# UNIVERSITY OF ARKANSAS.

## College of Education & Health Professions
### Education Reform

# WORKING PAPER SERIES

## Private School Choice and Character: More Evidence from Milwaukee

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February 26, 2019

EDRE Working Paper 2019-03

# Private School Choice and Character: More Evidence from Milwaukee*

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February 26, 2019

Final for release

**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 8th or 9th grade predicts lower rates of conviction for criminal activity and lower rates of paternity suits by ages 25 to 28. 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.


*Keywords:* bottom-up reform; school violence; character education; school voucher; private schooling; religious schooling; school choice

*JEL Classifications:* I28, I20

**Introduction**

Private school choice programs are government initiatives that directly or indirectly provide financial support that allows parents to enroll their child in a private school of choice. 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 29 states plus the District of Columbia, enrolling over 482,000 students in 2018-19 (EdChoice, 2019).[1]

Most evaluations of private school choice programs have examined their effects on standardized test scores. A recent meta-analysis of 19 experimental studies of 11 different programs around the world finds that private school vouchers have null or small positive effects on student achievement (Shakeel, Anderson, & Wolf, 2016). However, test-score outcomes vary significantly across evaluations 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 were published prior to 2012. They tilt negatively in studies that are quasi-experimental, focused on math, and were published after 2012 (Wolf & Egalite, 2018). Recent experimental evaluations report negative effects on math scores in the first two years of the D.C. Opportunity Scholarship Program (Dynarski et al., 2017; Dynarski et al., 2018) and negative effects on both math and reading scores in the first two years of the Louisiana Scholarship Program (Mills, 2015; Mills & Wolf, 2017a; Abdulkadiroglu, Pathak, & Walters, 2018) which turned to null after three years (Mills & Wolf, 2017b). Two recent quasi-

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[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 expenses because they are not "programs" per se.

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experimental evaluations find mostly negative effects of voucher programs on student test scores in Ohio and Indiana over four-year periods (Figlio & Karbownik, 2016; Waddington & Berends, 2018).

Standardized test scores, however, do not fully capture society's goals for education (Macedo & Wolf, 2004; Zimmer et al., 2009). Tests measure the effects of schools and teachers on the cognitive performance of students; however, schools are also social institutions that aim to improve non-cognitive skills such as grit, persistence, conscientiousness, and social functioning (Arthur & Davidson, 2000; Duckworth, Peterson, Matthews, & Kelly, 2007; Egalite, Mills, & Greene, 2016; Hitt, Trivitt, & Cheng, 2016). While some studies find a link between teachers' effects on standardized tests and their effects on long-term outcomes (Chetty, Friedman, & Rockoff, 2014), two recent reviews of the literature find that the effects of school choice interventions, schools 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, 2018; Hitt, McShane, & Wolf, 2018). Improving student non-cognitive character skills can lead to higher levels of educational attainment and better life outcomes as measured by lifetime earnings, employment, and citizenship (Reynolds, Temple, & Ou, 2010).

Do private school choice programs affect students' character skills? In theory, private school choice 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

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(MPCP) – on adult criminal activity and paternity suits. We find that exposure to the program in 8th or 9th grade is associated with lower rates of conviction for criminal activity and lower rates of paternity suits by the time students are 25 to 28 years old. The benefits associated with program participation tend to be largest for males and students with lower levels of academic achievement at baseline.

**Theory**

Schools should teach people to be responsible citizens, increase social cohesion, and boost democratic participation (Mann, 1855; Dewey, 1916; Tooley, 2000). Throughout U.S. history, one of the main arguments for allocating additional resources to schooling is to reduce crime (West, 1965). Additional years of educational attainment improve the economic prospects of young adults, providing them with an incentive to stay out of trouble (Rouse, 2005). Crimes have large negative impacts on society. McCollister, French, and Fang (2010) find that each instance of vandalism and robbery costs society $5,457 and $47,500, respectively, in 2016 U.S. dollars. Access to higher quality schools, or more school choices in general, could dissuade young adults from engaging in risky behavior.

Private school choice might improve character skills. Many parents expect schools to shape positively the character of their children (Zeehandelaar & Winkler, 2013; Stewart & Wolf, 2014; Erickson, 2019). When families choose their children's school, competitive pressure from the fear of losing students may provide an additional incentive for schools to develop the non-cognitive skills of students that parental customers desire (Chubb & Moe, 1988; Friedman, 1997; DeAngelis & Erickson, 2018).

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What specific mechanisms might schools of choice use to enhance character skills? Schools of choice involve voluntary associations of people attracted by a common set of values who can build social capital and a strong sense of community (Coleman & Hoffer, 1987; Hill, Foster, & Gendler, 1990; Brandl, 1998). Such "voluntary associations not only generate 'social capital'…they *presuppose* it." (Berkowitz, 1996, 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 & Buchanan, 2014; DeAngelis & Burke, 2017). Students interested in the distinctive mission of their schools and its curricula may be less likely to engage in risky behaviors out of boredom (Wurmser, 1974).

Religious schools have explicit moral commitments to serve the community and develop student character (Bellah et al., 1985; Bryk, Lee, & Holland, 1993; Johnson, 2011; Jeynes, 2012). Although sectarian private schools participating in choice programs tend to be funded at lower levels than neighboring public schools (Wolf & McShane, 2013; Egalite, 2015; Lueken 2018), "sectarian schools are communities generating and dispensing inspiration and nurture that accomplishes much more that money cannot buy." (Brandl, 2006, 32). Since most schools participating in choice programs are sectarian (e.g. Sude, DeAngelis, & 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.

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Finally, because private schools are typically located in more-affluent and lower-crime areas, access to private school choice could decrease risky behaviors simply by relocating students away from negative influences (DeAngelis & Dills, 2018). Peer pressure at more-advantaged schools may discourage the negative activities of students (Akerlof & Kranton, 2002). Similarly, police may be more familiar with rebellious students in public schools simply because public schools are more likely than private schools to have police officers on campus (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 if more tempting. As with males, 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 outcomes than
   students with higher levels of academic achievement at baseline; and,

   c. Male lower-performers will demonstrate the largest programmatic effects
   of any student subgroup.

## Literature Review

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

Few studies of private school choice examine its effects on outcomes besides test scores.
Five of six evaluations of the attainment effects of private school choice find that choice
increases rates of high school graduation and college enrollment (Cowen et al., 2013; Wolf et al.,
2013; Chingos & Peterson, 2015; Chingos & Kuehn, 2017; Chingos, 2018; Wolf, Witte, &
Kisida, 2018). In a review, DeAngelis (2017) identifies eleven studies indicating that school
choice program participation had null to positive effects on political participation, volunteering,
and charitable giving (e.g. Bettinger & Slonim, 2006; Campbell, 2008; Fleming, 2014;
DeAngelis & Wolf, 2018). In his review of 21 studies, Wolf (2007) finds generally positive
effects of both private and public school choice on the civic outcomes of students. Swanson's

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(2017) review of eight U.S. studies reports that school-level racial integration is either unaffected or improved by private school choice.

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 & 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 were 5.1 percentage points lower (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.

Most studies focusing on schooling impacts on criminal activity ignore school choice, instead evaluating the crime effects of drop-out rates and broad schooling laws (Luallen, 2006; Lochner, 2010; Anderson, 2014). Other studies examine school desegregation's impacts on crime (Weiner, Lutz, & Ludwig, 2009; Billings, Deming, & Rockoff, 2013), or how educational attainment can affect criminal activity (Machin, Marie, & Vujić, 2011). These evaluations find that higher levels of education do lead to lower crime rates, however, these same studies do not examine the effects of school type. School choice studies tend to ignore crime and crime studies tend to ignore school choice.

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Only five studies examine the intersection of school choice and crime. In a study of Charlotte-Mecklenburg County, North Carolina, in 2002, Deming (2011) compares the crime rates of the students who won charter school lotteries to the rates of the students who lost lotteries. He finds that winning a charter school lottery significantly decreases the likelihood of a high-risk student committing a crime. Dobbie and Fryer (2015) perform a similar experimental evaluation and find that winning a lottery to attend a public charter school in the Harlem Children's Zone eliminates the chance of incarceration for males while reducing the likelihood of a teen pregnancy by 59 percent for females. Dills and Hernández-Julián (2011) use national data to determine that a one standard deviation increase in residential school choice is associated with a reduction in juvenile crime of about 40 percent. Brinig and Garnett (2014) examine the systemic effect of Catholic school closings on crime rates in communities, finding that crime tends to increase when Catholic schools in urban areas shut their doors. In contrast, the increased availability of non-religious schools of choice, specifically public charter schools, has no significant effect on crime in the inner-city, they determine. Brinig and Garnett (2014) argue for increased access to private school choice programs to allow more Catholic schools to generate positive communal effects on crime reduction in American cities.

Only one study exists of the effect of a private school choice program on the criminal behavior of young adults (DeAngelis & Wolf, 2016). 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 age 22 to 25. 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

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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 the previous study in at least five important ways: (1) we look up the cumulative record of risky behaviors over three years later than the original study – in the fall of 2018 – when the students were 25 to 28 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 use a more holistic approach with more analytic power; (4) we include additional categories of outcomes such at the total amount of 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 based on gender and initial academic ability.

We proceed by describing the voucher program on which our evaluation is based and the data and analytical procedures we employ. Next, we present the results from the estimation of statistical models that predict different types of character outcomes, including the role of private schooling through the MPCP. We conclude with a discussion of what our results mean for future research on school choice.

**Description of the Program**

The MPCP launched in 1990 as a pilot program to test the concept of private school vouchers for low-income urban students. Program enrollment was capped at 1.5 percent of Milwaukee Public Schools (MPS) enrollment, or about 500 students, and only seven non-religious private schools

were allowed to participate (Witte et al., 2008). Starting in 1996, the enrollment cap was raised repeatedly, until it was eliminated in 2012. Religious schools were permitted to enroll voucher students starting in 1998. These policy decisions, allowing both demand and supply to grow, resulted in the program enrolling 25 percent of all K-12 students in Milwaukee by 2014-15. Charter schooling and open enrollment programs also spread throughout the city. Perhaps because of this proliferation of public and private school choice in Milwaukee, research indicates that competitive pressures in the Milwaukee education sector have led to better test score outcomes for children left behind in traditional public schools (Greene & Forster, 2002; Hoxby, 2003; Carnoy et al., 2007; Chakrabarti, 2008; Greene & Marsh, 2009). These positive competitive effects of school choice may explain why the latest evaluation of the program found only limited evidence of test score gains for actual voucher participants (Witte et al., 2012; 2014).

The MPCP is a government-run school voucher program. To qualify, applicants have to be entering grades K-12 and reside in the city of Milwaukee. Prior to 2012, students had to have a family income at or below 175 percent of the poverty level, an amount slightly below the federal lunch program ceiling, in order to qualify for the program. That same year, the income ceiling was raised to 300 percent of poverty. The students in our study, who all joined the program before the income eligibility ceiling was raised, tend to be disadvantaged relative to the average MPS student regarding family income and initial test scores but advantaged relative to their public school peers in their parent's level of education (Fleming, Cowen, Witte & Wolf, 2015).

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Students first enroll in a participating private school of their choosing and then, through the school, apply to the Wisconsin Department of Public Instruction for tuition assistance. This sequencing of events – choice of school first and voucher second – distinguishes the MPCP from other voucher programs in Cleveland, Ohio; Washington, DC; as well as the states of Indiana and Ohio, where students first are awarded vouchers and then choose their private school.

In the baseline study year of 2006, the voucher was worth up to $6,501, about 40 percent less than the average per pupil expenditure in MPS (Costrell, 2008). By 2011, the final year of the original evaluation that produced these data, the voucher maximum had been cut to $6,442 or 57 percent less than the average per pupil expenditure in MPS (McShane et al., 2012). Financial reports indicate that the private schools subsidized their voucher pupils by an average of $962 per student in 2006 (Kisida, Jensen, & Wolf, 2009) and $1,250 in 2011 (McShane et al., 2012).

In general, private schools participating in the MPCP must not charge tuition above the voucher amount for eligible students; however, since 2012, parents of students in grades 9-12 with an income greater than 220 percent of the federal poverty level may be charged additional tuition above the voucher amount. Participating private schools must administer state standardized tests, be accredited by the state within three years of program participation, allow students to opt out of religious activities, require all teachers and administrators to have a teaching license or a bachelor's degree, and must admit voucher-eligible students on a random basis (EdChoice, 2019).

**Data and Matching Procedure**

Since the Milwaukee program expanded in 1998, vouchers rarely have been assigned to students

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via lottery (Cowen et al., 2013). Although schools in the program must admit students by lottery when a specific grade in a school is oversubscribed, schools tend to recruit voucher-eligible students only until that ceiling is reached. Therefore, an experimental evaluation of the MPCP has not been possible since the expansion.

To generate comparable groups for our analysis, we matched MPCP (i.e. voucher) students with MPS students based on grade, neighborhood, race, gender, English Language Learner (ELL) status as well as math and reading test scores (Witte et al., 2008). The census of 801 MPCP students in 9th grade in the fall of 2006 and a randomly-selected sample of 290 MPCP students in 8th grade that year were combined into a total program sample of 1091. Researchers matched those voucher students to the set of MPS students in their same grade within the same neighborhood census tract who also were in the same 5 percent bandwidth of 2006 test scores. The specific MPS student who served as the match for each MPCP student was selected based on the student's nearest-neighbor propensity score informed by student race, gender, ELL status, and test score.

All but two students in the program sample were successfully matched, resulting in a treatment group of 1089 students exposed to the voucher program in 2006 and a matched group of 1089 highly similar comparison students in MPS in 2006, for a total analytic sample of 2178. The two matched groups of students do not differ regarding most key characteristics. 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 had lower baseline math test scores and came from households with lower income levels than their matched

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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 about here]

Education evaluations employing propensity score matching that prioritize "geography" (i.e. neighborhood) tend to closely replicate experimental results in within-study comparisons (e.g. Heckman, Ichimura, & Todd, 1997; Cook, Shadish, & Wong, 2008; Bifulco, 2012). Census tracts define neighborhoods in Milwaukee. Families who live in the same neighborhood tend to share similar unmeasured factors such as motivation and moral values that, if not balanced across our samples, might bias our quasi-experimental analysis of school choice effects (Ahlbrandt, 2013). The prioritization of neighborhood location in our propensity score matching protocol represents an advance in matching techniques in school choice research that is more likely to capture unmeasured values-based factors that otherwise threaten our study's internal validity.

After students were matched, their parents were surveyed by telephone about important family background data such as income, mother's and father's education, and whether both parents lived in the home (Witte et al., 2008). A total of 69 percent of parents in the sample responded, which is a very high response rate for a telephone survey. The rate for MPCP parents was 73 percent while the rate for MPS parents was 66 percent. Response weights in our analysis correct for any baseline differences between the two groups of respondents. For our more complete model estimations we use the subsample of 1506 students whose parents were survey respondents, in order to control for family characteristics that otherwise might bias our results.

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Our dependent variables were drawn from the Wisconsin Court System Circuit Court Access.[2] 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. The searchers were unaware of each person's status as a member of the MPCP or MPS sample. 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, a misdemeanor, a drug-related offense, property damage, disorderly conduct, battery, theft, or a 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 the severity of the crime. Thus, a single judicial record could produce multiple codings of "1" across the various behavioral indicators, a single "1" or 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 18 or older. Because we searched the database during the fall of 2018, the students were 25-28 years old when we looked up their records, and thus experienced 7 to 10 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.

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[2] Wisconsin Court System Circuit Court Access (2017). Retrieved from
https://wcca.wicourts.gov/simpleCaseSearch.xsl.

Table 2 summarizes our sample of 2178 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. On average, the students were assessed a total of $526 in crime-related fees. With little variation in our dependent variables, it may be difficult to detect actual differences across our comparison groups for most types of crime. In order to reduce the risk of such Type II errors, we include p < .10 as a threshold for marginal statistical significance of any group differences.

[Table 2 about here]

**Methods**

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

$$Outcome_i = \beta_0 + \delta_1 MPCP06_i + \varepsilon_{it} \tag{1}$$

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), $\delta_1$ is the difference associated with exposure to MPCP (enrolled in the MPCP in 2006). Each outcome observation is coded as non-negative integer values for each category besides "total fines" because the data are counts of cases. The category for total fines (in dollars) is also non-negative, but is a continuous variable rather than a count. We obtain robust standard errors of $\varepsilon_{it}$ 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 auto-correlated error terms. As our sample

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of 2,178 students comes from only 194 different census tracts, clustering the errors increases their size, thereby leading to more conservative program effect estimates.

We use an Intent-to-Treat (ITT) approach, as all of the students in the MPCP group are coded "1" for *MPCP* regardless of how long they subsequently 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 non-random 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:

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

where the outcome and MPCP exposure variables, as well as the error term, are the same as described for model 1. Added to this equation are vector X of student race, gender, and baseline grade (8th or 9th) 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 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

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behavior (e.g. academic ability, gender, relative age) while preserving all of the observations in our analytic sample. More extensive statistical models can control for family background variables which 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 non-random subgroup of our sample, adding those variables also risks introducing survey non-response 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 the form:

$$Outcome_i = \beta_0 + \delta_1 MPCP06_i + \beta_1 X_i + \beta_2 test_{2006} + \beta_3 Z_i + \varepsilon_{it} \qquad (3)$$

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. The sample size drops to 1385 in the parental controls models because not all parents responded to the surveys. Because of the count nature of our data, we also use Poisson regression and negative binomial regression as robustness checks for all results.

## Results

Table 3 presents the results for three different statistical models: the MPCP indicator variable with no control variables, the MPCP indicator with student controls, and the MPCP indicator with both student and parental controls. A negative coefficient represents a reduction in criminal convictions or paternity disputes and therefore a beneficial effect of exposure to the private

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school choice program. Exposure to the MPCP is correlated with a reduction in 9 of the 10

negative behavior measures. The only effect estimates that are positively signed are for theft and

none of those three estimates are even close to statistically significant. The effects of the MPCP

on 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 three 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 and paternity suits, at

least in the uncontrolled model and the model with student controls. The effects of the MPCP on

property damage convictions and paternity suits are not statistically significant in the model with

all of the control variables included. That difference appears partly due to a coefficient that is

about one-third smaller regarding property crimes and one-sixth smaller regarding paternity

suits. The main reason why the property damage and paternity effects are non-significant in the

all controls model, however, is that the loss of almost 800 observations leads the standard errors

of those estimates, and therefore the p-values, to increase dramatically.

Our preferred model 2 includes the complete sample with student-level 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

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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. Our Hypothesis 1 that exposure to private schooling through a choice program reduces subsequent risky behavior is confirmed for three of our 10 outcome measures. For the other seven measures we are left with uncertainty regarding whether or not MPCP exposure had an effect.

[Table 3 about here]

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; however, the number of paternity suits does not vary by gender. Blacks are more likely to commit crimes, but do not receive higher fines and do not have more or less paternity suits than whites, on average. Asians are generally less likely to commit crimes than whites, and they are less likely to go through paternity suits. Students with higher baseline achievement commit fewer crimes and are assessed less in fees but do not have more or less paternity suits as students with lower achievement levels.

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, with 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.

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**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 and also signals 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 on males was different than the effect of the program on 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 (Table 4), 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 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 Harlem Children's Zone were 59 percent less likely to experience a teen

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pregnancy than females who lost the lottery (Dobbie & 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

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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.

[Table 4 about here]

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 (Table 5). 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 5 about here]

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**Discussion**

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 10 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.

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

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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 indicate evidence of 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 crime. 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 arguably the most heavily regulated program in the United States (Stuit & Doan, 2013). It is located 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. Research on exactly how and why

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parental school choice may reduce the proclivity of students to engage in such undesirable

behaviors as young adults would also be especially welcome.

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| | MPS in 2006 | MPCP in 2006 | N |
|---|---|---|---|
| Female | 0.53 | 0.57* | 2178 |
| Black | 0.70 | 0.70 | 2178 |
| Hispanic | 0.18 | 0.19 | 2178 |
| Asian | 0.04 | 0.03 | 2178 |
| White | 0.07 | 0.07 | 2178 |
| Grade in 2006 | 8.73 | 8.74 | 2178 |
| Math in 2006 | 0.04* | -0.03 | 2178 |
| Reading in 2006 | 0.02 | 0.13*** | 2178 |
| Parent Completed College | 0.12 | 0.16** | 1506 |
| Parent Some College | 0.31 | 0.35 | 1506 |
| Income over 50k | 0.17*** | 0.05 | 1401 |
| Income under 25k | 0.54*** | 0.59 | 1401 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

| Variable | N | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| *Student Characteristics* | | | | | |
| **MPCP 2006** | 2178 | 0.50 | 0.50 | 0 | 1 |
| **Black** | 2178 | 0.70 | 0.46 | 0 | 1 |
| **Hispanic** | 2178 | 0.18 | 0.39 | 0 | 1 |
| **Asian** | 2178 | 0.04 | 0.19 | 0 | 1 |
| **White** | 2178 | 0.07 | 0.26 | 0 | 1 |
| **Female** | 2178 | 0.55 | 0.50 | 0 | 1 |
| **Grade in 2006** | 2178 | 8.74 | 0.44 | 8 | 9 |
| **Math Z Score** | 2178 | 0.00 | 0.87 | -3.13 | 3.00 |
| **Read Z Score** | 2178 | 0.07 | 0.90 | -2.97 | 2.54 |
| *Parent Characteristics* | | | | | |
| **Income>50** | 1401 | 0.11 | 0.31 | 0 | 1 |
| **35<Income<50** | 1401 | 0.14 | 0.35 | 0 | 1 |
| **25<Income<35** | 1401 | 0.18 | 0.39 | 0 | 1 |
| **Income>25** | 1404 | 0.31 | 0.46 | 0 | 1 |
| **Parent HS Grad** | 1506 | 0.29 | 0.45 | 0 | 1 |
| **Parent Some College** | 1506 | 0.33 | 0.47 | 0 | 1 |
| **Parent Completed College** | 1506 | 0.15 | 0.35 | 0 | 1 |
| **Both Parents in HH** | 1502 | 0.34 | 0.47 | 0 | 1 |
| **Parent Frequent Churchgoer** | 1500 | 0.58 | 0.49 | 0 | 1 |
| *Outcomes* | | | | | |
| **Felonies** | 2178 | 0.19 | 0.79 | 0 | 16 |
| **Misdemeanors** | 2178 | 0.27 | 0.96 | 0 | 17 |
| **Drug Crime** | 2178 | 0.11 | 0.54 | 0 | 12 |
| **Property Damage** | 2178 | 0.01 | 0.13 | 0 | 3 |
| **Disorderly Conduct** | 2178 | 0.07 | 0.34 | 0 | 4 |
| **Batteries** | 2178 | 0.03 | 0.21 | 0 | 3 |
| **Thefts** | 2178 | 0.05 | 0.35 | 0 | 7 |
| **Traffic** | 2178 | 0.73 | 1.80 | 0 | 21 |
| **Fines (in Current Dollars)** | 2178 | 526.05 | 1,843.96 | 0 | 37,717.84 |
| **Paternity Disputes** | 2178 | 0.11 | 0.37 | 0 | 3 |

**Table 3: Effects of the MPCP on Character (Three Different Statistical Models)**

| | (1)<br>Felonies | (2)<br>Misdems. | (3)<br>Drugs | (4)<br>Property | (5)<br>Disorder | (6)<br>Battery | (7)<br>Thefts | (8)<br>Traffic | (9)<br>Fines | (10)<br>Paternity |
|---|---|---|---|---|---|---|---|---|---|---|
| **1. MPCP (no controls)** | -0.023<br>(0.524) | -0.042<br>(0.376) | -0.065***<br>(0.007) | -0.011**<br>(0.041) | -0.007<br>(0.641) | -0.009<br>(0.324) | 0.004<br>(0.822) | -0.112<br>(0.218) | -45.737<br>(0.602) | -0.042**<br>(0.016) |
| R-Squared | 0.0002 | 0.0005 | 0.0036 | 0.0017 | 0.0001 | 0.0005 | 0.0000 | 0.0010 | 0.0002 | 0.0031 |
| N | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 |
| **2. MPCP (Student controls)** | -0.007<br>(0.846) | -0.032<br>(0.473) | -0.058***<br>(0.009) | -0.011**<br>(0.039) | -0.005<br>(0.735) | -0.010<br>(0.306) | 0.007<br>(0.695) | -0.094<br>(0.292) | -8.599<br>(0.920) | -0.042**<br>(0.015) |
| R-Squared | 0.0629 | 0.0666 | 0.0560 | 0.0152 | 0.0207 | 0.0104 | 0.0112 | 0.0278 | 0.0472 | 0.0127 |
| N | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 |
| **3. MPCP (All controls)** | -0.024<br>(0.589) | -0.042<br>(0.417) | -0.099***<br>(0.001) | -0.007<br>(0.235) | -0.009<br>(0.613) | -0.008<br>(0.561) | 0.017<br>(0.391) | -0.093<br>(0.283) | -27.918<br>(0.830) | -0.034<br>(0.138) |
| R-Squared | 0.0744 | 0.0966 | 0.0652 | 0.0174 | 0.0485 | 0.0124 | 0.0196 | 0.0397 | 0.0556 | 0.0183 |
| N | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 |

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. Student controls are for race, gender, grade, and baseline math and reading test scores. All controls include student controls and parental income, parental education, whether parents are frequent churchgoers, and whether both parents reside in the household. Coefficients for control variables available from the authors by request. Statistically significant results are robust to Poisson regression and negative binomial regression functional forms.

**Table 4: Heterogeneous Effects (Student Controls)**

| | (1) Felonies | (2) Misdems. | (3) Drugs | (4) Property | (5) Disorder | (6) Battery | (7) Thefts | (8) Traffic | (9) Fines | (10) Paternity |
|---|---|---|---|---|---|---|---|---|---|---|
| Male | -0.029 | -0.071 | -0.124*** | -0.024** | 0.003 | -0.017 | -0.002 | -0.126 | -67.074 | -0.045** |
| | (0.690) | (0.440) | (0.010) | (0.030) | (0.921) | (0.272) | (0.950) | (0.348) | (0.698) | (0.026) |
| | | | | | | | | | | |
| Female | 0.012 | -0.000 | -0.003 | 0.000 | -0.012 | -0.004 | 0.014 | -0.067 | 39.586 | -0.040* |
| | (0.421) | (0.996) | (0.629) | (0.981) | (0.344) | (0.710) | (0.329) | (0.488) | (0.475) | (0.078) |
| Difference | -0.041 | -0.071* | -0.121** | -0.024** | 0.015 | -0.013 | -0.016 | -0.059 | -106.66 | -0.005 |
| | (0.580) | (0.093) | (0.011) | (0.032) | (0.660) | (0.469) | (0.656) | (0.689) | (0.549) | (0.875) |
| | | | | | | | | | | |
| Low Read | -0.022 | -0.060 | -0.082** | -0.014 | -0.019 | -0.008 | 0.004 | -0.090 | 111.601 | -0.056*** |
| | (0.705) | (0.395) | (0.041) | (0.118) | (0.448) | (0.563) | (0.861) | (0.501) | (0.371) | (0.007) |
| | | | | | | | | | | |
| High Read | -0.015 | -0.024 | -0.045** | -0.008 | 0.003 | -0.012 | 0.001 | -0.131 | -197.450** | -0.034 |
| | (0.671) | (0.551) | (0.045) | (0.209) | (0.813) | (0.322) | (0.971) | (0.217) | (0.038) | (0.201) |
| Difference | -0.007 | -0.036 | -0.037 | -0.007 | -0.022 | 0.004 | 0.004 | 0.041 | 309.10** | -0.022 |
| | (0.920) | (0.650) | (0.429) | (0.538) | (0.439) | (0.828) | (0.893) | (0.812) | (0.042) | (0.478) |
| | | | | | | | | | | |
| Low Math | -0.025 | -0.036 | -0.097*** | -0.017* | 0.003 | -0.020 | 0.025 | 0.021 | 68.686 | -0.071*** |
| | (0.626) | (0.562) | (0.009) | (0.076) | (0.881) | (0.131) | (0.305) | (0.860) | (0.517) | (0.002) |
| | | | | | | | | | | |
| High Math | -0.010 | -0.053 | -0.024 | -0.004 | -0.025 | 0.004 | -0.026 | -0.279** | -164.142 | -0.013 |
| | (0.800) | (0.229) | (0.312) | (0.407) | (0.144) | (0.734) | (0.111) | (0.020) | (0.158) | (0.597) |
| Difference | -0.015 | 0.017 | -0.073 | -0.013 | 0.028 | -0.024 | 0.051* | 0.300* | 232.83 | -0.059* |
| | (0.822) | (0.808) | (0.102) | (0.246) | (0.327) | (0.170) | (0.064) | (0.063) | (0.121) | (0.059) |

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## Table 4 (Continued): Heterogeneous Effects (Student Controls)

|  | (1) Felonies | (2) Misdems. | (3) Drugs | (4) Property | (5) Disorder | (6) Battery | (7) Thefts | (8) Traffic | (9) Fines | (10) Paternity |
|---|---|---|---|---|---|---|---|---|---|---|
| Low Math Male | -0.073 | -0.078 | -0.208*** | -0.035* | 0.015 | -0.041* | 0.025 | 0.085 | 37.702 | -0.069** |
|  | (0.498) | (0.507) | (0.009) | (0.074) | (0.736) | (0.066) | (0.557) | (0.681) | (0.859) | (0.015) |
| High Math Female | -0.001 | 0.007 | -0.004 | 0.004 | -0.019 | -0.003 | -0.006 | -0.137 | -59.891 | -0.005 |
|  | (0.957) | (0.815) | (0.655) | (0.431) | (0.225) | (0.854) | (0.608) | (0.279) | (0.275) | (0.866) |
| Difference | -0.072 | -0.085 | -0.204* | -0.039 | 0.033 | -0.039 | 0.032 | 0.222 | 97.593 | -0.063 |
|  | (0.609) | (0.717) | (0.075) | (0.497) | (0.572) | (0.104) | (0.401) | (0.218) | (0.603) | (0.752) |
| Low Math Female | 0.027 | 0.013 | -0.008 | 0.000 | -0.003 | -0.001 | 0.025 | -0.034 | 106.151 | -0.075** |
|  | (0.234) | (0.717) | (0.193) | (0.857) | (0.889) | (0.958) | (0.318) | (0.764) | (0.176) | (0.044) |
| High Math Male | -0.027 | -0.135 | -0.052 | -0.015 | -0.034 | 0.013 | -0.052 | -0.461** | -302.485 | -0.021 |
|  | (0.761) | (0.170) | (0.274) | (0.183) | (0.307) | (0.486) | (0.101) | (0.025) | (0.222) | (0.487) |
| Difference | 0.054 | 0.148 | 0.060* | 0.015 | 0.031 | -0.013 | 0.077 | 0.427 | 408.636 | -0.054 |
|  | (0.609) | (0.717) | (0.075) | (0.497) | (0.572) | (0.104) | (0.401) | (0.218) | (0.603) | (0.752) |
| R-Squared | 0.0630 | 0.0670 | 0.0591 | 0.0172 | 0.208 | 0.0106 | 0.0114 | 0.0279 | 0.0474 | 0.0127 |
| N | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 | 2178 |

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 available from the authors by request. "Low reading" and "low math" refers to students with baseline test scores at or below the 50th 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 form 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. 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.

| | (1) Felonies | (2) Misdems. | (3) Drugs | (4) Property | (5) Disorder | (6) Batteries | (7) Thefts | (8) Traffic | (9) Fines | (10) Paternity |
|---|---|---|---|---|---|---|---|---|---|---|
| Male | -0.062 | -0.074 | -0.211*** | -0.019* | 0.001 | -0.008 | 0.021 | -0.162 | -133.197 | -0.045 |
| | (0.508) | (0.472) | (0.002) | (0.068) | (0.985) | (0.684) | (0.566) | (0.256) | (0.610) | (0.123) |
| Female | 0.007 | -0.016 | -0.011 | 0.003 | -0.016 | -0.008 | 0.015 | -0.038 | 54.535 | -0.024 |
| | (0.739) | (0.637) | (0.253) | (0.394) | (0.249) | (0.637) | (0.425) | (0.708) | (0.549) | (0.434) |
| Difference | -0.069 | -0.058 | -0.199*** | -0.022** | 0.017 | 0.001 | 0.006 | -0.124 | -187.73 | -0.021 |
| | (0.466) | (0.579) | (0.003) | (0.028) | (0.665) | (0.970) | (0.878) | (0.470) | (0.481) | (0.611) |
| | | | | | | | | | | |
| Low Read | -0.040 | -0.034 | -0.120** | -0.003 | -0.022 | 0.006 | 0.022 | -0.092 | 163.641 | -0.056* |
| | (0.576) | (0.708) | (0.029) | (0.741) | (0.447) | (0.750) | (0.542) | (0.519) | (0.357) | (0.059) |
| High Read | -0.032 | -0.073* | -0.096*** | -0.012* | 0.000 | -0.024 | 0.002 | -0.108 | -298.418* | -0.015 |
| | (0.511) | (0.056) | (0.001) | (0.057) | (0.989) | (0.145) | (0.827) | (0.345) | (0.063) | (0.639) |
| Difference | -0.007 | 0.040 | -0.024 | 0.010 | -0.022 | 0.031 | 0.019 | 0.017 | 462.08** | -0.041 |
| | (0.932) | (0.684) | (0.694) | (0.329) | (0.483) | (0.214) | (0.610) | (0.931) | (0.043) | (0.300) |
| | | | | | | | | | | |
| Low Math | -0.068 | -0.072 | -0.155*** | -0.012* | -0.012 | -0.012 | 0.031 | 0.028 | 50.479 | -0.070** |
| | (0.318) | (0.374) | (0.005) | (0.081) | (0.668) | (0.489) | (0.335) | (0.839) | (0.741) | (0.039) |
| High Math | 0.005 | -0.028 | -0.048* | -0.001 | -0.011 | -0.004 | -0.011 | -0.264** | -194.072 | 0.007 |
| | (0.924) | (0.514) | (0.075) | (0.888) | (0.456) | (0.838) | (0.378) | (0.020) | (0.289) | (0.817) |
| Difference | -0.073 | -0.044 | -0.107* | -0.011 | 0.000 | -0.008 | 0.042 | 0.293 | 244.55 | -0.076* |
| | (0.383) | (0.623) | (0.084) | (0.303) | (0.989) | (0.746) | (0.196) | (0.126) | (0.273) | (0.081) |

**Table 5 (Continued): Heterogeneous Effects (All Controls)**

| | (1) Felonies | (2) Misdems. | (3) Drugs | (4) Property | (5) Disorder | (6) Batteries | (7) Thefts | (8) Traffic | (9) Fines | (10) Paternity |
|---|---|---|---|---|---|---|---|---|---|---|
| Low Math Male | -0.153 | -0.108 | -0.316*** | -0.025* | 0.014 | -0.026 | 0.025 | 0.148 | -11.038 | -0.080* |
| | (0.279) | (0.502) | (0.005) | (0.071) | (0.808) | (0.357) | (0.686) | (0.558) | (0.970) | (0.065) |
| High Math Female | -0.004 | 0.009 | -0.010 | 0.007 | 0.004 | -0.019 | -0.021 | -0.027 | -67.114 | 0.015 |
| | (0.881) | (0.823) | (0.503) | (0.345) | (0.774) | (0.480) | (0.142) | (0.825) | (0.437) | (0.718) |
| Difference | -0.148 | -0.117 | -0.306* | -0.032 | 0.011 | -0.007 | 0.045 | 0.175* | 56.077 | -0.094 |
| | (0.271) | (0.970) | (0.087) | (0.798) | (0.289) | (0.148) | (0.625) | (0.055) | (0.719) | (0.996) |
| Low Math Female | 0.022 | -0.020 | -0.003 | 0.000 | -0.028 | 0.001 | 0.041 | -0.080 | 116.249 | -0.063 |
| | (0.408) | (0.696) | (0.760) | (0.881) | (0.202) | (0.949) | (0.148) | (0.582) | (0.318) | (0.219) |
| High Math Male | 0.010 | -0.086 | -0.108** | -0.013 | -0.035 | 0.016 | 0.000 | -0.585*** | -373.787 | -0.003 |
| | (0.927) | (0.282) | (0.045) | (0.405) | (0.234) | (0.430) | (0.983) | (0.003) | (0.354) | (0.934) |
| Difference | 0.012 | 0.066 | 0.105* | 0.013 | 0.006 | -0.015 | 0.041 | 0.505* | 490.035 | -0.060 |
| | (0.271) | (0.970) | (0.087) | (0.798) | (0.289) | (0.148) | (0.625) | (0.055) | (0.719) | (0.996) |
| R-Squared | 0.0749 | 0.0969 | 0.0722 | 0.0201 | 0.0486 | 0.0124 | 0.0196 | 0.0400 | 0.0560 | 0.0179 |
| N | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 | 1385 |

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 available from the authors by request. "Low reading" and "low math" refers to students with baseline test scores at or below the 50th 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 form 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.