors for low-math males is only robust to ordinary least squares regression.

IX. Conclusion

Our results suggest that private school choice is associated with either equal or better demonstrated character skills in the long run. Students who participated in the MPCP are less likely to commit drug and property crimes and experience paternity suits than their peers in MPS, all else being equal. We conducted ten statistical tests of hypothesis 1, that even limited exposure to the MPCP would reduce negative behavioral outcomes of young adults. Three of those tests permitted us to reject the null hypothesis of no significant effect, while the other seven did not allow us to reject the null. Our results regarding heterogeneity in those effects based on gender and initial academic ability were more mixed.

Our results generally confirmed hypothesis 2a, that males would experience larger effects than females. Those results generally did not confirm hypothesis 2b, that lower initial achievers would experience larger effects than higher initial achievers. When initial test score performance mattered, and it seldom did in the case of reading scores, study participants in the lower baseline achievement group sometimes experienced larger effects from the program, as we hypothesized, but participants in the higher baseline achievement group also sometimes benefited more from the school choice experience. When gender and initial math performance were both factored into generating subgroup comparisons, males consistently benefited more than females from exposure to the MPCP, whether they were lower or higher performing at baseline. Our results do not confirm hypothesis 2c—that male lower performers will demonstrate the largest programmatic effects of any student subgroup.

An important limitation of our study is that the students examined in the longitudinal evaluation of the program were not randomly assigned vouchers to attend private schools. If our baseline matching procedure does not fully establish equivalence on important unobservable characteristics that predict subsequent risky behavior, our results may be merely correlational rather than causal. However, we use a sophisticated matching procedure shown to replicate experimental results in other school choice evaluations (Bifulco 2012). In addition, the baseline differences that we observe signal both positive and negative selection into the MPCP, meaning that the direction of the overall selection bias, if any exists, is unclear.

Much more research on this topic is needed. Only three evaluations of public school choice examine the important outcome of criminal activity. This study is only the second evaluation linking private school choice to adult criminal activity and the first analysis connecting private school choice to paternity suits. Only two of the evaluations linking public school choice to crime use random assignment, and there are no random-assignment studies that connect private school choice to criminal outcomes.

Furthermore, both evaluations linking private school choice to adult crime examine the MPCP, which is a voucher program that differs from other school choice programs in a few important ways. It is the longest-standing modern-day voucher program in the United States. It is one of the most heavily regulated choice programs in the country (Stuit and Doan 2013). It is in a large urban area that experiences high crime rates relative to the rest of the country, and

students are admitted to private schools before they apply for the voucher. For these reasons, the results observed in this study should not be extrapolated with high confidence to other locations. Additional studies of private school choice programs that are different than the MPCP are needed before we can conclude that choice consistently reduces drug crimes, property crimes, and paternity suits. The specific mechanisms by which school choice interventions reduce criminal behavior and out-of-wedlock births for young adults also should be explored. Here, at least we have made a start.

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