5-2018

The Societal Impacts of Private School Choice around the World

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The Societal Impacts of Private School Choice around the World

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy in Education Policy

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May 2018
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Abstract

With the 2016 presidential campaign and the appointment of Betsy DeVos as Education Secretary of the United States, private school choice – in the form of vouchers, tuition tax-credits, and education savings accounts – has become increasingly policy relevant. While an introduction of competitive pressures into the schooling sector may improve educational quality levels, the effects on societal outcomes such as national test scores, student effort, and criminality may be less clear. After all, traditional public schools were created to ensure that children from diverse backgrounds became proper citizens.

These three dissertation chapters empirically examine a largely underexplored area: the societal impacts of private school choice around the world. The chapters explore the effects of private school choice on international student test scores, student effort, and student criminality using quasi-experimental methodology. The results suggest that private schooling improves student test scores and reduces the proclivity of students to commit crimes as adults. The analyses also suggest that private schooling increases student effort on international tests and decreases student effort on long surveys after international exams. I discuss each of these findings as they relate to the academic literature and current education policy debates.

Acknowledgements

I would like to thank each of my professors at the Department of Education Reform at the University of Arkansas – Gary W. Ritter, Gema Zamarro, Jay P. Greene, Patrick J. Wolf, Robert M. Costrell and Robert A. Maranto – for their guidance over the last few years.

I am especially grateful to John Merrifield for advising me during my time at the University of Texas at San Antonio. Thankfully, he nudged me several times to pursue the doctorate in education policy at the University of Arkansas.

I am also especially grateful to my current advisor and committee chair, Patrick J. Wolf. He has given me a great deal of professional advice, feedback, and opportunities.

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DeAngelis, C. A., & Wolf, P. J. (2018). The school choice voucher: A 'get out of jail' card? Under review at *Public Choice*.

Introduction

The current system of traditional public schooling in the United States is based on residential assignment. If a family is not happy with their residentially assigned schooling option, they usually only have three options: (1) they can move houses in order to have their child attend a different traditional public school, (2) they can continue to pay for their assigned public school and, in addition, pay for a private school out of pocket, or (3) they can use political pressure to try to get their children into special programs within the public school system. However, since option one has very large transaction costs, option two is usually financially infeasible, and option three is highly unlikely for groups without substantial political power, families often must keep their children in a school that is not serving them well. Consequently, traditional public schools yield a strong amount of monopoly power in the educational market. And as with any other monopoly situation, economists expect costs to rise and quality levels to fall (Friedman, 1990; Samuelson & Nordhaus, 1995).

There are currently sixty-three private school choice programs – in the form of vouchers, tax-credit scholarships, tax-credit deductions, and education savings accounts – in over half of the states in the U.S. today (EdChoice, 2018). These programs allow families to opt their children out of their residentially assigned schools in order to attend the private schools that best fit their needs. Because private school choice programs shift monopoly power away from government schools, and financially reward schools for a job well done, they are theorized to improve educational quality levels and reduce educational costs (Chubb & Moe, 1988; Chubb & Moe, 1990; Friedman, 1997; Hoxby, 2003). In addition, since private schools are able to charge tuitions, they benefit from the invaluable information and incentives generated by the price system (Hayek, 1945). After all, if a traditional public school does a splendid job with educating

children, they will still receive around the same amount of funding from the government the following year. Unfortunately, the incentives can be even more perverse for a traditional public school that does a poor job; many traditional public schools are rewarded with compensatory funding from the federal government if they perform poorly. Moreover, since district schools generally do not lose 100 percent of their per pupil funding when children exit – and they lose all of the costs associated with educating those children – they financially benefit when their customers leave (DeAngelis & Trivitt, 2016; Scafidi, 2012). On the other hand, private schools are financially rewarded for satisfying customers because they are able to raise their prices to meet supply and demand. Furthermore, schools of choice may improve quality levels simply by allowing for a better match between educators and students (DeAngelis & Holmes Erickson, 2018). After all, every individual student has unique learning styles, ability levels, and interests.

During the 2016 presidential campaign, Donald Trump called for a $20 billion allocation of federal funds towards school choice programs. Unsurprisingly, the concept of school choice has gained substantial public interest since the 2016 presidential election and the nomination of Betsy DeVos as the Education Secretary of the United States. Indeed, a Google Trends search of "school choice" reveals that the term reached its historic peak in public interest in early 2017, right around the time of the Betsy DeVos confirmation.[1] Consequently, heated discussion regarding the potential merits and shortcomings of private school choice programs frequently occurs. While some education scholars claim that an introduction of competitive pressures could improve the education system overall (Chubb & Moe, 1990; Friedman & Friedman, 1990; Hoxby, 2003), others contend that schools do not behave well in a market setting since they are primarily meant to produce benefits to the public (Gutmann, 1999; Ravitch, 2016; Saltman,

[1] School Choice. Google Trends. Retrieved from https://trends.google.com/trends/explore?date=all&q=school%20choice

2000). After all, traditional public schools were originally created in order to teach children how to become proper citizens within a stable democracy (Dewey, 1916; Mann, 1855; Rush, 1786).

The scientific evidence of the effects of private school choice on student achievement is abundant. There are twenty-one experimental studies of the effects of voucher programs on student test scores around the world, and only two have found statistically significant negative effects (Abdulkadiroglu, Pathak, & Walters, 2018; Dynarski et al., 2017). These two studies, however, only examine effects one year after children started using the voucher programs. Other voucher evaluations have found that student achievement impacts improve over time since children and schools adjust to the transitions (Mills & Wolf, 2017; Waddington & Berends, 2017). In addition, a meta-analysis of nineteen of the experimental voucher studies finds a moderately positive overall average effect on student achievement (Shakeel, Anderson, & Wolf, 2016). Out of the seventeen experimental studies of voucher programs in the United States, eleven studies (Anderson & Wolf, 2017; Barnard et al., 2003; Cowen, 2008; Greene, 2001; Greene, Peterson, & Du, 1999; Howell et al., 2002 (three locations); Jin, Barnard, & Rubin, 2010; Rouse, 1998; Wolf et al., 2013) find positive effects on test scores for some or all students and four (Bettinger & Slonim, 2006; Bitler et al., 2015; Krueger & Zhu, 2004; Mills & Wolf, 2017a) fail to detect any statistically significant effects. While the preponderance of the private school choice evidence is positive as it relates to test scores, some scholars point out that the most recent experimental evaluations in the United States are the negative ones (Abdulkadiroglu, Pathak, & Walters, 2018; Dynarski et al., 2017; Mills & Wolf, 2017b). Some education scholars argue that private school choice may reduce student learning today, even if most of the existing evidence indicates the opposite.

To further test this claim, I examine how changes in the private share of schooling within 63 countries around the world affect Program for International Student Assessment (PISA) scores in recent years, from 2000 to 2012. The study in this first chapter is able to establish a causal relationship because it uses relevant control variables and a new instrumental variable: short-run fluctuations in the demand for schooling within countries. Since public schools all around the world are constitutionally obligated to provide a free education to all children, unexpected shifts in schooling demand in the short-run are more likely to be absorbed by public schools. In addition, shocks in the demand for schooling within a country over time only affect PISA scores through their influence on the private share of schooling. I find that increases in the private schooling share have moderate positive effects on student math, reading, and science test scores.

However, private schooling critics may not be surprised by positive test score effects After all, a standardized test score is arguably a weak metric for capturing skills that benefit the rest of society. University of Arkansas researcher Jay P. Greene (2016) has pointed out that the results from at least ten school choice evaluations in the United States indicate a disconnect between test scores and the long-term outcomes (e.g. graduation rates, college enrollment, earnings, and crime) that we actually care about. For example, the experimental evaluation of the voucher program in D.C. finds little or no test score gains, but very large positive effects on high school graduation (Wolf et al., 2013), while charter schools in Boston produce huge test score gains but no effects on attainment (Angrist et al., 2016). In addition, cognitive abilities may be skills that have large private benefits. In other words, if a student chooses a school that maximizes their test scores, they are likely to receive a substantial portion of the financial benefits that result from that in the future. And after all, public schooling advocates claim that

public schools are necessary to shape skills that are necessary for social cohesion instead of private gain. For example, teaching a student to respect other people in society is likely to have large benefits that accrue to third parties (i.e. respect has positive externalities), so private schools of choice may underperform at teaching children the importance of respect.

In order to test whether private schooling is able to successfully shape skills with large theorized positive externalities, I examine effects on student test and survey effort, which may be driven by skills such as respect for others. In chapter two, I use a well-established instrumental variable (Heller-Sahlgren, 2018; West & Woessmann, 2010), the Catholic share of the population within a country in 1900, to predict the likelihood that a child will end up in a private school today. Catholic populations had a stronger incentive to set up a system of private schools in 1900 if Catholicism was not the state religion. Because larger groups of Catholic populations in a given country were able to more successfully establish a system of private schools over a century before, children that happen to be born in a country with a more extensive Catholic school network are more likely to attend a private school today. The study uses student non-response rates and careless answering as proxy measures for effort on the PISA survey and uses test-decline as a proxy for student effort on the PISA exam. The results indicate that private schooling increases test scores and test effort, but decreases survey effort, perhaps because the various measures employed capture different types of non-cognitive skills.

In chapter three, my coauthor and I conduct the first analysis of the effects of a private school choice program on adult crime. Specifically, we examine the effects of the Milwaukee Parental Choice Program on the likelihood that individual students will commit crimes between the ages of 22 and 25 years old. We use a quasi-experimental matching procedure that has been shown to replicate experimental results (Bifulco, 2012) and find that while mere exposure to the

voucher program may not have effects on adult criminality, four or more years of program use lead to substantial reductions in the likelihood that students will grow up to be criminals. While this is the first quasi-experimental study to examine the effect of private school choice on criminal activity, three other studies have either quasi-experimentally (Dills & Hernández-Julián, 2011) or experimentally (Deming, 2011; Dobbie & Fryer, 2015) evaluated the effects of public school choice on crime. Our results for students that received four or more years of the Milwaukee Parental Choice Program are similar to these three previous evaluations.

These three dissertation chapters add to the body of causal literature indicating that access to private schooling – around the world – leads to benefits that accrue to the individual (Shakeel, Anderson, & Wolf, 2016) and the rest of society (Bettinger & Slonim, 2006; Campbell, 2002; DeAngelis, 2017; Fleming, 2014; Fleming, Mitchell, & McNally, 2014; Wolf, 2007; Wolf, Peterson, & West, 2001). Based on the results found in this dissertation – and the preponderance of the quasi-experimental and experimental evidence existing on the topic – decision-makers ought to expand access to private school choice programs. However, decision-makers should also consider the potential effects of private school choice policy design on student outcomes as well. After all, one of the most highly regulated private school choice programs – the Louisiana Scholarship Program – was the first experimental evaluation in the world to find negative effects on student achievement (Mills & Wolf, 2017b). Since then, researchers have found that burdensome packages of regulations could lead to less private school specialization (DeAngelis & Burke, 2017) and lower quality private schooling options for children (Sude, DeAngelis, & Wolf, 2018).

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Chapter One

Does Private Schooling Affect International Test Scores?

Evidence from a Natural Experiment

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Abstract

I estimate the effect of private schooling on Program for International Student Assessment (PISA) scores of 63 countries across the globe from 2000 to 2012. I employ year and country fixed effects regression models and use the short-run demand for schooling within a country and year as an instrument to predict private share of schooling enrollment. I find evidence to suggest that an increased share of private schooling leads to improved PISA scores around the world. Specifically, the model using control variables alongside country and year fixed effects finds that a one percentage point increase in the private share of schooling enrollment is associated with a 1.4-point increase in math scores and a 1.1-point increase in reading scores. However, only the reading result remains statistically significant in the instrumental variables analysis.

Keywords: private school; school choice; PISA; international education; standardized testing

Section 1: Introduction

During the 2016 presidential campaign, Donald Trump called for a twenty-billion-dollar increase in federal funding of private school choice programs in the United States. What impacts would the proposed policy have within the U.S., and what could similar policies do to change educational success around the rest of the world? While some scholars believe that competitive pressures could enhance educational quality while minimizing costs (Chubb & Moe, 1988; Friedman & Friedman, 1990; Neal, 2002), others claim that the education sector may not behave like other industries (Gutmann, 1999).

For instance, if families have the ability to choose their educational product, and they do not have the information required to make informed decisions, they may choose schools that actually harm their children in the short-run. Additionally, since individual interests may differ from social interests, families may not choose an educational product that is effective at inculcating math, reading, and science skills (Boyles, 2004; Saltman, 2000). If families do not place a high enough value on the skills that are measured by standardized assessments, we may expect that access to private schools would reduce overall test scores. In addition, as the father of American public schooling, Horace Mann (1855), as well as John Dewey (1916), argued, common schooling may be necessary in order to inculcate a uniform set of values and to teach children from diverse backgrounds to get along with one another and to become proper democratic citizens.

However, if individual families choose educational products that improve cognitive abilities, and standardized tests capture student achievement, we might expect to observe improved Programme for International Student Assessment (PISA) scores resulting from increases in access to private schooling. PISA is a standardized assessment, coordinated by the

Organisation for Economic Co-operation and Development (OECD) that examines academic abilities of 15-year-old children around the world. The assessment is scaled to have a mean of 500 and a standard deviation of 100. In theory, a deviation from the public schooling monopoly on public funding within education systems around the world could increase educational quality through enhanced competitive pressures for schools to improve (Hoxby, 2003; Chubb & Moe, 1990).

I examine how changes in the private share of schooling within countries are related to PISA scores from 2000 to 2012 after controlling for factors such as gross domestic product (GDP) in billions, population (in millions), and government expenditures as a percent of GDP. This study is able to add to the literature in two ways: (1.) removing most of the endogeneity problems that arise from ordinary across-country comparisons by comparing countries to themselves over time, and (2.) using a relatively new instrumental variable, short-run fluctuations in the demand for schooling overall, to exogenously predict the private share of schooling within a given country/year observation. If private schooling can increase competitive pressures and provide valuable information through price differentiation (Friedman, 1997), then increases in the private share of schooling enrollment may increase PISA scores.

Section 2: Theory

It is possible that an increased share of private schooling within a country could increase the quality of the education experienced by students through increased competitive pressures, specialization, and an improved match between educator and student.

Since most systems of public schooling operate with a monopoly on public funds, public schools enjoy a great deal of monopoly power in general (Chubb & Moe, 1990). Theoretically, in any industry where a producer has monopoly power, quality levels remain low while costs

gravitate upwards (Samuelson & Nordhaus, 1995). This result is because the producer does not have much of an incentive to increase quality and decrease prices. If private schooling is introduced into the system, competitive pressures increase the incentives for both public and private schools to offer the highest-quality education at the lowest cost. There is evidence that private school choice programs could balance the distribution of power within the school system and families could exercise that power to pressure schools to improve (Egalite, 2013; Figlio & Hart, 2014).

Since public school officials may have an incentive to maximize their budgets (Levenson, 2012; Niskanen, 1971), and schools are funded based on enrollment, school leaders are inclined to keep as many students as they can. Additionally, private school choice programs can introduce price differentiation into the system of schooling. Price differentiation can entice new high quality schooling options to enter the market for education and can also communicate valuable information about what is valued by parents and children (Friedman & Friedman, 1990; Hayek, 1945). At the same time, tuition variation rewards high quality schools for serving parents and children while incentivizing low quality schools to improve, lose market share, or shut down.

An educational choice system can improve the match between educator and student through specialization (DeAngelis & Holmes Erickson, 2018). Children appear to have diverse interests, learning styles, ability levels and family structures. Providing specialized learning environments that meet the unique needs of children can improve the overall educational experience. Indeed, simply increasing the number of diverse options available to children could increase the likelihood that children are matched to a school that interests them. If these notions are true, then the increases in educational quality influenced by the introduction of private schooling within a country can lead to improved standardized test scores for students.

Alternatively, private schools may provide a quality education to children by enhancing skills that are not easily measured by standardized assessments like PISA. If private schools are allocating more resources towards improving skills that are not captured by standardized tests, we may observe a negative effect of private schooling on PISA scores. Critics of private schooling argue that since parents are not experts in pedagogy or education, they may not make good decisions when selecting schools for their children. The inability of parents to choose well, they argue, may lead to a lower-quality educational experience for children.

Section 3: Literature Review

The evidence on how private school choice impacts standardized test scores is abundant. Shakeel, Anderson, and Wolf (2016) perform a meta-analysis and systematic review of the evidence from 19 experimental studies and find that private school voucher programs around the world produce small positive impacts on student achievement. They also find that the results are typically larger for reading scores, programs outside of the United States, and publicly funded programs. In the United States, almost all experimental evaluations of private school voucher programs produce null to positive results. There are currently only two exceptions: (1.) Abdulkadiroglu, Pathak, and Walters (2018) find that the Louisiana Scholarship Program has negative impacts on student achievement in initial years and (2.) Dynarski et al. (2017) find that the voucher program in the District of Columbia (D.C.) has negative effects on student mathematics achievement after one year.

While the overall average of the experimental evaluations of private school choice programs is slightly positive overall (Shakeel, Anderson, & Wolf, 2016), the more recent experimental evaluations find null (Mills & Wolf, 2017b) to negative (Abdulkadiroglu, Pathak, & Walters, 2018; Dynarski et al., 2017; Mills & Wolf 2017a) effects on student standardized test

scores. This downwards trend over the years may cause concern about the overall merits and policy implications regarding private school choice programs today. This study adds to this recent literature by empirically analyzing how fluctuations in the private share of schooling within countries is related to student standardized tests scores in recent years, from 2000 to 2012.

The four experimental evaluations (Angrist et al., 2002; Angrist, Bettinger, & Kremer, 2006; Muralidharan & Sundararaman, 2015; Wolf, Egalite, & Dixon, 2015) of private school choice programs outside of the U.S. find slightly larger positive effects on student achievement. Muralidharan and Sundararaman's (2015) experiment finds that access to private school choice in India improves test scores by around 0.23 standard deviations overall. Tooley and Dixon (2005) also find that access to private schooling is associated with benefits for disadvantaged children around the world. Additionally, Shafiq and Myers (2014) find that access to private school vouchers in Sweden is associated with a slight increase in the students' civic attitudes between 1999 and 2009.

Hanushek, West, and Woessmann (2013) used PISA data to find that autonomy had a positive impact on test scores for high-performing countries, but a negative impact for developing countries. While the causal research connecting private schooling and PISA scores has been limited, Hanushek and Woessmann (2010) pointed out their optimism about research on the topic, stating that the outlook for international studies was "clearly bright" since "more than 60 countries" were planning to participate in the 2012 PISA exam.

Few existing studies attempt to determine the effect of private schooling on student test scores around the world. D'Agostino (2016) examined the private share of school enrollment in 30 countries in 2012, but did not find a statistically significant effect on PISA scores. After controlling for differences in student and parent characteristics across 19 OECD countries,

Dronkers and Robert (2008) found that government-dependent private schools outperformed comparable public schools on PISA scores in 2000, while public schools outperformed comparable government-independent private schools. Sakellariou (2017) examined schooling in 40 countries in 2012 and found that public schools outperformed private schools on PISA scores. Importantly, Hanushek and Woessmann (2012) found strong international evidence to suggest that student achievement – captured by PISA scores – leads to economic growth. However, since these studies all used cross-country comparisons, they cannot be interpreted as causal. There are several uncontrolled differences in students from different countries – such as language, culture, religion, household income, values, and ethnicity – that likely affect their PISA scores and therefore result in biased estimates. In addition, the definition of what it means to be a private school could be inconsistent across countries, further leading to biased estimates in across-country analyses.

West and Woessmann (2010) used 2003 PISA data for 29 nation-states and found that countries with higher private shares of schooling were associated with improved international test scores. Importantly, they used the percent of Catholics within a country from the year 1900 as an instrument to predict current private share of schooling. Since the historic Catholic share of the population is highly correlated with whether a student ended up in a private school in 2003, but is unrelated to the student's test score in 2003, they suggest, their paper identifies a causal relationship between private schooling and higher student achievement. Similarly, Heller Sahlgren (2018) used the same instrumental variable – historical Catholic share of the total population – and found that private schooling improved student PISA scores in 2012.

While this approach was a reasonable attempt to remove endogeneity, the instrumental variable is unfortunately correlated with many omitted variables such as current country culture,

political structure, and economic structure. For example, it may be that countries with larger Catholic shares of the population in 1900 are also less racially heterogenous in 2003. Racially homogeneous countries may have a less difficult time educating children in math, reading, and science, regardless of whether they are in public or private schools (Partanen, 2011). For these reasons, it is possible that their instrument does not remove the endogeneity problem with the explanatory variable of interest.

This study improves upon the methods used by West and Woessmann (2010) and Heller-Sahlgren (2018) in two ways. First, I have access to five separate years of data for 62 nations, so I am able to use year and country fixed effects in order to compare PISA scores within, rather than across, countries. Second, as a robustness check, I use an instrument that is more exogenous to the model than the historical share of Catholic population: the short-run change in the demand for total schooling within a country and year. Additionally, this study is the first to endeavor to causally link private schooling to the recent PISA evaluation mentioned by Hanushek and Woessmann in 2010.

Section 4: Data

I use pooled cross-sectional country-level data from multiple sources for the years of 2000 to 2012. I use data from the World Bank[2] and the United Nations Data Retrieval System[3] for the independent variable of interest, the private share of total primary schooling enrollment. As outlined by OECD,[4] this study defines a private educational institution as one that "is controlled and managed by a non-governmental organization, or if its governing board consists mostly of members not selected by a public agency." I also use the World Bank for GDP, population, life expectancy, and total schooling enrollment.

[2] http://data.worldbank.org/indicator/SE.SEC.PRIV.ZS
[3] http://data.un.org/Data.aspx?d=UNESCO&f=series%3APRP_1
[4] https://stats.oecd.org/glossary/detail.asp?ID=2123

The three dependent variables of interest are from the Program for International Student Assessment (PISA). I use national-level PISA math, reading, and science test scores for 63 countries around the world from 2000 to 2012. The models use 214 country-year observations for math and science, and 212 country-year observations for reading. These data are publicly-available online at the National Center for Education Statistics website.[5] Of course, since the analytic sample only captures about a third of the 195 countries that exist in 2017, it is not globally representative. The analytic sample includes 33 of the 44 countries in Europe, 6 of the 23 countries in North America, 6 of the 12 countries in South America, 15 of the 48 countries in Asia, 2 of the 14 countries in Oceania, and 1 of the 54 countries in Africa.

Section 5: PISA Assessment

PISA is a standardized assessment, coordinated by the Organization for Economic Cooperation and Development (OECD), which examines academic abilities of 15-year-old children around the world. PISA started in 2000 with 32 participating countries and has been administered every three years. In 2012, nationally-representative samples of children took the assessment from 65 different countries. The subjects included reading, math, science, problem solving, and financial literacy.

In order for the data from a country to be valid, OECD requires that each nation tests at least 4,500 students from at least 150 different schools. The testing period can be no longer than 42 days, and the response rate must be equal to or greater than 65 percent of the original sample of schools. As a validity check, Westat analyzes the final list of schools before data is made publicly-available. At the school level, the response rate must be equal to or greater than 80 percent of the sampled students. The sampling procedure is stratified systematic sampling with each observation weighted by the inverse of the probability of being sampled.

[5] http://nces.ed.gov/surveys/pisa/idepisa/dataset.aspx

During the sample period, the test was mostly paper-and-pencil with 17 different examination booklets randomly assigned to students. Each student received only one booklet which had four different clusters of material. Each cluster contained about 30 minutes of material on one of the following: reading, math, science, or financial literacy. About half of the questions were multiple-choice, a fifth were short-response, and about a third were constructed-response.

Although the 2015 PISA results are available, I am unable to use them for the analyses since data from the same period are not yet available for the explanatory variable of interest or the control variables.

Section 6: Methods

I use a year and country fixed effects regression approach of the form:

$$PISA_{it} = \beta_0 + \beta_1 PrivateShare_{it} + \beta_2 GDP_{it} + \beta_3 GovtExpend_{it} + \beta_4 Pop_{it} + \beta_5 Enroll_{it} + \beta_6 LifeExpect_{it} + \beta_7 Mortality_{it} + \alpha_i + \varepsilon_{it}$$

Where *PISA* is one of the three dependent variables of interest for country *i* at period *t*. The three dependent variables of interest are math, reading and science test scores as measured by the international PISA assessment.

PrivateShare is the independent variable of interest, the private school share of total primary schooling enrollment, for country *i* in period *t*. If private schooling could increase competitive pressures in education and improve student academic success captured by standardized tests, then the coefficient of interest, β_1, will be positive.

I include a set of country-level control variables since certain characteristics of countries may cause them to become better educated as well as increase private-sector schooling. For example, an increase in GDP could lead a country to increase spending on schooling since it has more income. Concurrently, the PISA scores within a country are likely to increase due to an

increase in its income. If parents and the state have more money to spend on educational services, and standardized tests capture student achievement, PISA scores would be expected to rise. *GDP* is the gross domestic product (in billions of U.S. dollars) for country *i* in year *t*. *GovtExpend* is the total of all government expenditures as a percent of GDP, *Pop* is the population (in millions), *LifeExpect* is the average life expectancy (in years), *Mortality* is the infant mortality rate, and *Enroll* is the total number of students enrolled in private and public schooling for country *i* in period *t*. Due to the observed non-linear relationship between the dependent variables and GDP, population, and enrollment, I also include squares of these terms in the models. While the preferred model includes squared terms, the observed results are also robust to models without squared terms. Finally, α_i is the set of country-level time-invariant parameters, such as ethnicity, language, and culture, and ε_{it} is the random error term.

Including year fixed effects allows me to control for global time series trends, while including country fixed effects allows me to compare countries to themselves over time. Using country-level fixed effects is especially important in this type of analysis because of the fact that private schooling systems, and the definition of a private school in general, function differently across countries. Since I am able to compare countries to themselves over time, and definitions of private schooling remain relatively constant within countries, I am able to remove the across-country problem of a possible endogenous relationship.

In theory, the explanatory variable of interest, private share of total primary schooling enrollment, may still suffer from an endogeneity issue. For example, an omitted variable measuring the amount of regulation in the schooling industry could create an upward bias on the effects since it is negatively associated with private share of schooling and perhaps also negatively correlated with PISA scores as well, since more regulation could simply reduce

teacher autonomy in both private and public sectors. Because of this potential issue, I also employ an instrumental variable year and country-level fixed effects two-stage least squares regression of the form:

$$\text{PrivateShare}_{it} = \lambda_0 + \lambda_1 \text{ChildPop}_{it} + \lambda_2 X_{it} + \alpha_i + \varepsilon_{it} \quad (1)$$

$$\text{PISA}_{it} = \beta_0 + \beta_1 \text{PrivateShare}_{it} + \beta_2 X_{it} + \alpha_i + \varepsilon_{it} \quad (2)$$

Where the second-stage, possibly endogenous explanatory variable of interest, *PrivateShare*, is predicted in the first stage with an exogenous instrument, *ChildPop*, the percent of the total population that is between the ages of 0 and 14 for country *i* in year *t*. The instrument represents an unexpected shock in the demand for schooling overall in the short-run. Since public schools around the world are constitutionally-obligated[6] to provide a free primary education for all children, public schools may be more likely to absorb this excess demand. On the other hand, private schools may be less likely to respond to short-run shocks in demand since the profit-incentives for school expansion and market entry may not appear quickly enough. Descriptive statistics of the instrumental variable can be found in Table A1 in the Appendix.

As a result, I expect that the instrument may be strongly negatively correlated to the share of private schooling enrollment within a country and year. The instrument passes the redundancy condition since it does not likely directly affect the four outcome variables of interest; the number of children in a given country/year should not directly affect PISA scores within a country and year. Furthermore, when I include this instrument in the structural model, I do not find evidence to suggest that the instrument is correlated with any of the outcome variables. Lastly, the instrument is exogenous since it is not correlated with any omitted variables that may concern us. For example, an unexpected shock within a country, such as a coup d'état, could increase the need for private schooling while simultaneously decreasing PISA scores. After all, a

[6] http://www.worldpolicycenter.org/policies/is-education-tuition-free/is-primary-education-tuition-free

coup d'état could temporarily shift government resources to national defense spending and away from education expenditures. At the same time, the instability of a coup d'état could make it more difficult for children and teachers to focus on maximizing learning. While a coup could increase private schooling, the relative number of children within a country and year is not directly related to the likelihood of a coup. In addition, DeAngelis and Shakeel (2017) used the same instrumental variable to exogenously predict changes in the private share of schooling within countries over time. Similarly, Hoxby (2000) used natural variation in population as an instrumental variable to predict changes in class size. I also include all of the controls from the previous models in vector X. The descriptive statistics of the analytic sample can be found in Table 1 below.

Table 1: Descriptive Statistics

	Mean	Overall Standard Deviation	Within-Country Standard Deviation	Minimum	Maximum
PISA Math	468.0	56.4	10.5	292.1	573.5
PISA Reading	466.4	50.8	11.0	284.7	556.0
PISA Science	473.1	51.3	9.0	322.0	563.3
Private Share	13.7	16.9	3.0	0.0	99.1
GDP (Billions)	285.5	1,194.7	320.0	0.0	17,348.1
Govt Expend (% GDP)	16.4	8.7	3.2	6.2	27.6
Population (Millions)	34.1	130.6	7.0	0.0	1,364.3
Enrollment (Millions)	3,409.0	12,125.9	1,190.2	0.0	141.2
Life Expectancy	68.4	9.7	1.9	38.0	83.0
Infant Mortality (%)	3.2	2.9	0.7	0.2	14.6
Child Population (%)	30.5	10.8	1.9	12.9	50.4
OECD	0.2	0.4	0.0	0.0	1.0

Section 7: Results

I first present results for the country-level fixed-effects models without time-variant controls. Then, I present results based on the model with year and country-level fixed effects. Finally, I present the instrumental variables year and country-level fixed effects results.

Section 7.1: Year and Country Fixed Effects

Table 2 reports results using country and time fixed effects. Results in this first model indicate that an increase in private share of total schooling enrollment is associated with higher PISA scores for all three subjects.

In particular, Table 2 shows that a one percentage point increase in the private share of schooling enrollment is associated with a 2.5-point increase in math scores, a 1.4-point increase in reading scores, and a 1.3-point increase in science scores. These results are equivalent to a 24 percent of a standard deviation increase in math scores, a 13 percent of a standard deviation increase in reading scores, and a 15 percent of a standard deviation increase in science scores. These effect sizes are considered small to medium using standards created by Jacob Cohen (1992) and Mark Lipsey (1990). However, for research in education, these effect sizes are quite large (Hill et al., 2008).

Table 2: The Effect of Private Schooling on PISA Scores

	Math	Reading	Science
Private Share	**2.51*** **	**1.46** **	**1.33*** **
	(0.00)	**(0.02)**	**(0.01)**
Constant	444.30***	455.36***	459.27***
	(0.00)	(0.00)	(0.00)
R-Squared Within	0.11	0.17	0.11
Countries	64	64	64
N	218	216	218

Note: P-values in parentheses. All models use country and year fixed effects.
* p<0.10, ** p<0.05, *** p<0.01

Section 7.2: Year and Country Fixed Effects with Controls

Since there are important factors that may significantly vary within countries in a relatively short time period, I include an additional model which controls for many of these factors. Table 3 reports results for the model which includes controls and year and country fixed effects. These

results indicate that an increase in private share of schooling enrollment is associated with an increase in PISA scores. However, perhaps because of the inclusion of multiple control variables and reliance on the statistical power generated by only 214 observations, the standard errors increase relative to the previous model without controls.

Specifically, Table 3 below shows that a one percentage point increase in the private share of schooling enrollment is associated with a 1.4-point, or 13 percent of a standard deviation, increase in PISA math scores. A one percentage point increase in the private share of schooling enrollment is associated with a 1.1-point increase in PISA reading scores and a 0.9-point increase in PISA science scores. This equates to a 10 percent of a standard deviation increase in reading scores and a 10 percent of a standard deviation increase in science scores; however, the effect on science becomes statistically insignificant with a p-value of 0.11.

The control variables are in their expected directions when statistically significant. In particular, it appears that large increases in GDP within a country are positively associated with reading test scores, however this result is only marginally significant. Perhaps this finding is because wealth and resources can increase the quality of educational institutions and ultimately the well-being, and test scores, of children. In addition, students in households with higher incomes are more likely to learn vocabulary and grammar in the home (Dahl & Lochner, 2012). As we would expect, infant mortality rates within a country are significantly negatively related to all three types of PISA scores. This particular variable may be capturing many unobservable characteristics within a country that are negatively associated with the well-being of the students and educators, such as disease or poverty-level shifts. If students and educators have to deal with these negative shocks, they will probably have less time and ability to focus their efforts on a successful education.

The models do not detect many significant effects of the control variables used, perhaps because there is not much variation in these factors over time. In other words, it may be that many of the control variables could be considered as country-level fixed effects. Furthermore, since these control variables could simply result in a power issue, the previous simpler model may be preferred.

Results for OECD and non-OECD subgroups are found in Table 4 below. The statistically significant results for the non-OECD countries suggest that the overall results may be driven largely by non-OECD countries; however, the results for OECD countries face a substantial power issue. As shown in Table 1, only 18 percent of the original 214 observations are from OECD countries, and OECD nations have less than half of the amount of private schooling variation observed within non-OECD countries. In fact, the within-country standard deviation for private schooling is around 3.29-percentage points in non-OECD countries, while it is only 1.37-percentage points in OECD countries. Alternatively, it may be that public schools are less competitive, relative to private schools, in developing countries than developed countries.

Table 3: The Effect of Private Schooling on PISA Scores

	Math	Reading	Science
Private Share	**1.43****	**1.05***	**0.88**
	(0.03)	**(0.10)**	**(0.11)**
GDP (Hundreds of Billions)	0.00	0.00	0.00
	(0.88)	(0.11)	(0.61)
GDP^2 (Hundreds of Billions)	0.00	-0.00	0.00
	(0.89)	(0.65)	(0.97)
Govt Expend	-1.07	-0.64	-0.17
	(0.25)	(0.46)	(0.83)
Population (Hundreds of Millions)	0.04	-0.08	-0.12
	(0.77)	(0.48)	(0.21)
$Population^2$ (Hundreds of Millions)	-0.00	0.00	0.00
	(0.80)	(0.86)	(0.57)
Enrollment (Hundreds of Millions)	0.00	0.00	-0.00
	(0.76)	(0.73)	(0.65)
$Enrollment^2$ (Hundreds of Millions)	-0.00	0.00	-0.00
	(0.33)	(0.91)	(0.78)
Life Expectancy	-1.62	-0.53	0.20
	(0.42)	(0.78)	(0.90)
Infant Mortality	-2.82***	-2.49***	-1.33**
	(0.00)	(0.00)	(0.03)
Constant	611.77***	551.02***	519.17***
	(0.00)	(0.00)	(0.00)
R-Squared Within	0.28	0.28	0.20
Countries	63	63	63
N	214	212	212

Note: P-values in parentheses. All models include country and year fixed effects.
* p<0.10, ** p<0.05, *** p<0.01

Table 4: Heterogeneous Effects (Year and Country Fixed Effects)

	Math	Reading	Science
Non-OECD	**2.83****	**3.02****	**1.19**
	(0.02)	**(0.01)**	**(0.25)**
OECD	**0.31**	**-0.15**	**0.40**
	(0.71)	**(0.85)**	**(0.55)**
Controls	Yes	Yes	Yes
R-Squared Within	0.31	0.33	0.21
Countries	63	63	63
N	214	212	214

Note: P-values in parentheses. All models include country and year fixed effects and all added controls. Coefficients are for private schooling in OECD and Non-OECD countries. Each model uses a term that interacts the private share of schooling with OECD status for each observation.
* p<0.10, ** p<0.05, *** p<0.10

Section 7.3: 2SLS Regression with Year and Country Fixed Effects

For each of the three regressions, the instrument is strongly associated with the private share of total schooling enrollment. As shown in Table 5A, the coefficient is around -0.87 in the first stage of each model. In other words, a one percentage point increase in child share of the total population is associated with a 0.87 percentage point reduction in private schooling. This is evidence to confirm the hypothesis that private schools are less able to absorb short-run demand shocks of students than public schools. The instrument is also redundant since child share of population should not directly influence a nation's standardized test scores within a given year. In fact, when I include this as a control in the structural model, the p-value associated with the instrument is above 50 percent for math and reading scores. However, I do find a statistically significant negative relationship between the instrument and reading PISA scores. Although this empirical relationship emerges, the relationship between the child share of total population and PISA reading scores does not follow intuition and therefore could be a false positive finding.

I present the results for the second stage of the instrumental variables fixed effects regression in Table 5B below. The p-value for math jumps to around 20 percent and the coefficient is similar to before, which could be an indication that the model is suffering from a lack of power, which is not uncommon for a 2SLS model with a sample size of only around 214. Conversely, the result for reading scores becomes more statistically significant while the effect size increases to around a half of a standard deviation. The effect for science attenuates towards zero. It may be that the instrument is only redundant to the models for math and science. However, intuitively, the instrument is more exogenous to the models than private schooling itself. Results from all models can be found in Table 6 below.

Table 5A: The Effect of Private Schooling on PISA Scores (1st Stage)

	Private (Math)	Private (Reading)	Private (Science)
Child Share	**-0.87***** **(0.00)**	**-0.88***** **(0.00)**	**-0.87***** **(0.00)**
Controls	Yes	Yes	Yes
R-Squared Within	0.43	0.43	0.43
Countries	63	63	63
N	214	212	214

Note: P-values in parentheses. All models include country and year fixed effects with all added controls. All results are from the first stage of the IV fixed effects models.
* p<0.10, ** p<0.05, *** p<0.01

Table 5B: The Effect of Private Schooling on PISA Scores (2nd Stage)

	Math	Reading	Science
Private Share	**2.45**	**4.88****	**0.09**
	(0.20)	**(0.02)**	**(0.95)**
Controls	Yes	Yes	Yes
Constant	596.87***	494.89***	530.59***
	(0.00)	(0.01)	(0.00)
R-Squared Within	0.27	0.09	0.19
Countries	63	63	63
N	214	212	214

Note: P-values in parentheses. All models include country and year fixed effects with all added controls. All results are from the second stage of the IV fixed effects models.
* p<0.10, ** p<0.05, *** p<0.01

Table 6: Overall Results by Method

	Math	Reading	Science
No Controls	**2.51*****	**1.46****	**1.33*****
	(0.00)	**(0.02)**	**(0.01)**
Controls	**1.43****	**1.05***	**0.88**
	(0.03)	**(0.10)**	**(0.11)**
Instrumental Variable – 2SLS	**2.45**	**4.88****	**0.09**
	(0.20)	**(0.02)**	**(0.95)**
Countries	63	63	63
N	214	212	214

Note: P-values in parentheses. All models include country and year fixed effects.
* p<0.10, ** p<0.05, *** p<0.01

Section 8: Conclusion and Policy Implications

The model using control variables and year and country fixed effects finds that a one percentage point increase in the private share of schooling enrollment is associated with a 13 percent of a standard deviation increase in math scores, an 10 percent of a standard deviation increase in reading scores, and a 10 percent of a standard deviation increase in science scores; however, these results are statistically insignificant for science, with a p-value of 0.11. In addition, the only results that are robust to the instrumental variables analysis are for reading scores. It may be that private schools improve reading skills by exposing children to advantaged peers that have strong vocabularies. It may also be that schools with specialized missions have an advantage at getting children to take interest in the class reading material. It should also be noted that – as shown in table A1 in the Appendix – the instrumental variable is trending downward over time for almost every country included in the study sample. The observed downward trend is a limitation of this study, as the instrumental variable may be correlated with societal factors that are also consistently trending downward over time. However, this study controls for some macro-level changes in societal factors over time by including year-fixed effects in each analytic model.

Since private schooling may increase scores on international assessments, we should consider promoting policies that increase private schooling within countries. Specifically, decision-makers should consider expanding access to private schooling through private school choice programs such as vouchers, tuition-tax credit scholarships, and education savings accounts. Each of these programs would expand the share of private schooling and competitive pressures within a country. However, decision-makers must realize that there may be heterogeneous effects across countries. Policymakers should increase the amount of data available on private schooling around the world so that researchers could examine differential

impacts for subgroups. Specifically, city-level data provided by institutions such as the World Bank would allow for enough statistical power to detect effects for different regions of the world and different types of countries. In addition, policymakers should also consider the preponderance of the evidence on this subject, especially since this is the first study using these methods to determine the effect of private schooling on test scores.

To increase the supply of private schooling options, policy-makers may also consider reducing regulatory costs for private schools to participate in school choice programs. After all, recent studies in the U.S. suggest that large packages of regulations may decrease the quality level (Sude, DeAngelis, & Wolf, 2018) and specialization (DeAngelis & Burke, 2017) of the supply of private schools. Finally, since test scores are not always good proxies for long-term outcomes (Greene, 2016), policymakers should consider these results alongside evaluations of the effects of private schooling on non-academic outcomes (DeAngelis, 2017; DeAngelis & Wolf, forthcoming 2018; Wolf, 2007) such as civic engagement (Carlson, Chingos, & Campbell, 2017; Fleming, 2014; Fleming, Mitchell, & McNally, 2014), criminality (DeAngelis & Wolf, 2016), charitable activity (Bettinger & Slonim, 2006), and effort (DeAngelis, 2017).

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Chapter 1: Appendix

Table A1: Child Share of the Population over Time (Percent)

Country	2000	2003	2006	2009	2012
Australia	21	20	20	19	19
Austria	17	16	16	15	14
Azerbaijan	31	28	25	23	22
Belgium	18	17	17	17	17
Brazil	30	28	27	26	24
Bulgaria	16	14	13	13	14
Canada	19	18	17	17	16
Chile	27	25	24	22	21
Colombia	32	30	28	27	25
Costa Rica	31	29	27	25	24
Croatia	17	16	15	16	15
Cyprus	22	21	20	18	17
Czech Republic	16	15	15	14	15
Denmark	18	19	19	18	18
Estonia	18	16	15	15	15
Finland	18	18	17	17	16
France	19	19	18	18	19
Germany	15	15	14	14	13
Greece	15	15	15	15	15
Hungary	17	16	15	15	15
Iceland	23	23	22	21	21
Indonesia	31	30	30	29	28
Ireland	21	20	20	21	21
Israel	28	28	28	27	28
Italy	14	14	14	14	14
Japan	15	14	14	13	13
Jordan	39	38	37	37	36
Kazakhstan	28	26	24	24	25
Kyrgyzstan	35	33	31	30	30
Latvia	18	16	15	14	14
Lithuania	20	18	16	15	14
Luxembourg	19	19	18	18	17
Malaysia	34	32	30	28	26
Mexico	34	33	32	30	29
Netherlands	18	18	18	18	17
New Zealand	23	22	21	21	20
Norway	20	20	19	19	18
Panama	32	31	30	29	28
Peru	34	33	31	30	29
Poland	19	17	16	15	15

Table A1 (Continued)

Country	2000	2003	2006	2009	2012
Portugal	16	16	15	15	15
Qatar	26	24	20	14	14
Romania	19	17	16	16	16
Russia	18	16	15	15	15
Serbia	21	20	18	18	17
Singapore	21	20	19	18	17
Slovakia	20	18	16	15	15
Slovenia	16	15	14	14	14
South Korea	21	20	18	17	15
Spain	15	14	14	15	15
Sweden	18	18	17	17	17
Switzerland	17	17	16	15	15
Thailand	24	23	21	20	19
Trinidad and Tobago	26	23	21	21	21
Tunisia	30	27	25	24	23
Turkey	31	29	28	27	26
United Arab Emirates	26	21	17	14	14
United Kingdom	19	18	18	18	18
United States	21	21	20	20	19
Uruguay	25	24	24	23	22
Vietnam	32	29	26	24	23
Average	23	21	20	20	19

Chapter Two

Does Private Schooling Affect Non-Cognitive Skills?

International Evidence Based on Test and Survey Effort on PISA

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Abstract

I use Programme for International Student Assessment (PISA) data from over 300,000 individual students within 44 countries in 2009 and a historical natural experiment to estimate the causal impact of private schooling on student effort. Since nations with larger shares of Catholics in 1900 tend to have larger shares of private schooling today, the study uses the Catholic share of the population in 1900 as an exogenous instrument to predict whether a given child is in a private school in 2009. The results suggest that private schooling increases student effort on PISA tests, as measured by test decline, while decreasing diligence on student surveys, as measured by careless answer patterns and non-response rates. In addition, I find that private schooling substantially increases PISA test scores, and that stronger non-cognitive skills are associated with higher PISA scores.

Keywords: private school; school choice; non-cognitive skills; civic education; character education

Section 1: Introduction

Plenty of literature examines the impacts of school choice and private schooling on the academic abilities of children; however, not much has been done to determine the effects of private schooling on non-cognitive skills. To my knowledge, this is the first study to estimate the causal impact of private schooling on non-cognitive skills as measured by effort on tests and answering patterns on long student questionnaires. In theory, private schools facing competitive pressures are incentivized to provide a higher quality educational service. If non-cognitive skills such as effort, diligence, and respect are included in families' perceptions of quality, we may expect that private schools could have a positive impact on related measures.

On the other hand, if individual families do not value the skills that are being captured by test and survey effort measures, we would expect to find negative effects. If a certain skill has very little benefits accrued to the individual family unit and large benefits accrued to others in society, we would also expect to find negative effects of private schooling on certain character skills such as respect. This result is because skills with large positive externalities may be under consumed if voluntarily selected by self-interested families (Coase, 1937; Pigou, 1932). Correspondingly, traditional public schools around the world were created in order to subsidize skills with theoretically large positive externalities such as respect for others (Gatto, 2002; Fichte, 1993; Mann, 1855). Prussia first implemented the modern compulsory education system in 1763 and strengthened its focus on social cohesion in the early 19th century after its defeat in the Napoleonic Wars. This system quickly spread throughout the rest of the world and was adopted in Massachusetts in 1852 (Tyack, 1974). Since the world's modern system of public schooling has historically placed much weight on social cohesion, it is possible that public schools are more effective in shaping the non-cognitive skills that primarily benefit others in

society. However, private schools may still have an advantage at shaping skills that benefit others through competitive pressures. Because of the competing theories, it is not clear whether private schooling improves all types of student effort.

Importantly, this study examines the effects of private schooling on non-cognitive skills. One non-cognitive measure, effort on Programme for International Student Assessment (PISA) tests, captures a skill that largely benefits individual students. The other non-cognitive measure, effort on PISA student surveys, captures a skill that largely benefits the broader society. Of course, performance on PISA tests and long PISA surveys do not have any bearing on the long-run outcomes of the students. However, a given student is more likely to perceive that their performance on the PISA exam has a substantial effect on their later life outcomes than the long PISA survey, especially since students are conditioned to perform well on various academic tests throughout their entire K-12 educational experience. For at least thirteen years, children learn that performing well on academic exams translates to grades that impact their grade point average, college admissions, and lifetime earnings. Since students are less likely to make that connection with a long survey, performing well on the PISA survey requires a larger degree of respect of teachers or other authority figures. Further, students are not even aware that their effort on long surveys can be evaluated by metrics such as non-response and carelessness. The study takes advantage of a natural experimental setting around the world to examine the effects of private schooling on non-cognitive skills such as test and survey effort. Of course, it is uncertain whether private schooling will improve or reduce skills such as respect. While positive externalities exist for skills such as respect, competitive pressures could still lead to higher levels of both test and survey effort if individual schooling decisions strongly consider benefits that

accrue to other members of society. As supplementary analyses, the impacts of private schooling on PISA test scores and the effects of non-cognitive skills on PISA scores are estimated.

Section 2: Literature Review

The literature on how access to private schooling can affect student test scores is extensive. Of the 20 existing random assignment studies of the effects of private school choice on student achievement, only two have found negative impacts in the final year of their analyses (Abdulkadiroglu, Pathak, & Walters, 2018; Dynarski et al., 2017). A recent meta-analysis of 19 of these experimental studies finds that private school voucher programs have small positive impacts on student test scores around the world (Shakeel, Anderson, & Wolf, 2016). Shakeel, Anderson, and Wolf (2016) find larger positive impacts for reading than for math, for international programs than for those in the United States, and for programs that are publicly funded than those that are privately funded. Further, private school choice studies often find evidence to suggest that voucher programs are better at shaping student test scores after a few years of use (Mills & Wolf, 2017; Shakeel, Anderson, & Wolf, 2016; Waddington & Berends, 2017)

The studies examining longer-term cognitive outcomes such as high school graduation are less abundant; however, most of these have found large positive effects. Wolf et al. (2013) took advantage of a randomized lottery-admissions process to find that students using a voucher in the D.C. Opportunity Scholarship Program had a 21-percentage point higher likelihood of graduating. Other studies in the U.S. find similar large positive impacts on attainment (Chingos & Peterson, 2015; Cowen et al., 2013; Neal, 1997; Warren, 2008; Wolf, Witte, & Kisida, 2018). One recent experimental study finds that the D.C. Opportunity Scholarship Program had no effects on college enrollment (Chingos, 2018).

Most relevant to this paper are the studies that have examined the impacts of school choice on non-cognitive outcomes of students such as tolerance of others, civic engagement, and social order. In his review of the literature, DeAngelis (2018) found eleven such studies, and none of them determined that access to private schooling harmed societies overall. Half of the studies examining tolerance found that private school choice improved tolerance levels (e.g. Campbell, 2002; Wolf, Peterson, & West, 2001), while the remaining half found no significant difference (e.g. Fleming, Mitchell, & McNally, 2014). Three of five studies found that private school choice boosted civic engagement through increased political participation, volunteering, and charitable activity (Bettinger & Slonim, 2006; Fleming, 2014; Fleming, Mitchell, & McNally, 2014). Furthermore, the only reviewed study on social order found that school choice led to less criminal activity (DeAngelis & Wolf, 2016).

Few studies have successfully determined the impact of private schooling on student outcomes across the world. D'Agostino (2016) examined the private share of schooling enrollment in thirty countries in 2012, but did not find a statistically significant impact on Programme for International Student Assessment (PISA) scores. Sakellariou (2017) examined schooling in forty countries in 2012 and found that public schools outperformed private on PISA scores. Dronkers and Robert (2008) found that government-dependent private schools outperformed comparable public schools on PISA scores in 2000, while public schools outperformed comparable government-independent private schools. However, since these three studies simply used cross-country comparisons, they cannot be interpreted as causal.

The first study to find a causal relationship between privates schooling and PISA scores took advantage of a natural experiment. West and Woessmann (2010) had access to the proportion of the population that identified as Catholic in 1900 and used that measure as an

exogenous instrument to predict whether an individual student was in a private school over a hundred years later, in 2003. Using this experimental setting, West and Woessmann (2010) found that private schooling had substantially large impacts on individual student PISA scores in 2003. Heller-Sahlgren (2018) applied the same instrumental variable for the sample of around 295,000 children from 34 OECD countries that took the 2012 PISA exam and survey. Heller-Sahlgren (2018) similarly found that private schooling largely increased PISA math, reading, and science scores; however, he also found that private schooling slightly reduced student well-being at school, as reported on the PISA survey. DeAngelis (2017) found similar positive results for math, reading, and science scores by using county-level fixed effects over twelve years and a new instrumental variable, short-run fluctuations in the demand for schooling overall within nations over time. However, these previous studies all focus on cognitive skills that accrue to individual students, so the results are not particularly surprising.

This study adds to the literature in at least four significant ways: (1) this is the first study to estimate potentially differential impacts of private schooling on non-cognitive skills with benefits for individuals (i.e. effort on the PISA test) and third parties (i.e. effort on the PISA student survey); (2) this study is the first to estimate the effect of private schooling on non-cognitive skills as measured by test decline and survey response patterns in 44 countries around the world; (3) effects of private schooling on student-level PISA scores after controlling for three measures of non-cognitive skills are examined; and, (4) the relationships between three measures of students' non-cognitive skills and their performance on the PISA exam are examined.

Section 3: Theory

The strongest arguments for how private schooling institutions could affect non-cognitive skills are associated with competitive pressures, specialization, and the potential conflict between the goals of individuals and the societies in which they reside.

The first theory is the simple economic argument that competition leads to enhanced levels of quality as defined by the customer. In the case of the market for education, individual families are the consumers while individual schools are the producers (Neal, 2002). If families can choose the school that best fits their definition of quality at a reasonable price, competitive pressures will lead to lower tuition levels and higher school quality (Hoxby, 2003). If, on the other hand, families face exorbitant transaction costs associated with switching schools, the producers of education will hold large amounts of monopoly power (Friedman & Friedman, 1990). Since power is often highly concentrated in this way in a system of public schooling, most economists would expect low levels of quality alongside high prices (Chubb & Moe, 2011). In addition, since a market of private schooling forces institutions to cater to the needs of individuals, it can lead to increased levels of specialization and enhanced quality simply through an improved match between educator and student (DeAngelis & Holmes Erickson, 2018). Of course, access to private schooling would only improve measures that capture how individual families define quality. Since families value skills such as effort, I expect to find more effort exhibited by students in private schools.

As noted, the results will all depend on the values of individual families and the societies in which they reside. Individual family values are conveyed through choices made among alternative options in order to maximize household utility. On the other hand, social values are usually demonstrated through political participation by citizens and decision-making by

representatives. An effect could be detected simply if the summation of individual utility-maximizing decisions is at odds with the collection of decisions that are made by democratically elected officials.

Since individual families make private school choice decisions, scholars argue that those decisions could be at odds with social goals. According to economists, since the transaction is only between a school and an individual family, the voluntary exchange will occur if each party perceives individual benefits to be greater than individual costs (Buchanan & Tollison, 1984). Since total costs are in excess of costs accrued to the individual, even an economically inefficient transaction would occur (when total costs exceed total benefits). In other words, there may be positive or negative externalities to schooling that are not fully considered by individuals (Hall, 2006; Pigou, 1932). As a result, a system of private schooling may be more likely to improve non-cognitive skills largely accrued to the individual (e.g. determination) than it is to improve non-cognitive skills that largely benefit third parties (e.g. respect). However, it is still plausible that private schooling could lead to higher levels of non-cognitive skills that have significant positive spillover effects because of stronger competitive pressures.

Since the survey effort measures may be more likely to capture the skill of respect, it is less clear whether private schools will have an advantage at shaping survey effort. On the other hand, since effort on tests has a stronger intuitive relationship with cognitive skills and students' long-term outcomes, I expect to find a private school advantage for shaping student effort on tests. While the motivation to exert effort on a test is primarily linked to perceived long-term rewards to the individual, the motivation to exert effort on a long survey may be primarily linked to short-run rewards to the authority figure.

Section 4: Data

I have access to student level data from the 2009 PISA for 300,628 15-year-old children in 44 nations across the globe. Following Zamarro, Hitt, and Mendez (2016), although 74 countries participated in PISA in 2009, the current analysis is limited to the 44 countries that took the traditional booklet versions that had more rigorous questions than the versions given to the historically lower performing countries. Since these booklet tests were more difficult, readers can be more confident that this study captures individual student effort throughout the assessment. New countries with low initial scores, and those historically performing at lower test score levels, were offered the opportunity of taking an easier set of test booklets with the aim of better capturing lower levels of performance. The current analysis focuses on those countries that took the traditional, more difficult, version of the exam. The PISA exam is coordinated by the Organization for Economic Cooperation and Development (OECD) and is given to 15-year-old students every three years. The subjects tested include reading, math, science, problem solving and financial literacy. The PISA assessment has two parts: the actual exam and then a survey that is distributed after the test. This analysis uses both parts to capture different forms of student effort.

I use 58 control variables, listed in Appendix B, at the student, school, and country level from the PISA questionnaires and the World Bank. The three dependent variables of interest all come from students' PISA exams and surveys. I take advantage of student test decline, careless answering patterns, and survey non-response rates in order to construct the three measures of non-cognitive skills. In addition, data on the Catholic share of the population in 1900 and 2010 are gathered from Davis and James (2015) in order to construct the instrumental variable. A

supplementary analysis of the effects of private schooling on PISA math, reading, and science scores is also provided.

Section 5: The Measures

I will now present the three measures of non-cognitive skills used: test decline, non-response rates, and careless answering.

Section 5.1: Test Decline

Scholars have found that student performance on PISA exams has declined from the beginning to the end of the assessment (Balart, Oosterveen, & Webbink, forthcoming; Borghans & Schils, 2012). Importantly, the order of the questions across PISA booklets was randomized in 2009. Additionally, booklets are randomly assigned to individual students, so item position varies in its test position across students. The random assignment of questions is imperative for this study since it suggests that student effort is being measured. For example, if problems that are more difficult were systematically assigned to the end of test booklets, we would not be able to determine if test decline was caused by declining effort or question difficulty. Consequently, a student with no change in motivation across test questions, on average, will have the same probability of correctly answering a question regardless of its position on the assessment. In addition, since the PISA 2009 booklet had around 60 questions, and was expected to take around two hours to complete, we can be confident that getting through the relatively long exam requires students to exert non-cognitive skills.

The test decline measure is calculated as the total number correct on the first ten questions on the PISA exam minus the total number correct on the last ten questions. In other words, a positive value for this variable indicates that student effort declined from the first half to the second half of the assessment. In the sample, this measure had a mean of about 1.37 and a

standard deviation of 2.36. In other words, the average student answered 1.37 fewer questions correct on the last ten than on the first ten. This matches the methodology used in Zamarro, Hitt, and Mendez (2016).

Section 5.2: Non-Response Rates

Item non-response rate is the rate at which students skip questions or provide the answer "I don't know" on the hour-long student questionnaire after the assessment. Since PISA surveys rarely ever use "I don't know" as a plausible answer choice, I calculate non-response rates as the rate at which students skip questions. This calculation is simple: it is the number of skipped questions divided by the total number of possible responses. In the sample, the average non-response rate is 3.03 percent, with a standard deviation of 4.14 percent across students. In other words, the average student skipped 3.03 percent of the questions on the survey. In 2009, the PISA survey had around 170 questions, so the average student in the overall sample skipped about 5 survey questions. The lowest non-response rate for a student was 0 percent and the highest was 81.6 percent.

This measure captures student effort on the long student survey component of PISA. Since a student survey is not a reflection of the students' actual academic abilities, children will have less of an incentive to exert effort on surveys overall. However, if school leaders and teachers condition children to value respecting authority figures, students will have more of an incentive to exert effort on long surveys.

Section 5.3: Careless Answering

Obviously, some students may choose to answer questions carelessly rather than skipping them. For example, a given student could simply fill in the bubbles closest to the right side of the page.

Similarly, careless students could bubble in patterns in a zigzag pattern. In order to capture this type of behavior empirically, this study employs a method developed by Hitt (2016).

One way to do so is to follow psychometric tests such as Cronbach's alpha. This measure takes related questions and examines whether students respond to them similarly. For example, one item may state, "School has done little to prepare me for adult life when I leave school," while another may state, "School has taught me things which could be useful in my job." A careless student just bubbling down the right side of the page, marking "strongly agree," to both would lead to a lower Cronbach's alpha. In addition, researchers have used correlations between item-answers on a given scale to identify careless answering patterns. After all, items within a given scale should be correlated within student observations for each particular scale.

Following Hitt (2016), I construct a bivariate regression for each Likert-type item on the PISA Student Survey. I examine 84 items across 12 different scales using the following equation:

$$Y_{ijs} = \beta_0 + \beta_1 X_{is,-j} + \varepsilon_{ijs}$$

Where Y_{ijs} is the answer provided by student *i* on item *j* for scale *s*. The coefficient of interest, β_1, captures the average of the rest of the items in scale s (other than j) by student i. These regressions are equivalent to the item-rest correlations used in psychometrics (Hitt, 2016). For the measure of interest, I store the estimated student level residuals, ε_{ijs}. This residual term captures the extent to which a given student, *i*, provided unpredictable answers on their surveys. The absolute value of each residual is then standardized to have a mean of zero and standard deviation of one. The standardized scores are averaged for each student observation in order to create a composite score for careless answering. A higher score here means that a given student was not giving consistent answers within scales over the survey. The study sample has a mean

careless answering composite score of negative 3 percent with a standard deviation of 25 percent across students.

This measure captures student effort on the long student survey component of PISA. Since a student survey is not a reflection of the students' actual academic abilities, children will have less of an incentive to exert effort on surveys overall. However, if school leaders and teachers condition children to value respecting authority figures, students will have more of an incentive to exert effort on long surveys. Table 1 below shows the descriptive statistics for the analytic sample.

Table 1: Descriptive Statistics

Variable	N	Mean	Std. Dev.	Min	Max
Test Decline	300,628	1.37	2.36	-9.75	11.82
Careless Answer	300,569	-0.03	0.25	-0.90	3.71
Non-Response	300,628	3.03	4.14	0.00	82.60
Private	301,529	0.19	0.39	0.00	1.00
Plausible Math	301,529	502.65	96.48	46.59	1022.21
Plausible Reading	301,529	496.52	93.26	6.65	871.12
Plausible Science	301,529	505.62	95.67	10.95	883.76
Catholic Share 2010	301,529	0.37	0.33	0.00	0.88
Catholic Share 1900	301,529	0.46	0.42	0.00	1.00
Age	301,529	15.77	0.29	15.17	16.33
Female	301,529	0.50	0.50	0.00	1.00
Father Degree	301,529	0.35	0.48	0.00	1.00
ESCS (SES)	300,199	-0.05	1.00	-6.04	3.41
Country	301,529	22.28	13.77	1.00	44.00
Student/Teacher Ratio	301,529	8.34	5.96	0.00	21.00
Teacher Salary ($)	301,529	21,474	18,222	0.00	71,508.00
GDP Per Capita ($)	280,177	35,722	18,352	2,263	125,108.00

Section 6: Methods

I first employ an ordinary least squares regression (OLS) approach of the form:

$$Outcome_{it} = \beta_0 + \beta_1 \boldsymbol{Private}_{it} + \beta_2 Country_{it} + \beta_3 School_{it} + \beta_3 Student_{it} + \varepsilon_{it} \quad (1)$$

Where *Outcome* is one of the three dependent variables of interest for student *i* in year 2009. The three dependent variables of interest are test decline, careless answering, and non-response rates. For each of these outcomes, lower values are associated with higher non-cognitive skills demonstrated by individual students.

Private is the binary independent variable of interest, taking on the value of one if student *i* is in a private school in 2009. The coefficient of interest, β_1, is expected to be negative for the test decline outcome measure, indicating enhanced effort on individual student assessments for private schools students. On the other hand, β_1 is expected to be positive for the careless answering and non-response measures, indicating less compliance on long surveys for students in private institutions.

I also include vectors of 58 controls at the country, school, and student level. These include GDP per capita, student – teacher ratio, Catholic share of population in 2010, principal non-response rate, teacher salary, country-level education, student immigrant status, student age, student gender, student house location, number of books in the household, mother and father's education level, mother and father's profession, and student test booklet ID. A complete list of these 58 control variables appears in Appendix B. Since private schools differ across countries, a model that uses country-level fixed effects is included in order to control for any remaining unobserved nation-level characteristics and compare students within countries.

Since whether a student is in a private school in 2009 is obviously endogenous to the OLS model, I follow the literature by West & Woessmann (2010), Heller-Sahlgren (2018), and others by using a two-stage-least-squares regression (2SLS) approach of the form:

$$Private_{it} = \lambda_0 + \lambda_1 \boldsymbol{CatholicShare}_{j1900} + \lambda_2 Country_{it} + \lambda_3 School_{it} + \lambda_3 Student_{it} + \varepsilon_{it} \quad (2)$$

$$Outcome_{it} = \beta_0 + \beta_1 \boldsymbol{Private}_{i2009} + \beta_2 Country_{it} + \beta_3 School_{it} + \beta_3 Student_{it} + \varepsilon_{it} \quad (3)$$

Where the endogenous explanatory variable of interest, *Private*, is predicted with the Catholic share of the population in 1900 in country *j*, where the student *i* resides in present day. A linear probability model is used in the first stage since the endogenous explanatory variable is binary. However, all results are robust to models using probit or logit in the first stage, as shown in Table A.3 in Appendix A. If the Catholic share of the population in 1900 within a country is higher, the probability that the Catholic population set up a system of private schooling over a hundred years ago is also higher. As a result, I expect that the coefficient on the instrument will be positive and significant in the first stage. This expectation is only likely to hold, however, if Catholicism was not the mandated state religion in 1900. If Catholicism was the mandated state religion within the country in 1900, the public schools would be Catholic-centered, and the Catholic population would not have the need to set up a system of private schools. Consequently, this part of the analysis is limited – using information from Barro and McCleary (2005) – to countries that did not have a Catholic-mandated state religion in 1900.

This instrument is exogenous since it is not correlated with any omitted variables that would otherwise bias the estimates. In other words, the only way that the Catholic share of the population in 1900 could affect the three outcomes of interest is through affecting the probability that a child, *i*, in a given country, *j*, would end up in a private institution over a hundred years later, in 2009. One could plausibly argue that the Catholic share of the population in 1900 is correlated with Catholic share of the population in present day, resulting in a potentially invalid instrument. Nevertheless, all models also control for the present-day (2010) share of the population that identifies as Catholic. In addition, several other scholars have used this

instrumental variable as well (Allen & Vignoles, 2015; Cohen-Zada, 2009; Cohen-Zada & Elder, 2009; Falck & Woessmann, 2013; Heller-Sahlgren, 2018; West & Woessmann, 2010).

Even if the instrument were to be invalid due to other omitted variables related to the cultural values of different religiosity, we should expect the resulting bias to make it more difficult to find benefits of private schooling. Literature indicates that Protestants, relative to Catholics, have placed more importance on education historically (Green, 1979; Rupp, 1996; Becker & Woessmann, 2009). Additionally, West and Woessmann (2010) point out that the Catholic share of the population is negatively associated with literacy rates and GDP per capita in 1900. Consequently, if any bias does exist, the estimates that favor public schooling would be larger in magnitude, or even the incorrect sign. However, the estimates that favor private schooling would be considered conservative.

Finally, a model is provided which estimates the effects of private schooling on student PISA math, reading, and science scores. The instrumental variables specification is employed and all of the controls included in the previous models are included. I expect that these results will align with those published by West and Woessmann (2010) and Heller-Sahlgren (2018). All reported results are for models with standard errors clustered at the level of treatment, the school (Barrios et al., 2012; Cameron & Miller, 2015; Green & Vavreck, 2007).

Section 7: Results

I now present results for the OLS and 2SLS estimates of the effects private schooling on non-cognitive skills. In addition, I present the results from models estimating the effects of private schooling and students' non-cognitive skills on test scores.

Section 7.1: OLS Estimates

Tables 2 and 3 illustrate the OLS estimates. Both models indicate that being in a private school leads to lower levels of test decline, even after adding all 58 controls. This effect is equivalent to around a 5 percent of a standard deviation decrease in test decline in the ordinary regression model, and a 3 percent of a standard deviation decrease in the model including country fixed effects, indicating higher levels of student effort on the PISA exam due to private schooling. The results for careless answering are not statistically different from zero, while the first model finds that private schooling leads to a 0.3 percentage point increase in levels of non-response rates. This effect is around an 8 percent of a standard deviation increase in student non-response, indicating that private schooling decreases student effort on lengthy questionnaires. These results may seem counterintuitive at first, as they are in opposite directions. However, effort on surveys may require different types of non-cognitive skills than effort on tests. Effort on surveys may rely more on respecting authority figures such as teachers. Alternatively, it may be that effort on tests relies more on cognitive skills than effort on surveys. Consequently, the test-decline measure may capture more cognitive skills than the other two measures. However, all models include student PISA math, reading, and science exam scores as controls for cognitive abilities. It may also be that additional regulation within the public sector means that paperwork is being completed more often by students. If this is the case, the public school advantage may be driven partially by public school students having more training with filling out long surveys. All of these results are also robust to models that cluster standard errors at the country level.

Coefficients on controls usually behave as expected where significance arises. Female students displayed more non-cognitive skills on all three measures. Students with higher PISA math, reading, and science scores generally exhibited more effort on the surveys based on both

measures included. Students with a higher socio-economic status experienced less test decline and demonstrated less careless answering, but they were also more likely to skip survey questions. Students with college-educated parents were less likely to experience test decline and skipped fewer survey questions, but they demonstrated more careless answering on the survey. Students that were immigrants, and students with parents who were immigrants, generally demonstrated lower levels of effort based on all three measures.

Table 2: Private School Impacts on Non-Cognitive Skills (OLS)

	(1) Test Decline	(2) Careless Answer	(3) Non-Response
Private	**-0.123***** **(0.000)**	**0.001** **(0.779)**	**0.328***** **(0.000)**
Controls	Yes	Yes	Yes
R^2	0.018	0.032	0.133
Countries	40	40	40
Clusters	1,097	1,097	1,097
Students	277,997	277,942	277,997

Notes: P-values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors clustered at the school level. All models use 58 country, student, and school level controls. All results are also robust to models that cluster standard errors at the country level.

Table 3: Private School Impacts on Non-Cognitive Skills (OLS Country Fixed Effects)

	(1) Test Decline	(2) Careless Answer	(3) Non-Response
Private	**-0.070***** **(0.000)**	**0.003** **(0.150)**	**-0.051** **(0.212)**
Controls	Yes	Yes	Yes
R^2	0.027	0.079	0.148
Countries	44	44	44
Clusters	1,097	1,097	1,097
Students	299,317	299,262	299,317

Notes: P-values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors clustered at the school level. All models use country fixed effects and student and school level controls. All results are also robust to models that cluster standard errors at the country level.

Section 7.2: 2SLS – Catholic Share in 1900

Tables 4A and 4B show the results from the two-stage-least-squares regression model. As expected, the Catholic share of the total population in 1900 is significantly positively related to whether a given student is in a private school in the present day. In all models, the instrument has a p-value of approximately zero. In addition, a 10-percentage point increase in historical Catholic share of the population is associated with a 1.5-percentage point higher likelihood of a student being in a private school in 2009. The relationship between the historical Catholic share of the population within each country and its current-day share of private schooling can be found Table A3 in Appendix B.

The coefficients are all in the same direction as before; however, they become significantly larger in magnitude than in the OLS model, perhaps because the instrument removed attenuation bias from each model. It may be the case that private schooling's effect on test score effort is negatively biased in the OLS model because Catholicism is generally negatively correlated with academic outcomes (Becker & Woessmann, 2009; Green, 1979; Rupp, 1996; West & Woessmann, 2010), while private schooling's effect on survey effort may be positively biased in the OLS model because Catholicism is generally positively correlated with deference to authority (McGreevy, 2004). Here, private schooling is associated with around a 76 percent of a standard deviation decrease in test decline, a 1.6 standard deviation increase in careless answering, and 2.4 standard deviation increase in non-response rates. As expected, private schooling is associated with large increases in effort on assessments that aim to measure student achievement. On the other hand, private schooling decreases effort on lengthy questionnaires. This result may highlight the intuition that private schools are associated with shaping skills with larger private benefits, while public schools improve children's abilities to

please authority figures in order to create elevated social cohesion. Alternatively, the measure of test-decline may very well be capturing more cognitive skills than the other two. After all, a student that has strong academic abilities may not find the exam difficult, and therefore may not need to work very hard to get through the PISA assessment.

As shown in Table 5, the results indicating enhanced levels of test effort are robust to all model specifications, while the results indicating lower survey effort levels depend on the method used. Nonetheless, the preferred instrumental variable model shows that private schooling leads to less testing decline, more careless answering on surveys, and higher non-response rates on surveys.

Table 6 shows results from the preferred instrumental variables specification for various subgroups. For all but 2 of the 24 subgroup analyses, I find statistically significant results indicating that private schooling increases test effort while decreasing survey effort. Compared to the model using all observations, the effects for students that have high math and reading abilities (above the 50^{th} percentile) are larger in size, and all are statistically significant and in the same direction. The coefficients for the more advantaged subgroups are usually larger in size than for disadvantaged subgroups. All statistically significant results are robust to models that cluster at the student and school level. All coefficients are identical in models that cluster standard errors at the country level; however, statistical significance does not remain, perhaps because 31 clusters do not allow for sufficient power. Nonetheless, since the instrument replicates randomization of the private schooling treatment, the preferred models cluster standard errors at the school level (Barrios et al., 2012; Cameron & Miller, 2015; Green & Vavreck, 2007).

Table 4A: Private School Impacts on Non-Cognitive Skills (1st Stage)

	(1) Private (Test Decline)	(2) Private (Careless Answer)	(3) Private (Non-Response)
Catholic Share 1900	**0.152*** (0.000)**	**0.152*** (0.000)**	**0.152*** (0.000)**
Controls	Yes	Yes	Yes
R^2	0.101	0.101	0.101
Countries	31	31	31
Clusters	976	976	976
Students	190,916	190,873	190,916

Notes: P-values in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001. Robust standard errors clustered at the school level. All models use 58 country, student, and school level controls.

Table 4B: Private School Impacts on Non-Cognitive Skills (2nd Stage)

	(1) Test Decline	(2) Careless Answer	(3) Non-Response
Private	**-1.790*** (0.000)**	**0.408*** (0.000)**	**8.362*** (0.000)**
Controls	Yes	Yes	Yes
Countries	31	31	31
Clusters	976	976	976
Students	190,916	190,873	190,916

Notes: P-values in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001. Robust standard errors clustered at the school level. All models use 58 country, student, and school level controls. 1st stage uses a linear probability model, and results are robust when the 1st stage uses probit or logit models, as shown in Table A1 in Appendix A.

Table 5: Overall Results by Analytic Technique

	(1) Test Decline	(2) Careless Answer	(3) Non-Response
OLS	**-0.123***** (0.000)	**0.001** (0.779)	**0.328***** (0.000)
OLS Country Fixed Effects	**-0.070***** (0.000)	**0.003** (0.150)	**-0.051** (0.212)
2SLS - Catholic Inst. (1900)	**-1.790***** (0.000)	**0.408***** (0.000)	**8.362***** (0.000)
Student/School/Country Controls	Yes	Yes	Yes
Countries	31	31	31
Clusters	1,097	1,097	1,097
Students	277,997	277,942	277,997

Notes: P-values in parentheses. * p<0.05, ** p<0.01, *** p<0.001. P-values for all instruments are ~0.000 in all first stage regressions. Robust standard errors clustered at the school level.

Table 6: Heterogeneous Effects on Non-Cognitive Skills (2SLS)

	(1) Test Decline	(2) Careless Answer	(3) Non-Response
High ESCS Student	**-1.923****	**0.414**	**8.310**
	(0.000)	(0.131)	(0.448)
Low ESCS Student	**-1.613*****	**0.400*****	**8.431*****
	(0.000)	(0.000)	(0.000)
High Math Student	**-2.183*****	**0.421*****	**8.778*****
	(0.000)	(0.000)	(0.000)
Low Math Student	**-1.167*****	**0.388*****	**7.701*****
	(0.001)	(0.000)	(0.000)
High Read Student	**-2.536*****	**0.433*****	**9.112*****
	(0.000)	(0.000)	(0.000)
Low Read Student	**-0.922*****	**0.380*****	**7.488*****
	(0.010)	(0.000)	(0.000)
Female Student	**-1.959*****	**0.396*****	**8.179*****
	(0.000)	(0.000)	(0.000)
Male Student	**-1.619*****	**0.433*****	**8.703 *****
	(0.000)	(0.000)	(0.000)
Student/School/Country Controls	Yes	Yes	Yes

Notes: P-values in parentheses. * p<0.05, ** p<0.01, *** p<0.001. P-values for all instruments are ~0.000 in all first stage regressions. Robust standard errors clustered at the school level. All results are from 2SLS regressions including 58 control variables. High ESCS, high math, and high reading are binary variables indicating that the student's ESCS or PISA scores were above the 50th percentile. Low ESCS, low math, and low reading are binary variables indicating that the student's ESCS or PISA scores were below the 50th percentile.

Section 7.3: Effects on Test Scores

Results from an analysis of the effects of private schooling on individual student math, reading, and science scores are displayed in Table 7 below. This analysis is similar to West and Woessmann (2010) since the same instrumental variable is used, the Catholic share of the population in 1990; however, the instrument is used to predict the likelihood that each individual student ends up in a private school today, while West and Woessmann (2010) predicted the likelihood that countries have larger private school shares today.

In accord with West and Woessmann (2010), these results also suggest that private schooling has significant positive effects on PISA math, reading, and science scores. This result suggests that the instrumental variables specification is working as expected and that the estimates are robust. In particular, private schooling increases student math scores by 179 scale points, reading scores by 73 scale points, and science scores by scale 218 points. These effects are all substantially large, as they equate to around a 185 percent of a standard deviation increase in math scores, a 79 percent of a standard deviation increase in reading scores, and over a two standard deviation increase in science scores. The dependent variables in this analysis are the first reported plausible values for student test scores, constructed by PISA.[7] These results are robust to all five plausible values reported by PISA.

As shown in Table 8 below, the effects of private schooling on cognitive skills, as captured by test scores, remain even after controlling for all three measures of non-cognitive skills. As expected, higher non-cognitive skills, as measured by test and survey effort, are associated with higher PISA test scores on all three subjects. These results align with Zamarro et al. (2016), who find that stronger survey effort is positively associated with academic outcomes

[7] PISA 2012 Technical Report. OECD. Retrieved from https://www.oecd.org/pisa/pisaproducts/PISA-2012-technical-report-final.pdf

such as educational attainment. It is likely that students with strong non-cognitive skills also have strong cognitive skills, and it also may be that student test scores capture some forms of non-cognitive skills as well. Of course, students that work hard in school will have a stronger ability to accumulate the knowledge and skills necessary to perform well on PISA math, reading, and science exams. In addition, student survey non-response rates exhibit the strongest relationship with test scores. For example, a one standard deviation increase in student survey non-response rates is associated with around a quarter of a standard deviation decrease in PISA math scores, while one standard deviation increase in the other two measures is associated with reductions of only around a twentieth of a standard deviation.

Table 7: Private Schooling's Effect on PISA Scores (2nd Stage)

	(1) Math	(2) Reading	(3) Science
Private	**178.98***** **(0.00)**	**73.41*** **(0.02)**	**218.47***** **(0.00)**
Controls	Yes	Yes	Yes
Countries	31	31	31
Clusters	976	976	976
Students	190,873	190,873	190,873

Notes: P-values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors clustered at the school level. P-values for all instruments are ~0.000 in all first stage regressions. All models use 55 country, student, and school level controls.

Table 8: Private Schooling's Effect on PISA Scores Controlling for Non-Cog (2nd Stage)

	(1) Math	(2) Reading	(3) Science
Private	**216.76*** **	**103.79** **	**255.18*** **
	(0.00)	(0.00)	(0.00)
Test Decline	**-2.24*** **	**-3.54*** **	**-2.94*** **
	(0.000)	(0.00)	(0.00)
Careless Answering	**-18.48*** **	**-11.50*** **	**-16.72*** **
	(0.00)	(0.00)	(0.00)
Non-Response Rate	**-5.63*** **	**-5.28*** **	**-5.82*** **
	(0.00)	(0.00)	(0.00)
Controls	Yes	Yes	Yes
Countries	31	31	31
Clusters	976	976	976
Students	190,873	190,873	190,873

Notes: P-values in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors clustered at the school level. P-values for all instruments are ~0.000 in all first stage regressions. All models use 58 country, student, and school level controls. Dependent variables are the first plausible value calculated by PISA for individual student scores. Results are robust to all five plausible values reported by PISA.

Section 8: Conclusion and Policy Implications

This is the first study to estimate the effects of private schooling on non-cognitive outcomes as measured by patterns of student responses on exams and surveys. Three innovative measures are employed to capture student effort and diligence: test decline on PISA exams, careless answering patterns on PISA questionnaires, and non-response rates on PISA questionnaires. While I suggest that the first measure captures effort on tests and that the other two measures capture effort on surveys, other explanations ought to be explored. For example, the last two measures could be capturing *conscientiousness* or *respect* as well.

In addition, I must acknowledge that the attributes captured in these metrics are likely a mix of cognitive and non-cognitive skills. In other words, cognitive and non-cognitive skills are

not mutually exclusive. For example, it may be that the test-decline measure captures more cognitive skills than I initially expected. If a given student has very little difficulty getting through a test that they do not find academically rigorous, they may not have to exert as much non-cognitive skills. On the other hand, a student that finds the exam extremely difficult will have to put forth more effort to get through the exam.

The test score findings align with the results from West and Woessmann (2010) and Heller-Sahlgren (2018), indicating that private schools improve PISA math, reading, and science test scores. Importantly, I find that the private school test score advantage remains large even after including all three measures of student-level effort as control variables. Interestingly, I also find that all three measures of low student effort are negatively associated with PISA scores, indicating that students with strong non-cognitive skills also perform well on standardized cognitive assessments.

This study also finds causal evidence to suggest that private schooling improves student effort on tests, while public schooling improves student effort on surveys. The policy implications all depend on the power of these measures, and, perhaps most importantly, the goals of any given society. If decision-makers aim to improve non-cognitive skills such as performance (West & Woessmann, 2010) and individual effort, they should expand access to private school choice programs. On the other hand, if policy-makers instead desire to improve the levels of social order, they may want to limit access to private school choice programs. However, researchers do not yet know exactly what skills each of these measures are actually capturing, so policy-makers should not make policy decisions based solely on the results from this study.

Nonetheless, research linking non-cognitive skills to specific school programs is extremely limited. Since this is the first study to connect types of schooling institutions to non-cognitive outcomes, additional research on this topic is especially welcome. Once more years of the outcome variables become available to researchers, they will be able to use time and country fixed effects regression to add to the literature. Further research is necessary to determine exactly what non-cognitive skills the three measures in question actually capture. Additionally, the literature would benefit substantially from experimental evaluations of private school choice programs that examine these types of outcomes. However, researchers and policy-makers must be especially cautious about the role that Campbell's Law has in this specific context (Campbell, 1979). If school leaders, teachers, and students know that these measures exist, and they are used to make policy decisions, the measures could become useless due to near-costless corruption.

References: Chapter 2

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Chapter 2: Appendix A

Table A1: 2SLS Results by 1st Stage Analytic Technique

	(1) Test Decline	(2) Careless Answer	(3) Non-Response
Probit 1st Stage	**-1.997***** (0.000)	**0.098***** (0.000)	**5.600***** (0.000)
Logit 1st Stage	**-1.814***** (0.000)	**0.088***** (0.000)	**5.100***** (0.000)
Linear Probability Model 1st Stage	**-1.790***** (0.000)	**0.408***** (0.000)	**8.632***** (0.000)
Student/School/Country Controls	Yes	Yes	Yes
Countries	31	31	31
Clusters	1,097	1,097	1,097
Students	277,997	277,942	277,997

Notes: P-values in parentheses. * p<0.05, ** p<0.01, *** p<0.001. P-values for all instruments are ~0.000 in all first stage regressions. Robust standard errors clustered at the school level. 85% correctly classified in probit and logit models. 82% correctly classified in linear probability model.

Chapter 2: Appendix B

Table A2: Complete List of Control Variables Used

Type of Characteristic	Variables
Country	Catholic share of country's population in 2010; percent of hardworking individuals in country; GDP per capita in country; percent of 25-34 year olds with tertiary education in country; teacher starting salary in country; pupil to teacher ratio in country
School	Principal non-response rate
Household	Number of books in household (5 variables); index of Economic, Social, and Cultural Status (ESCS); size of city of residence
Parent	Father's education (2 variables); mother's education (2 variables); father's occupation (8 variables), mother's education (8 variables); immigrant status of father; immigrant status of mother
Student	Age; gender; immigrant status; test booklet ID (13 variables); plausible PISA math score; plausible PISA reading score; plausible PISA science score

Table A3: Catholic Share of the Population and Private Schooling by Country (Percent)

Country	Catholic Share (1900)	Private Schooling (2009)
Australia	22	40
Austria	92	12
Belgium	97	69
Canada	40	7
Chinese Taipei	0	3
Croatia	82	4
Czech Republic	86	5
Denmark	0	18
Estonia	1	39
Finland	0	3
France	98	5
Germany	36	0
Greece	1	4
Hong Kong	1	7
Hungary	61	93
Iceland	0	1
Indonesia	0	12
Ireland	89	46
Israel	2	64
Italy	100	1
Japan	0	17
Latvia	33	5
Liechtenstein	97	28
Lithuania	90	38
Luxembourg	97	6
Macau	8	1
Netherlands	35	15
New Zealand	14	1
Norway	0	96
Poland	77	60
Portugal	100	1
Russia	0	5
Shanghai - China	0	7
Singapore	2	11
Slovakia	85	10
Slovenia	95	0
South Korea	1	2
Spain	100	8
Sweden	0	2
Switzerland	40	12

Table A3 (Continued)

Country	Catholic Share (1900)	Private Schooling (2009)
Thailand	1	38
Turkey	1	13
United Kingdom	6	1
United States	14	7

Chapter Three

The School Choice Voucher: A "Get Out of Jail" Card?

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Abstract

We examine crime rates for young adults who experienced Milwaukee's citywide voucher program as high school students compared to matched public school peers, using unique data collected as part of a longitudinal evaluation of the program. We find that mere exposure to private schooling through a voucher is associated with lower rates of criminal activity, but the relationship is not robust to different analytic samples. Students who used the program through 12th grade, however, were much less likely to have criminal records than their public school peers. These results are apparent when controlling for a robust set of student demographics, test scores, and parental characteristics. We conclude that merely being exposed to private schooling for a short time through a voucher program may not have a significant impact on criminal activity, though persistently attending a private school through a voucher program can decrease subsequent criminal activity.

Keywords: bottom-up reform; school violence; character education; civic education; democratic education; private schooling; school choice

Section 1: Introduction

School choice programs include a variety of mechanisms by which parents can actively choose their child's school as opposed to accepting a default residential assignment, including securing a residence in a specific neighborhood to gain access to a particular public school (i.e. Tiebout Choice), public charter schools, intra-district and inter-district public school choice, and private school choice. School choice can be conveniently divided into public school choice and private school choice in the form of self-financed private schooling, government vouchers, tax-credit scholarships or Education Savings Accounts.

Private school choice was one of the most contentious education policies in the 2016 U.S. presidential election. Donald Trump promised federal initiatives to increase the availability of private school choice. Hillary Clinton opposed private school choice, claiming that it would harm society. If President Trump expands private school choice in the U.S., what effects might that have, especially on the broader society?

The evaluation literature on private school choice globally overwhelmingly focuses on the single outcome of student achievement (Angrist, Bettinger, & Kremer, 2006; Cowen, 2008; Greene, Peterson, & Du, 1999; Howell, Wolf, Campbell, & Peterson, 2002; Metcalf, West, Legan, Paul, & Boone, 2003; Mills & Wolf, 2017; Muralidharan & Sundararaman, 2015; Rouse, 1998; Witte, 2000; Witte, Wolf, Cowen, Carlson, & Fleming, 2014; Wolf et al., 2013; Wolf, Egalite, & Dixon, 2015). A recent meta-analysis and systematic review of the experimental studies indicates that private school choice tends to have small positive impacts on student test scores, though there is substantial variation in those findings across countries, programs, years and subject areas (Shakeel, Anderson, & Wolf, 2016).

Many commentators argue that schools have a responsibility beyond what is measured by standardized test scores (Lawton, Cairns, & Gardner, 2004; Macedo & Wolf, 2004; Zimmer et al., 2009). At best, test scores only can measure some of the cognitive abilities of students (Hitt & Trivitt, 2013). Schools also can be thought of as social institutions that aim to improve the non-cognitive skills of students such as grit, persistence, conscientiousness, and how to form relationships with others (Arthur & Davidson, 2000; Duckworth, Peterson, Matthews, & Kelly, 2007; Egalite, Mills, & Greene, 2016; Hitt, Trivitt, & Cheng, 2016). Improving student cognitive and non-cognitive 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).

Relatively few studies of private school choice evaluations examine the effects of choice on student non-cognitive skills and non-academic outcomes. Most recent evaluations find that private school choice increases student attainment in the form of higher rates of high school graduation and college enrollment (Chingos, 2018; Chingos & Kuehn, 2017; Chingos & Peterson, 2015; Cowen, Fleming, Witte, Wolf, & Kisida, 2013; Wolf et al., 2013; Wolf, Witte, & Kisida, 2018). In his review of the causal evidence, DeAngelis (2017) found eleven studies indicating that program participation had null to positive effects on political participation, volunteering, and charitable giving (Bettinger & Slonim, 2006; Campbell, 2008; DeAngelis & Wolf, 2017; Fleming, 2014; Fleming, Mitchell, & McNally, 2014; Wolf, Peterson, & West, 2001; Wolf, Peterson, & West, 2001). Similarly, in his review of 21 studies, Wolf (2007) found positive effects of private school choice on the civic outcomes of students. Swanson's (2017) review of eight U.S. studies finds that school-level racial integration is either unaffected or

improved by private school choice (e.g. Greene, Mills, & Buck, 2010; Greene & Winters, 2007; Egalite, Mills, & Wolf, 2017).

Throughout U.S. history, one of the main arguments for allocating additional resources to schooling is that it can reduce crime (West, 1965). Schools can teach people to be better citizens, increase social cohesion and increase democratic participation (Mann, 1855; Tooley, 2000). Educational attainment improves the economic prospects of young adults, providing them with an incentive to stay out of trouble (Rouse, 2005). The U.S. is considered to be "the most violent advanced industrial society on earth" (Currie, 2013). Crime is most problematic in urban areas, where students have less access to quality schools. Crimes have large negative impacts on society as a whole. McCollister, French, and Fang (2010) find that each instance of vandalism and robbery cost society $5,457 and $47,500 (in 2016 U.S. dollars), respectively. Access to higher quality schools, or more school choices in general, could affect crime.

We might expect private school choice to have a desirable effect on reducing crime. When families are able to choose their children's educational institutions, competitive pressures may provide additional incentives for schools to improve non-cognitive skills (Chubb & Moe, 1988; Chubb & Moe, 1990; Hoxby, 2003; Friedman, 1997). Schools of choice involve voluntary associations of people attracted by a common set of values who, as a result, can build a strong sense of community and social capital (Coleman & Hoffer, 1987; Hill, Foster, & Gendler, 1990). Religious schools, in particular, foster a strong sense of community because of their explicit moral commitments and prioritization of developing student character (Bellah, Madsen, Sullivan, Swidler, & Tipton, 1985; Brandl, 1998; Bryk, Lee, & Holland, 1993; Jeynes, 2012; Johnson, 2011). Since private schools are typically located in more-advantaged areas, access to the program could decrease criminal activity simply by relocating students away from negative

environments. Criminal activity could decrease because students in more-advantaged schools may discourage peers from engaging in rebellious behavior. For these reasons, private school choice might have its largest impact on the life-choices of students who experience it, such as decisions to commit or not commit crimes.

Most studies that look at schooling impacts on criminal activity ignore school choice, instead focusing on the crime effects of drop-out rates and broad schooling laws (Anderson, 2014; Lochner, 2010; Luallen, 2006). Other studies have looked at schooling desegregation and its impacts on crime (Billing, Deming, & Rockoff, 2013; Weiner, Lutz, & Ludwig, 2009), or how educational attainment can affect later criminal activity (Groot & Brink, 2010; Lochner, 2011; Machin, Marie, & Vujić, 2011; Oreopoulos & Salvanes, 2011). These evaluations indicate that higher levels of education cause less criminal activity, but they do not examine differences in outcomes based on the type of schooling. School choice studies tend to ignore crime and crime studies tend to ignore school choice.

Only four studies examine the intersection of school choice and crime. Deming (2011) compares the criminal activity of students who won and lost the charter school lottery in the Charlotte-Mecklenburg County of North Carolina in 2002. He finds that winning the lottery significantly decreased 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 go to a 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 how residential (a.k.a. Tiebout) school choice is related to criminal activity. They find that a one standard deviation increase in 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. They find that crime tends to increase when Catholic schools in urban areas shut their doors. The increased availability of non-religious schools of choice, specifically public charter schools, has no significant effect on crime in the inner-city, they argue. 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.

We conduct the first student-level analysis of the effect of a private school choice program on the criminal behavior of young adults, using data from the Milwaukee Parental Choice Program (MPCP). The MPCP is the nation's first urban school voucher system, currently enrolling over 27,000 students in over 110 different private schools. Our results suggest that sustained participation in the MPCP has a significant downward effect on the likelihood of a student engaging in criminal activity as a young adult. We proceed by describing the voucher program on which our evaluation is based and the data and analytical procedures we employ. Next, we present tables and statistical models of the conditions that predict different types of criminal activity, 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.

Section 2: Background

The MPCP was 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, 2000). Starting in 1996, the enrollment cap was raised substantially and repeatedly, until it was eliminated in 2012, and religious schools were permitted to enroll voucher students starting in 1998. These policy

decisions, which allowed both demand and supply to grow, resulted in the program enrolling about 25 percent of all K-12 students in the city of Milwaukee in 2014-15.

The MPCP is a government-run school voucher program. 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; and 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 per year, about 40 percent less than the average per pupil expenditure in MPS (Costrell, 2008). To qualify for a voucher, applicants had to live in the city of Milwaukee, be entering grades K-12, and have a family income at or below 175 percent of the poverty level, an amount slightly below the ceiling to qualify for the federal lunch program.

Section 3: Data and Matching Procedure

In most cases, vouchers were not randomly assigned to students in Milwaukee via lottery (Cowen, Fleming, Witte, Wolf, & Kisida, 2013). Although schools in the program are required to admit students by lottery when a given grade in a school is oversubscribed, school personnel tend to recruit voucher-eligible students only until that ceiling is reached. As a result, admission to most of the grades in most of the voucher schools does not require a lottery.

To generate comparable groups for the analysis we used an algorithm that matched MPCP (i.e. voucher) students with MPS students based on grade, neighborhood, race, gender, English Language Learner (ELL) status and math and reading test scores (Witte, Wolf, Cowen, Fleming, & Lucas-McLean, 2008). First, the entire census of 801 MPCP students who were in 9th

grade in the fall of 2006, along with a randomly-selected representative sample of 290 MPCP students in 8th grade that year, were organized into a total program sample of 1091. Researchers matched these voucher students to the set of MPS students in their same grade within the same neighborhood census tract. Census tracts largely define neighborhoods in Milwaukee. Families who live in the same neighborhood tend to share similar unmeasured background factors such as motivation and moral values that, if not balanced across our samples, might bias our analysis of school choice effects (Ahlbrandt, 2013). Matches were further restricted to MPS students that were in the same 5 percent bandwidth of 2006 test scores. Finally, the specific MPS student that would serve as the match for each MPCP student was selected based on the nearest-neighbor propensity score calculated by student race, gender, ELL status, and test score. All but two students in the program sample were successfully matched. The result is a treatment group of 1089 students exposed to a voucher in 2006 and a matched group of 1089 highly similar comparison students in MPS in 2006, for a total analytic sample of 2178. Previous research shows that this type of non-experimental matching design which factors "geography" (i.e. neighborhood) into the match can come close to replicating "gold standard" experimental results (Bifulco, 2012).

Table 1 provides information about the two matched groups of students in our analysis. They do not differ regarding the key characteristics of race and baseline math scores, but there are statistically significant differences in gender at the $p < 0.05$ level and reading scores at the $p < 0.01$ level. Students that were enrolled in MPCP at the baseline year of 2006 are more likely to be female and more likely to have higher reading scores. These differences are controlled for in our model estimations below.

Table 1: Statistics on Model Covariates

	MPCP in 2006	MPS in 2006
Female	0.58**	0.53
Black	0.72	0.71
Hispanic	0.17	0.17
Other Race	0.10	0.12
Math in 2006	-0.03	0.03
Reading in 2006	0.14***	0.00
N	1089	1089

Notes: ** p<0.05, *** p<0.01

After students were matched, their parents were surveyed by telephone to gather important family background information such as family income, mother's and father's education, and whether both parents lived in the home. The telephone survey was administered by researchers at Westat's survey call center in two waves – an initial wave in November-December of 2006 and a follow-up wave limited to initial non-respondents in November-December of 2007. The survey instrument, described in detail in Witte, Wolf, Cowen, Fleming, and Lucas-McLean (2008), drew upon questions asked of participants in previous school voucher evaluations in Milwaukee; New York City; Dayton, Ohio; and Washington, DC; with some refinements by the research team. A total of 69 percent of parents in both the MPCP and MPS samples eventually responded – a very high response rate for a telephone survey. The response rate for MPCP parents was 73 percent while the rate for MPS parents was 66 percent. In the analysis below, we use response weights to correct for any baseline differences between the two groups of respondents. For our more complete model estimations we use this subsample of 1506 students whose parents were survey respondents so that we can control for family background characteristics that might otherwise bias our estimation of the voucher program effect on crime.

For our dependent variables we used the Wisconsin Court System Circuit Court Access (2017) to search for records using student first name, last name and date of birth. The researchers

conducting the search were unaware of each person's status as a member of the MPCP or MPS sample. We used seven different categories for dependent variables. First, we classified criminal activity based on the type of crime committed. Our categories for convicted criminals were: felony, misdemeanor, traffic-related, theft-related and drug-related. These categories are not all mutually exclusive. For example, all students that were convicted of a crime were also accused of a crime at some point, although not all accused were convicted. Drug-related and theft-related crimes could also be felonies or misdemeanors, depending on the specific crime. Misdemeanors are mutually exclusive of felonies, while traffic-related crimes are mutually exclusive of both. We also examined two other categories: whether the student was convicted of any type of crime and whether the student was accused of any type of crime. Criminal records were not present in the data unless the student was an adult at the time of the crime. Students graduate around the age of 18, so the effects of voucher exposure on adult criminality were captured. Since we searched the database during the summer and fall of 2015, the students in our sample were 22-25 years old when we checked for criminal records on them.

Table 2 summarizes our full sample of 2,178 unique students and their characteristics. Around 4 percent of the sample was found guilty of a felony, 9 percent of a misdemeanor, 19 percent of a traffic-related crime, 5 percent of theft and 6 percent of a drug-related crime. With little variation in our dependent variables, it may be difficult with our current sample size to detect any differences (if they exist) across our comparison groups for most types of crime.

Table 2: Descriptive Statistics of Variables

Variable	N	Mean	Std. Dev.	Min	Max
Grade in 2006	2178	8.74	.44	8	9
Black	2178	.70	.46	0	1
Hispanic	2178	.18	.39	0	1
Other Race	2178	.11	.31	0	1
Female	2178	.55	.50	0	1
MPCP 2006	2178	.50	.50	0	1
Full Dose	2178	.20	.40	0	1
Income>50	1401	.11	.31	0	1
35<Income<50	1401	.14	.35	0	1
25<Income<35	1401	.18	.39	0	1
Parent HS Grad	1506	.29	.45	0	1
Parent Some College	1506	.33	.47	0	1
Parent Completed College	1506	.15	.35	0	1
Math Z Score	2178	.00	.87	-3.13	3
Read Z Score	2178	.07	.90	-2.97	2.54
Both Parents in HH	1502	.34	.47	0	1
Parent Frequent Churchgoer	1500	.58	.49	0	1
Accused	1842	.38	.49	0	1
Convict	1842	.35	.48	0	1
Felony	2178	.04	.20	0	1
Misdemeanor	2178	.09	.29	0	1
Traffic	2178	.19	.39	0	1
Theft	2178	.05	.21	0	1
Drugs	2178	.06	.24	0	1

Section 4: Methods and Results

We present estimates of the effects of voucher exposure using controls for student characteristics alone and student and parent characteristics together. We present estimates of the effects on students that persisted through the voucher program and also perform subgroup analyses for males.

Section 4.1: Criminal Intent-to-Treat (ITT) Effects Controlling for Student Characteristics

Our basic model conditions the probability that a given student, i, reached a certain criminal activity outcome:

$$Prob\ (Criminal\ Activity)\ =\ \beta_0 + \delta_1 MPCP06_i + \beta_1 X_i + \beta_2 test_{2006} + \varepsilon_i$$

which we estimate via probit, where for each outcome of interest (felony, misdemeanor, traffic-related, theft-related, drug-related; found guilty of any type of crime; or simply accused of a crime), $\delta_1$is the difference associated with exposure to MPCP (enrolled in the MPCP in 2006) after accounting for the 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. Each crime outcome observation is coded "1" or "0" for each category. Young adults who had committed multiple crimes in a given category were rare but, when they occurred, they were simply coded "1" for the category. We did this because using an actual count of crimes instead of a 0/1 classification would have required us to use a more complex statistical operation (ordered probit) that would have been highly inefficient given the distribution of our data. Since we control for student 2006 test scores, any effect that the MPCP has on reducing criminal behavior by boosting student test scores would be captured by that control variable for students in the program prior to 2006, making our independent estimate of the effect of the MPCP overly conservative. Previous research using some of these same data suggests that any test score effects of the voucher program were modest, only in reading, and only clear in the year in which the test was "high stakes" for the voucher students and private schools (Witte et al., 2014). We use robust standard errors in all probit models due to the heteroskedastic nature of models with binary dependent variables. We cluster robust standard errors by census tract since students within the same geographic region tend to be similar on unobservable characteristics.

We start with an Intent-to-Treat (ITT) analysis, as all of the students in the MPCP group are coded "1" for MPCP06 regardless of how long they persisted in the program. This section of the analysis estimates the effect of "exposure" to the MPCP (for whatever duration of time) on

subsequent criminal behavior. We use this ITT approach at the outset of our analysis because non-random sorting of students across sectors took place after the 2006 baseline year (Cowen, Fleming, Witte, & Wolf, 2012) that otherwise might bias our estimates of the program's effect.

Some types of crimes had a sample size of 1842 because several student names were matched to crimes but without the confirmatory match of their birth dates. Since such matches were not conclusive, we omitted those cases from our analysis.

For our initial ITT analysis, exposure to the MPCP has tiny and inconsistent effects on our seven crime measures (Table 3). For five of the outcome variables (misdemeanor, accused, convicted, drugs, and traffic) participation in the MPCP has a negative effect on crime (meaning a crime reduction) and for theft it has a positive effect. Importantly, none of the coefficients estimating the effect of the MPCP on crime outcomes achieves statistical significance at the $p < 0.05$ level. It appears that merely being exposed to private schooling through a voucher may not produce a statistically significant change in one's early propensity to commit crimes.

Table 3: Probit ITT Estimates with Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
mpcp06	-0.02	-0.02	-0.01	0.00	-0.01	-0.02	0.01
	(0.262)	(0.386)	(0.362)	(0.943)	(0.241)	(0.276)	(0.654)
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1842	1842	2178	2178	1842	1842	1842

P-values in parentheses. Results are average marginal effects. Robust standard errors clustered by census tract. * p<0.10, ** p<0.05, *** p<0.01

Results from the control variables suggest that our finding of no significant correlation between the MPCP and criminal behavior is not solely due to the noisy nature of the data. Female students were less likely to be associated with any of these criminal activities at levels that were

statistically significant with high confidence. Black students were significantly more likely to be accused or convicted of crimes in general and, in some cases, students with higher test scores were less likely to be associated with crimes. The exception to that rule, the positive association between math scores and traffic violations, likely is because high school students who are doing better in math are more likely to have the resources to own and drive a car than are students who are doing poorly in math.

Section 4.2: ITT Effects Controlling for Student and Parent Characteristics

The second model we estimate, via probit, is:

$$Prob\ (Criminal\ Activity) = \beta_0 + \delta_1 MPCP06_i + \beta_1 X_i + \beta_2 test_{2006} + \beta_3 Z_i + \varepsilon_i$$

where for each outcome of interest, $\delta_1$is the difference associated with exposure to MPCP (enrolled in the MPCP in 2006) after accounting for the vector X of student race, gender, and baseline grade (8th or 9th) indicators; vector Z of parent income levels, education levels, churchgoing activity, and whether both parents lived at home; 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.

The sample size drops to 1385 in the parental characteristics models since not all parents responded to the surveys. This can lead to bias since certain types of parents may be more or less likely to complete surveys and those tendencies could be correlated with participation in the MPCP, but we apply non-response weights to the data to mitigate that problem.

When we control for parental characteristics, as displayed in Table 4, we can see that MPCP exposure is associated with a reduction in every type of crime except theft, which has a coefficient of zero. The effect of the MPCP on reducing criminal behavior is statistically insignificant at the $p < 0.10$ level except for being convicted of a misdemeanor and the general

category of simply being accused of a crime. Being accused of a crime was one of the few crime categories, along with traffic violations, containing more than trivial variation in the dependent variable and therefore provided us greater statistical power to identify a significant relationship between the voucher program and crime. Mere exposure to a voucher program at baseline results in students being 5 percentage points less likely of being accused of a crime as young adults, all else equal. Mere exposure to the MPCP at baseline results in students being 2 percentage points less likely of being found guilty of a misdemeanor.

Table 4: Probit ITT Estimates with Parent and Student Controls

	(1) Accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
mpcp06	-0.05*	-0.04	-0.02*	-0.00	-0.02	-0.03	0.00
	(0.088)	(0.156)	(0.095)	(0.815)	(0.274)	(0.190)	(0.727)
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1177	1177	1385	1385	1177	1177	1177

P-values in parentheses. Results are average marginal effects. Robust standard errors clustered by census tract. * p<0.10, ** p<0.05, *** p<0.01

Most of the control variables for parental characteristics behave as expected in the estimations. Having two parents in the home is strongly and consistently associated with a reduced likelihood of all types of criminal activity. The children of parents with more exposure to college are less likely to commit various crimes. The children of families with higher incomes actually are more likely to commit misdemeanors or drug crimes, all else equal, but that could be because, within a low-income population, more resources bring with them more temptations.

Merely being enrolled in the MPCP in 2006 is only significantly associated with a reduction in crime in two of the 14 “Intent-to-Treat” model estimations in our analysis. It may be

that the kind of character transformation required to truly change the criminal trajectories of young, low-income, urban students necessitates that they receive more sustained exposure to a positive private school environment. Thus, even though mere exposure to the MPCP might not produce a clear reduction in subsequent criminal behavior, sustained exposure to private schooling through the voucher program could have such effects. Therefore, we proceed to measure the effect of remaining in the program for 4 or 5 years, for baseline 9th graders and 8th graders respectively, on criminal activity.

For this "Local Average Treatment Effect" (LATE) analysis we cannot simply compare the criminal records of persistent MPCP participants with all other students in the sample (non-persistent MPCP students and all MPS) or even to all matched MPS students. The students who persist in the MPCP all the way to high school graduation are a selective group, more likely to be female, white, Hispanic, and to have higher test scores than the students who did not persist in the program (Cowen, Fleming, Witte, & Wolf, 2012). Although we could control for differences in these measurable factors in our models, the fact that MPCP persisters differ from their peers so clearly on measurable factors suggests that they also differ from them on unmeasurable factors such as grit and conscientiousness that are related to the propensity to commit crimes. A simple comparison of the criminal activity of sustained participants in the MPCP with matched MPS students would produce estimates of MPCP effects that likely would be biased in the direction of over estimating the effect of the MPCP on reducing crime. Because of this concern, we use Instrumental Variables (IV) in the context of probit to remove selection bias from the *fulldose* variable of interest.

Section 4.3: LATE Using IV Probit Controlling for Student Characteristics

We are interested in understanding the effect of getting the full intended dose of the voucher program treatment on subsequent student criminal activity. We define full dose as a 2006 voucher student staying within the program through 12th grade. We use the exposure to the voucher in 2006 as an instrumental variable since it predicts whether the child is going to get the full dose of the program. Enrollment in the MPCP in the baseline year is a strong, relevant instrument, since the correlation between the instrument and the supposedly endogenous variable in the first stage of the IV Probit estimation is 0.43. The instrument is exogenous based on the assumption that the original matching procedure is successful in approximating random assignment. Central to this assumption is the fact that we matched students on neighborhood as well as key student background characteristics such as test scores, an approach that appears to proxy for parent motivation and moral values. Bifulco (2012) finds in his within-study replications that matching procedures like ours are the best way to replicate experimental results.

Our third model conditions the probability that a given student, i, reached a certain criminal activity outcome is:

$$Prob\ (Criminal\ Activity) = \beta_0 + \delta_1 \sim MPCPfulldose_i + \beta_1 X_i + \beta_2 test_{2006} + \varepsilon_i$$
$$Prob\ (MPCP\text{fulldose}) = \alpha_0 + \pi_1 MPCP06_i + \alpha_1 X_i + \alpha_2 test_{2006} + \varepsilon_i$$

which we estimate via probit, where for each outcome of interest, $\delta_1$is the difference associated with persistence in the MPCP (enrolled in the MPCP through 12th grade) after accounting for the 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. We use MPCP06 (exposure to the voucher in 2006) as our instrument for being enrolled in the program through 12th grade, with *~MPCPfulldose* as the predicted value of *MPCPfulldose* from the first stage. Because *MPCPfulldose* represents the group-wide

prediction of persisting in the program, and not the actual sorting behavior of students, it is much less likely to be biased in estimating the effect of the MPCP on crime.

The results displayed in Table 5 show that six of the categories of crimes (misdemeanor, felony, accused, convict, drugs, and theft) have negative coefficients while only one (traffic) has a positive coefficient. Two of these are marginally significant. The full dose of the voucher program leads to a 6 percentage point reduction in the likelihood of being found guilty of a misdemeanor and a 4 percentage reduction in the likelihood of being found guilty of a felony. To gain greater precision in our estimates, we proceed to our final IV Probit estimation which adds parent controls to the model.

Table 5: IV Probit LATE Estimates with Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
~fulldose	-0.04	-0.03	-0.06*	-0.04*	-0.00	0.03	-0.04
	(0.479)	(0.599)	(0.057)	(0.100)	(0.975)	(0.521)	(0.264)
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1842	1842	2178	2178	1842	1842	1842

P-values in parentheses. Results are average marginal effects. Robust standard errors clustered by census tract. * p<0.10, ** p<0.05, *** p<0.01

Section 4.4: LATE Using IV Probit Controlling for Student and Parent Characteristics

The fourth model is:

$$Prob\ (Criminal\ Activity) = \beta_0 + \delta_1 \sim MPCPfulldose_i + \beta_1 X_i + \beta_2 test_{2006} + \beta_3 Z_i + \varepsilon_i$$
$$Prob\ (MPCP\text{fulldose}) = \alpha_0 + \pi_1 MPCP06_i + \alpha_1 X_i + \alpha_2 test_{2006} + \alpha_3 Z_i + \varepsilon_i$$

which we estimate via probit, where for each outcome of interest, δ_1 is the difference associated with persistence in the MPCP (enrolled in the MPCP through 12th grade) after accounting for the vector X of student race, gender, and baseline grade (8th or 9th) indicators; vector Z of parent

income levels, education levels, churchgoing activity, and whether both parents lived at home; 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. Again, we instrument for actual MPCP persistence by replacing that variable with the prediction of persistence obtained by using 2006 MPCP enrollment as an instrumental variable in the first stage of an IV Probit estimation.

The results, displayed in Table 6, show the estimated Local Average Treatment Effect after adjusting for non-compliance by instrumenting for whether or not a student received a full dose of the MPCP treatment. The signs of all the coefficients on the full dose variable are negative, except traffic. Full exposure to the voucher program again is statistically insignificant in its association with every type of crime except for misdemeanors. Full exposure to the voucher program in high school resulted in students being about 7 percentage points less likely to be found guilty of a misdemeanor, all else equal. Again, most control variables behave as expected, with being female and living in a household with two parents demonstrating consistently strong effects on reducing the likelihood of criminal activity.

Table 6: IV Probit LATE Estimates with Parent and Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
~fulldose	-0.05	-0.03	-0.07**	-0.03	-0.01	0.03	-0.03
	(0.488)	(0.677)	(0.044)	(0.207)	(0.777)	(0.558)	(0.310)
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1177	1177	1385	1385	1177	1177	1177

P-values in parentheses. Results are average marginal effects. Robust standard errors clustered by census tract. * p<0.10, ** p<0.05, *** p<0.01

Section 4.5: Average Treatment Effect Using Propensity Score Matching

The IV Probit approach, though assumed to be necessary in this case, is analytically inefficient. To gain more efficiency in our estimation of the Average Treatment Effect (ATE) of full exposure to the MPCP program on crime we use propensity score matching to pair up full dose MPCP students with the MPS students most likely to have been persistent MPCP participants had they been enrolled in the MPCP in 2006, based on student and family background factors. Table 7 indicates that students getting the *full dose* of the program commit less crimes on average compared to MPS students with a "full dose" propensity, for certain types of crime. At the 99 percent level of confidence, students that got the full dose were 5 percentage points less likely to be found guilty of a misdemeanor and 3 percentage points less likely to be found guilty of a felony. At the 90 percent confidence level, they were 2 percentage points less likely to be convicted of theft.

Table 7: Propensity Score Matching ATE Estimates with Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
~fulldose	-0.04	-0.03	-0.05***	-0.03***	-0.03	0.02	-0.02*
	(0.272)	(0.444)	(0.005)	(0.000)	(0.114)	(0.635)	(0.098)
N	1842	1842	2178	2178	1842	1842	1842

P-values in parentheses. Results are average marginal effects.
* p<0.10, ** p<0.05, *** p<0.01

Section 4.6: Male ITT Using Probit Controlling for Student and Parent Characteristics

The most recent statistics indicate that the vast majority, over 93 percent, of all prison inmates are males (Federal Bureau of Prisons, 2017). Since males are much more likely to commit crimes than their female counterparts, we continue with a male subgroup analysis using the three different approaches which all control for student and parent characteristics. First, we start with ITT estimates for males that were exposed to the voucher program at baseline. These results,

found in Table 8, are negative but are not statistically significant. It appears that mere exposure to the program at baseline does not have a statistically significant effect on criminality for males.

Table 8: Male Probit ITT Estimates with Parent and Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
mpcp06	-0.06	-0.04	-0.03	0.00	-0.02	-0.04	0.01
	(0.154)	(0.338)	(0.109)	(0.994)	(0.212)	(0.222)	(0.751)
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1177	1177	1385	1385	1177	1177	1177

P-values in parentheses. Results are average marginal effects. Robust standard errors clustered by census tract. * p<0.05, ** p<0.01, *** p<0.001

Section 4.7: Male LATE - IV Probit Controlling for Student and Parent Characteristics

We continue with a male subgroup analysis using the IV Probit approach controlling for student and parent characteristics. Here, we examine the effect of the full dose of the program on male students and present results in Table 9.

Table 9: Male IV Probit LATE Estimates with Parent and Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
~fulldose	-0.05	-0.03	-0.07	-0.03	-0.01	-0.03	-0.03
	(0.634)	(0.788)	(0.137)	(0.260)	(0.806)	(0.742)	(0.394)
Student Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1177	1177	1385	1385	1177	1177	1177

P-values in parentheses. Results are average marginal effects. Robust standard errors clustered by census tract. * p<0.10, ** p<0.05, *** p<0.01

The signs of all the coefficients on the full dose male variable are negative. The magnitudes of the coefficients on the full dose variable are much larger for males, but standard errors are still relatively high since the estimates are derived from the male half of the original sample. Most control variables behave as expected, with being female and living in a household with two parents demonstrating consistently strong effects on reducing the likelihood of criminal activity. Being in a higher grade at baseline is associated with a higher likelihood of male students being found guilty of certain types of crimes, perhaps because they are a year older than the baseline 8th graders also in the sample.

Section 4.8: Male Average Treatment Effect Using Propensity Score Matching

Since the IV Probit approach is analytically inefficient, we also use propensity score matching for the male subgroup analysis. Table 10 indicates that male students getting the full dose of the program commit fewer crimes on average compared to male MPS students with a "full dose" propensity for three different types of crimes: drugs, felonies, and theft. Male students were about 4 percentage points less likely to be found guilty of a drug-related crime, 2 percentage points less likely to be found guilty of a felony, and 4 percentage points less likely to be found guilty of a theft-related crime. In addition, male students who received the full dose of the program were about 7 percentage points less likely to be accused – and 11 percentage points less likely to be convicted – of any type of crime.

Table 10: Male Propensity Score Matching ATE Estimates with Student Controls

	(1) accused	(2) convict	(3) misdem	(4) felony	(5) drugs	(6) traffic	(7) theft
~fulldose	-0.07*	-0.11**	-0.04	-0.02**	-0.04**	-0.04	-0.04***
	(0.051)	(0.016)	(0.185)	(0.036)	(0.019)	(0.316)	(0.002)
N	1177	1177	1385	1385	1177	1177	1177

P-values in parentheses. Results are average marginal effects.
* p<0.10, ** p<0.05, *** p<0.01

Section 5: Overall Results & Discussion

The evidence from our five analytic model estimations on seven crime variables summarized in Table 11 suggests that participation in the MPCP school voucher program may lead to a decrease in a variety of different types of criminal activity later in life. The clearest results emerge from our most efficient statistical models: those that include parental control variables or use propensity score matching in place of IV Probit to correct for assumed selectivity in our full dose measure of program exposure. Our model estimates indicate that experiencing the MPCP throughout high school reduces the likelihood of a student committing a misdemeanor as a young adult by 2 to 7 percentage points, of committing a felony by 3 to 4 percentage points, of being accused of any crime by 5 percentage points, and of being found guilty of a theft-related crime by 2 percentage points.

Table 11: Effect Estimates by Model

Test	Accused	Con-victed	Mis-demeanor	Felony	Drugs	Traffic	Theft
ITT Probit	-0.02	-0.02	-0.01	0.00	-0.01	-0.02	0.01
ITT (Parental Controls) Probit	-0.05*	-0.04	-0.02*	-0.00	-0.02	-0.03	0.00
ITT (Parental Controls) Male	-0.06	-0.04	-0.03	0.00	-0.02	-0.04	0.01
LATE IV Probit	-0.04	-0.03	-0.06*	-0.04*	-0.00	-0.03	0.04
LATE (Parental Controls) IV Probit	-0.05	-0.03	-0.07**	-0.03	-0.01	0.03	-0.03
LATE (Parental Controls) IV Probit Male Subgroup	-0.05	-0.03	-0.07	-0.03	-0.01	-0.03	-0.03
ATE Propensity Score Matching	-0.04	-0.03	-0.05***	-0.03***	-0.03	0.02	-0.02*
ATE Propensity Score Matching Male Subgroup	-0.07*	-0.11**	-0.04*	-0.02**	-0.04**	-0.04	-0.04***

Note: * p<0.10, ** p<0.05, *** p<0. 01

These effects of the Milwaukee school voucher program on reducing crime are remarkably similar to the estimated effect of a 50 percent reduction in criminal activity from participating in public school choice identified by Deming (2011) and 40 percent reduction due to residential school choice specified by Dills and Hernandez-Julian (2007). The statistically significant percentage point reductions in crime associated with a full dose of the MPCP in our analysis, as a

percentage of their respective incidence rates, are 71 to 84 percent for felonies, 22 to 76 percent for misdemeanors, 47 percent for theft, and 13percent for any accusation. The two previous studies of school choice and crime had much larger samples than our study, contributing to their more precise and consistent estimates of choice effects, but for at least some of our estimates of the effect of private school choice on crime reduction, we obtain statistically significant results that confirm those of the prior studies.

Most of the statistically significant relationships are found for male students, even though the analytic sample is about half of the size. The statistically significant reductions relative to incidence rates for males are 22 percent for felonies, 20 percent for misdemeanors, 21 percent for convictions, 29 percent for drug-related crimes, and 41 percent for theft (Table 12).

Table 12: Statistically Significant Relative Crime Reduction Estimates of MPCP

Group	Accused	Convicted	Mis-demeanor	Felony	Drugs	Traffic	Theft
All Students	13%	-	22-76%	71-84%	-	-	47%
Male Subgroup	-	21%	20%	22%	29%	-	41%

This is the first empirical study of the effect of a private school choice program on subsequent young adult criminal activity. Although the rates of criminal activity in our sample are refreshingly low, in part because these subjects from low-income urban families had only been adults for 4-7 years when we scanned the database for any criminal records, we still are able to identify a significant association between attending a private school throughout high school, via the Milwaukee Parental Choice Program, and subsequent lower levels of criminal activity in most of our more efficient statistical models. Importantly, none of our estimates indicated that

exposure to the MPCP resulted in a statistically significant increase in subsequent criminal activity. The effects of the MPCP on crime that we estimate all are neutral-to-negative (with "negative" meaning crime reduction) with the greatest number of effects on crime reduction due to the MPCP evident where we would most expect them: for young men who experienced a "full dose" of private schooling throughout their high school years.

This study has a number of limitations that we mention throughout the article. Because students were not randomly assigned to the MPCP or the public school comparison group, we cannot assume causality regarding the relationship between the voucher program and crime and must, instead, infer causality. We think that causal inference is justified in this case because: (1) there are strong theoretical reasons to expect that private schooling through a voucher program will reduce criminal behavior; (2) we use a variety of reputable statistical methods to reduce the threat of bias in our effect estimates, including "intent-to-treat", Instrumental Variables, and highly sophisticated student matching approaches; (3) our results differ little regarding the direction of the MPCP effect on crime (it is almost always negative, signaling a reduction in criminal activity) regardless of the estimation method used; (4) we observe the largest number of statistically significant reductions in crime due to the MPCP where we would expect to see them – on males based on our most efficient model estimations; and, (5) no previous experimental or quasi-experimental study of the effect of school vouchers and crime exists. Although our study is not flawless, it is the best study yet conducted on the crucial question of whether or not access to private schooling through vouchers leads to reductions in criminal behavior.

Since avoiding contact with the legal system is one of the strongest predictors of a variety of future quality of life indicators, and low-income urban students often are at high-risk of eventually committing crimes, the case for more research on the effect of school choice

programs on crime prevention is compelling. Should President Trump succeed in promoting more private school choice through federal resources and pilot programs, we urge policymakers to include an evaluation of the program's effects on crime in the policy design. Research on exactly how and why parental school choice reduces the proclivity of students to commit crimes would be especially welcome.

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Chapter 3: Appendix (Institutional Review Board Memo)

UNIVERSITY OF ARKANSAS

Office of Research Compliance
Institutional Review Board

September 11, 2017

MEMORANDUM

TO:	Patrick Wolf	Jay Greene
	Brian Kisida	Jeff Dean
	Meghan Condon	Devan Carlson
	David Fleming	Jonathan Mills
	Michael McShane	Anna Jacob
	Albert Cheng	Julie Trivitt
	Evan Rhinesmith	Ayoola Carleton
	Keith Bardsley	Corey DeAngelis
	Matthew Lee	
FROM:	Ro Windwalker IRB Coordinator	
RE:	PROJECT MODIFICATION	
IRB Protocol #:	1709042283 (previously 06-12-318)	
Protocol Title:	*State Mandated Milwaukee Longitudinal School Choice Evaluation*	
Review Type:	☐ EXEMPT ☒ EXPEDITED ☐ FULL IRB	
Approved Project Period:	Start Date: 09/08/2017 Expiration Date: 12/19/2017	

Your request to modify the referenced protocol has been approved by the IRB. **This protocol is closed to enrollment.** If you wish to make any further modifications in the approved protocol, including enrolling more participants, you must seek approval *prior to* implementing those changes. All modifications should be requested in writing (email is acceptable) and must provide sufficient detail to assess the impact of the change.

Please note that this approval does not extend the Approved Project Period. Should you wish to extend your project beyond the current expiration date, you must submit a request for continuation using the UAF IRB form "Continuing Review for IRB Approved Projects." The request should be sent to the IRB Coordinator, 109 MLKG Building.

For protocols requiring FULL IRB review, please submit your request at least one month prior to the current expiration date. (High-risk protocols may require even more time for approval.) For protocols requiring an EXPEDITED or EXEMPT review, submit your request at least two weeks prior to the current expiration date. Failure to obtain approval for a continuation *on or prior to* the currently approved expiration date will result in termination of the protocol and you will be required to submit a new protocol to the IRB before continuing the project. Data collected past the protocol expiration date may need to be eliminated from the dataset should you wish to publish. Only data collected under a currently approved protocol can be certified by the IRB for any purpose.

If you have questions or need any assistance from the IRB, please contact me at 109 MLKG Building, 5-2208, or irb@uark.edu.

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Conclusion

These dissertation chapters indicate that access to private schooling can have modest to large positive effects on student achievement, student effort on standardized tests, and adult crime reduction. This adds to the causal evidence suggesting that private schooling leads to benefits for individuals (Foreman, 2017; Shakeel, Anderson, & Wolf, 2016) and the societies in which they reside (DeAngelis 2017; DeAngelis & Wolf, 2018; Wolf, 2007).

The improved student test scores found in chapter one could lead to economic competitiveness at the international level (Hanushek & Woessmann, 2012). Indeed, if the United States experienced a ten-percentage point increase – an increase that is not common in the U.S. – in the private share of private schooling enrollment, the results from chapter one suggest that the learning gains would translate to more than a 13 percent gain in lifetime earnings, or over $150,000 per person (Hanushek, 2011). Furthermore, the United States ranked 40th in math and 24th in reading on the 2015 PISA exam. If the United States had experienced a 16-point increase in math and a 12-point increase in reading, the nation would have ranked around 28th in math (with a score similar to Luxembourg) and around 12th in reading (with a score similar to New Zealand) (EdChoice, 2017). Of course, since large increases in the private share of schooling may have differential effects on academic outcomes, this extrapolation should be considered with caution. However, the benefits of economies of scale and market entry could lead to even larger positive effects of private schooling (Hess, 2010). However, standardized test scores may not always be the best proxies for long-term outcomes that society deems important (Greene, 2016). Policy-makers also should examine the effects of private schooling on outcomes that theoretically have larger impacts on societies than individuals (Jones, 2015).

The results from chapter two indicate that private schooling improves student effort on standardized tests but decreases student effort on the long survey immediately following the PISA exam. There are two primary explanations for this. The first is that private schools could be better at shaping the kind of student effort (i.e. determination) that is perceived to lead to larger shares of private benefits, while public schools may have an advantage at shaping the type of effort necessary (i.e. respect) to complete tasks that largely benefit others in society (i.e. tasks with large positive externalities). Secondly, the three measures of student effort that we employ may each capture a different mix of cognitive and non-cognitive skills. For example, the ability for a child to try hard throughout the PISA exam may be more influenced by cognitive skills than the ability for a child to do a good job at answering basic demographic questions on the post-exam survey. However, I attempt to eliminate the second explanation by controlling for student math, reading, and science scores. Regardless of the explanation, chapter two is the first study to examine the effects of private schooling on these measures of non-cognitive skills, and, to my knowledge, the first study to find effects that point in opposite directions. This result also illuminates the fact that the underlying skills captured by these measures may actually be quite different.

The third chapter reveals that while mere exposure to a private school choice program may not have long-term effects on the proclivity of a student to commit crimes, persistence within a program may have lasting impacts on their lives. While this is the first study to causally evaluate the effects of a private school choice program on the likelihood of adult crime, two studies (Deming, 2011; Dobbie & Fryer, 2015) find comparably large impacts for public charter schools, and one study (Dills & Hernández-Julián, 2011) similarly finds substantial crime-reducing effects of stronger degrees of residential choice. Nonetheless, while the existing

research on this important topic is exceptionally scarce, this study tends to reinforce previous results in both direction and magnitude. Since avoiding contact with the legal system is one of the strongest predictors of a variety of future quality of life indicators, and low-income urban students often are at high-risk of eventually committing crimes, the case for more research on the effect of school choice programs on crime prevention is compelling.

Decision-makers should consider the positive effects that access to private schooling has on students and the rest of society. Of course, policy decisions should be made considering these effects in addition to the preponderance of the quasi-experimental and experimental evidence. Based on the existing evidence – and the results from these three chapters – on the effects of private school choice, policymakers should increase access to private school choice programs including private school vouchers, tax-credit scholarships, tax-credit deductions, and education savings accounts. However, these policy decisions must also consider the effects of different types of school choice policy designs on student outcomes. Clearly, we should not expect every single private school choice program to have robust positive effects on students and societies. Consequently, decision-makers need to weigh the benefits of attempting to prevent families from making bad educational decisions with the potential negative effects of burdensome regulations on the supply of private schools (DeAngelis & Burke, 2017; Sude, DeAngelis, & Wolf, 2018). Furthermore, policymakers should consider increasing access to student-level data so that researchers can continue to expand their focus on the non-academic effects of private school choice.

References: Conclusion

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