Three Essays on the Economics of Early Education

A Dissertation
SUBMITTED TO THE FACULTY OF
UNIVERSITY OF MINNESOTA
BY

Brandt Adams Richardson

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

Judy Temple

August 2017

Acknowledgements

I would like to profoundly thank my advisor, Professor Judy Temple, for her support and supervision from my first day in the Applied Economics program through my final defense. Without her guidance and direction, this dissertation would not have been possible.

I am also deeply grateful for the support from Professor Arthur Reynolds, who provided an opportunity, support and encouragement throughout my graduate career.

I would also like to thank my committee members, Elizabeth Davis and Maria Hanratty, for their feedback.

My colleagues and friends at the University of Minnesota, both in the Applied Economics Department and at the Human Capital Research Collaborative, helped me preserve and provided support, commentary and review that were crucial to completion of my dissertation. Thank you.

Finally, thank you to my family. My parents, sister, wife and son. Without their love and positivity, this would not have been possible or worth doing.

Dedication

For their unwavering support and love, this thesis is dedicated to Nicole and Oliver.

Table of Contents

List of Tables

List of Figures

CHAPTER 1: INTRODUCTION

1.1 Introduction

This dissertation is comprised of three essays investigating the impacts of the Child-Parent Centers (CPC), a high quality early education intervention serving children between the ages of three and nine, and their families. The program is described more fully below. The three essays analyze the impacts of CPC program in three key areas: how the program can help build human capital skills, how the program may offset shocks that negatively impact the acquisition of human capital skills and how the program may impact the participating families' consumption of later education.

In 2012, using a new source of federal funding, the CPC program expanded with additional sites in the Chicago Public School district, where the program has traditionally operated, and in additional districts in Illinois and Minnesota. While the effectiveness of the original CPC program is well-documented, Chapter 2 of this dissertation investigates the impacts of the first year of the scaled-up CPC program in Chicago. Using propensity score analysis to address concerns about group differences and attrition, this chapter analyzes the overall impact of the program, while also looking at previously unanalyzed subgroups, including full-day and part-day programming and students that speak Spanish at home. Finally, we compare the impacts of current iteration of the program to the historical impacts of the CPC program to assess the effectiveness of the program in today's academic climate.

Chapter 3 uses the same children analyzed in Chapter 2 to assess the impacts of high neighborhood crime near the preschool site on academic achievement. By collecting the location and timing of crime in the immediate vicinity of the preschool and applying a difference-in-differences technique, we are able to assess the impacts, if any, this crime imposes on the students that attend those schools. The services described in section 1.2 of this introduction detail some of the key elements that set the CPC program apart from typical publicly provided preschool. Chapter 3 investigates if some key elements of the program may help offset any negative impacts of neighborhood crime that may be imposed on the students.

Finally, Chapter 4 investigates the impacts attending the CPC program on later school choice. Given that the CPC has an extensive parent component, in addition to the student component, we test the hypothesis that attending the CPC program will increase the value of education later in life and CPC families will seek out higher quality academic opportunities. We use propensity score methods in combination with coefficient bounding methodology to identify the impacts of the program on high school choice.

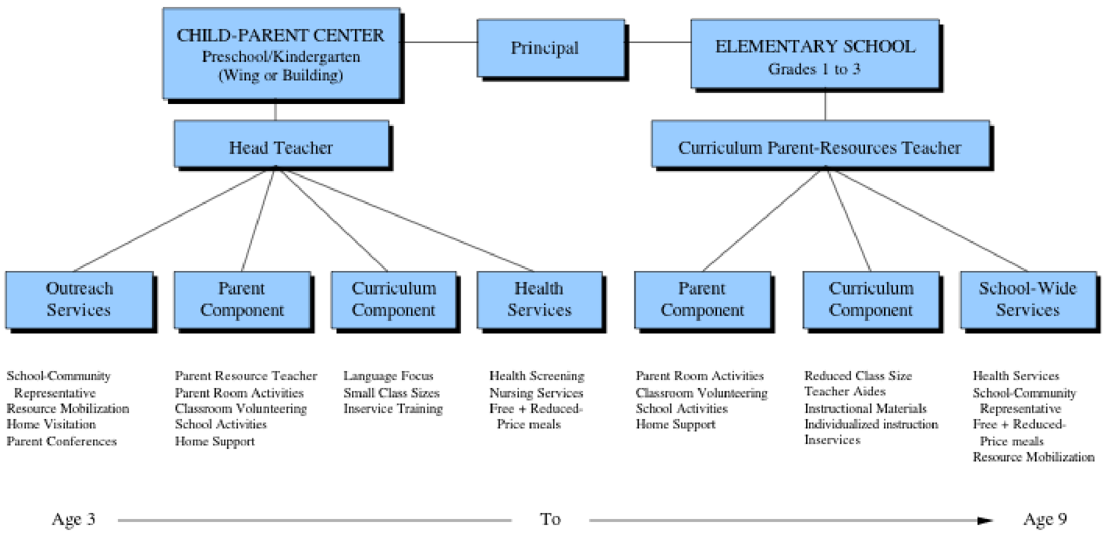
While all three chapters use varying methods of probability weighting to address concerns about group comparability, each chapter uses a different approach to address additional threats to validity. Each paper faces unique concerns regarding the estimation of valid results, from the possibility of attrition in Chapter 2, to the omitted or endogeneous variables in Chapters 3 and 4. The combination of approaches in each chapter helps minimize concern regarding these threats and increases the robustness of the results.

1.2 Child Parent Centers

The Child-Parent Centers (CPC) are an early childhood intervention program that began in low-income neighborhoods in Chicago. Founded in 1967, the CPC aims to provide comprehensive educational and family support to economically disadvantaged youth in urban Chicago. By doing so, the CPC program aims to improve social and educational outcomes in both the short and long-run. CPC serves children from preschool through third grade, making it the oldest federally funded extended early childhood intervention in the country. After Head Start, it is the second oldest federally funded preschool program. Historically, the Child-Parent Centers have been funded through No Child Left Behind funds and Title I funding (Reynolds, 1999), though in recent years, new funding sources have arisen.

The CPC program, as the name suggests, is comprised of a student component and a parent component, that work in conjunction to help develop the skills of the attending students. The student component has several key elements that focus on improving the skills of the CPC students. A head teacher, responsible for implementing the program and training the CPC classroom teachers, directs each CPC site. Small class sizes, with a teacher's aide, limited to a ratio of 17 students to 2 adults, promote individualized attention and helps student develop social skills. Classroom teachers implement curriculum focused on developing math and literacy skills in the students. The parent component, led by the Parent Resource Teacher, originally required 2.5 hours per week of parent involvement at the site. While this typically was volunteering in the classroom or on field trips, some CPC sites offered activities for parents, including GED coursework or parenting skills (Reynolds, 1999). Figure 1 describes the services provided by the CPC program between kindergarten and third grade.

Child-Parent Center Program



Evidence of the effectiveness of the CPC program has traditionally been examined using the Chicago Longitudinal Study (CLS). The CLS follows 1,539 low-income, minority students born in 1980. Of the total sample, 989 attended CPC programs at twenty different sites, while the control group of 550 children attended five randomly-selected Chicago Public Schools alternative programs in kindergarten. The control group represented the treatment as usual condition, with the majority of the sample receiving no structured preschool programming, while roughly 10 percent of the sample attended a Head Start program (Reynolds, 1999). In 2017, this sample is still being tracked and data collected to help evaluate the impact of the program on life course outcomes. There is a well-documented history of effectiveness of the program. Recent impact evaluations and cost-benefit analyses can be found in Reynolds et al. (2011a), Reynolds et al. (2011b), Temple and Reynolds (2007), Reynolds et al. (2001) and Arteaga (2010). Evidence of CPC effectiveness using data from an earlier study is reported in Fuerst (1993). Some recent findings of program effects indicated that the CPC program has an effect on educational attainment, involvement in crime and other outcomes. A few notable findings are that participation in the preschool program is associated with improvements in on-time high school graduation from 36.6% to 44.3% and reductions in having any arrests from 54.3% to 47.9%. Compared to students with no participation, extended participation from preschool through second or third grade is associated with improvements in on-time graduation from 31.3% to 48.6%. These and other outcomes are reported in Reynolds et al. (2011a). Benefit-cost analyses reported in Reynolds et al. (2011b) indicated that an additional dollar spent on preschool program generates $10.83 in social benefits with the extended program having a benefit-cost ratio of $8.24 to $1.

Despite the positive impacts of the program, due to budget concerns, the number of CPC sites in Chicago dwindled from a maximum of 24 in the mid-1980s down to 11 by 2011. In 2012, with support from the Federal government through an Investing in Innovation (i3) grant and local private funds, an effort was made to scale-up the CPC program as part of the Midwest CPC Expansion project (MCPC). Five school districts in Minnesota and Illinois opened new CPC locations. With the additional funding, the MCPC project was able to refine the six key elements of the CPC program and target populations that had previously not been served, including schools with a majority Hispanic population and those outside of urban areas. Key program refinements are outlined in Table 1:

Element	**CLS (1985)**	**MCPC (2012)**
Collaborative Leadership	Low Principal Involvement	High Principal Involvement
Effective Learning	Teacher-Directed Half-Day	Balanced Full-Day
Curriculum Alignment	None	Endorsed Plans
Parent Involvement	School-Focused	Menu-Based
Professional Development	Leadership Team	Full System, Modules, Coaching
Continuity & Stability	Limited	Major Outreach, Staff Support, Mentors

The refinement of the CPC model has been studied previously by Gaylor et al (2016), Reynolds, Richardson, Hayakawa, Lease, Warner-Richter and Englund et al (2014), Reynolds, Richardson, Hayakawa, England and Ou (2016) and Richardson, Reynolds, Temple and Smerillo (2017). By comparing the outcomes of 1,724 treatment students to the outcomes of control students in matched Chicago Public School preschools, these early studies have found that the first year of the CPC program has been effective in increasing school readiness among the first year. Encouragingly, the results have been significant despite the differences in comparison groups between the CLS and MCPC implementations. While only 10 percent of the CLS control students received public preschool, every MCPC control student attended public preschool. So, while inferences drawn from results of the CLS sample are the results of a high quality early intervention program, it cannot be said definitively that the CPC program works because it is high quality or because preschool programming is effective. The results drawn from the MCPC program indicate that the quality of the program is driving the results as the CPC students are compared exclusively to those students in a publicly-provided preschool. Chapters 2 and 3 of this dissertation use data from the MCPC sample to determine impacts of the CPC program, while Chapter 4 examines the impacts of the program on the CLS sample.

CHAPTER 2: EVALUATING THE IMPACTS OF THE FIRST YEAR OF THE MIDWEST CPC EXPANSION: A PROPENSITY APPROACH

2.1 Introduction[1]

In recent years, public funding of early childhood programs has continued to expand. Expenditures for the U. S. Department of Education's Race to the Top contest, preschool development grants to states, enhancements to federal Head Start programs, block grants to states for child care, and state expansion of prekindergarten programs totaled more than a billion dollars in new funding over the past five years (Council of Economic Advisors, 2015). Public-private sector initiatives such as Pay for Success have also been implemented to help fund public preschool programs (Temple & Reynolds, 2015). An important goal of these investments is expanded access to high-quality programs at city, state, and national levels.

Extensive research has consistently shown that participation in effective preschool and early education programs can improve school readiness skills, subject-matter achievement, and reduce the need for later remedial education services (Karoly & Auger, 2016). However, some of the evidence of the effectiveness of preschool programs is mixed, indicating that while there are developmental and cognitive effects, state-funded preschool programs may not impact special education rates, behavioral problems or parent involvement (Gilliam and Zigler, 2000). Other preschool programs have been found to have a drop off in effects as children get older (Currie and Thomas, 1993), though evidence of effectiveness persists in several areas, including increased earnings

---

[1] Reprinted in part from *Children and Youth Services Review.* School readiness in the Midwest child-parent center expansion: A propensity score analysis of year 1 impacts. Richardson, B. A., Reynolds, A. J., Temple, J., & Smerillo, N. (2017).

Key Abbreviations:
CPC: Child-Parent Centers
MCPC: Midwest Child-Parent Centers
CLS: Chicago Longitudinal Study
TSGOLD: Teaching Strategies GOLD, a teacher assessment
IPWRA: Inverse probability weighting with regression adjustment

and lower likelihood of committing crime (Garces, Thomas and Currie, 2000). Exemplary and high-quality programs have also demonstrated economic returns of more than 7 dollars per dollar invested (Heckman, Moon, Pinto, Savelyev & Yavitz, 2010; Reynolds and Temple, 2008; Schweinhart, Barnes & Weikart, 1993). This evidence was cited in the President's State of the Union Address in 2013 as the rationale for the Preschool for All initiative.

One of the leading examples of an evidence-based, early childhood program with a high return-on-investment is the Child-Parent Centers (CPC; Reynolds, Temple, Robertson & Mann, 2002). CPC has not only shown positive effects on school performance and achievement (Reynolds and Ou, 2010), but also in crime reduction (Reynolds, Temple and Ou, 2010). The CPC program is currently undergoing scale-up in the Midwest through new federal investments and Pay for Success initiatives.

The Child-Parent Centers, a high quality, early education intervention that serves students from preschool through third grade, began operation in 1967. By the mid-1980s there were 24 centers in Chicago, but despite the well-documented positive effects of the program, the number of sites fell to 10 by 2011, primarily due to lack of financial support. In 2012, with support from the Federal government through an Investing in Innovation (i3) grant and local private funds, an effort was made to scale-up the CPC program as part of the Midwest CPC Expansion project (MCPC). Five school districts in Minnesota and Illinois opened new CPC locations. This paper focuses on the 16 CPC sites (10 existing and 6 additional) operating in Chicago. For comparison, data was collected from 14 Chicago Public School preschools to serve as the control group.

While the impact of the original CPC program has been well-documented through the Chicago-Longitudinal Study (CLS), we investigate the impacts of the scaled-up program following key changes in the implementation, including responding to parental needs by offering full-day rather than half-day programming in some sites. Additional changes were to enhance parental involvement experiences and to offer additional professional development to teachers. Another key change was the nature of the comparison group. While in the 1980s few comparison group children attended preschool, in the current research the CPC participants are compared to children who attended state-funded preschool offered in the Chicago Public Schools. A final difference between the CLS study and the current evaluation of the CPC program is the inclusion of Hispanics in the current sample. The original study of the CPC program was based on a sample that was almost entirely African-American.

We examine the impacts of the program on teacher-administered assessments that evaluate students on their mathematics, literacy, socio-emotional and science skills, as well as an overall score of school readiness. We also investigate the impacts of full- and part-day CPC programming, as well as the differential impacts by free lunch status. We also examine CPC preschool effects for children whose families speak Spanish at home. Finally, we use the same methodology to examine the impacts to the first year of the original CLS sample on school readiness to compare the effect sizes across implementations, roughly 30 years apart. We focus on the first year of the program, analyzing the impacts of one year of CPC preschool.

Given significant differences in the socio-economic characteristics of the treatment and control group despite using a matching procedure to determine the control sites, we use inverse propensity score weighting regression adjustment (IPWRA; Wooldridge, 2007) to estimate treatment effects and to address dosage issues of full versus half-day intervention. The estimation approach is described as doubly robust because results are unbiased if either the regression model or the propensity score equation is mis-specified. While differences in group characteristics at baseline may cause bias, we are also concerned about differential attrition influencing the results. Though 89 percent of the sample had at least one spring teacher assessment, only 74 percent had assessments for each domain analyzed (math, literacy, science, socio-emotional learning and overall score). If the attrition is non-random, this may also bias the results. To address this, we apply the IPWRA methodology, weighting for both treatment probability and attrition probability.

2.1.1. Child-Parent Centers and the Midwest CPC Expansion

The original CPC program was offered in schools located in high poverty neighborhoods in Chicago and targeted students between the ages of 3 and 9. The CPC program offered a high-quality preschool program staffed by teachers with four-year degrees and small class sizes of 8 or 9 children per teacher or teacher’s aide, a 17:2 child to adult ratio. Started in the 1960s and still ongoing today, the CPC program is a comprehensive, educational intervention with an intensive parental involvement component. Students may enroll in the CPC preschool program for one or two years and then continue in the elementary school component of the program that offers small class sizes, field trips, and a modest amount of additional classroom resources.

The name “Child-Parent Center” indicates that parental involvement is an important component of the program. Parents were expected to volunteer at least one-half day each week at the center in various capacities. Each CPC site had a dedicated parent-resource teacher who encourages parent participation and a parent resource room that provides a location for parent program activities.

The effectiveness of the CPC program is well-documented. The Chicago Longitudinal Study follows 1,539 low-income, minority students born in 1980. Of the total sample, 989 attended CPC preschool programs at twenty sites, while the control group of 550 attended either five randomly-selected Chicago Public Schools with alternative programs or enrolled in CPCs in kindergarten without preschool participation (Reynolds, 1999). Reynolds, Temple, White, Ou and Robertson (2011) examine the impacts of the CPC on educational achievement, special education, crime and welfare. These benefits are monetized and weighed against the costs of the program. Using adult data through age 26, the authors find a total public and private $10.83 return for every dollar invested for the preschool program and a $3.97 return for every dollar invested for the school-age program. The largest components of these benefits are the savings of reduced crime and the increased earnings capacity and tax revenue (Reynolds et al, 2011). These results are indicative of the positive effects the CPC for both the individual and society as a whole. Arteaga, Humpage, Reynolds and Temple (2014) use propensity score weighting to analyze the dosage impacts of one or two years of CPC participation using the CLS sample. Similar to previous studies, they find positive impacts of one year of program participation on academic achievement, health and adult SES outcomes. In addition, students that elect to attend CPC for a second year are less likely to receive special education or to commit crime compared to those students that only received one year of the intervention. A recent evaluation of the CPC preschool program by Gaylor et al. (2016) indicated that participation was associated with a lower probability of special education placement in kindergarten and higher rates of school readiness. These studies are a small selection of the available literature on the CPC, but they are representative of the positive results seen by both individual students and to the communities that implement the program.

Although the program has been in existence for 50 years, studies of the CPC are especially important in light of the recent strong national interest by policymakers in Prek-3 education. As described by Shore (2009) and Takanishi (2011), concern about the perceived lack of persistence of the benefits of preschool, especially for economically-disadvantaged children, has focused attention on the need for early programs that last longer, perhaps through third grade.

Given the success of the original CPC program, the model was modified and expanded in the 2012-2013 school year in Chicago, Evanston and Normal, IL and Saint Paul, MN. Drawing from the foundation of the original model, the expansion program focused on parental involvement, small class sizes and improving kindergarten readiness. However, the Midwest CPC expands on the original model by developing six main components that must be met: a collaborative leadership team lead by a head teacher, effective learning experiences driven by small class sizes and certified teachers, parent involvement and engagement, an aligned curriculum from preschool through 3rd grade, a focus on continuity and stability, and professional development (Human Capital Research Collaborative, 2016). In the first year of implementation, there was a strong push to develop key leadership teams in each site, led by the principal. These teams focused on offering an aligned curriculum from preschool through third grades to reduce the possibility of fadeout effects and increased professional development for the teachers. These six components are the driving factors for increasing the human capital skills of the CPC students, but also increase the generalizability of the model.

In the first analysis of the Midwest CPC expansion, Reynolds, Richardson, Hayakawa, Lease, Warner-Richter & Englund et al (2014) analyze the impacts of full-day preschool. Using a sample of students in Chicago from schools that had both full and part day CPC programs, Reynolds et al (2014) find positive impacts of the full-day program on early school readiness, measured by the teacher assessment Teaching Strategies GOLD (TSGOLD), as well as improved attendance, both increased average daily attendance and reduced chronic absence. Compared to the original CPC program that almost exclusively served low-income, black students, the Midwest CPC program provides services to a much wider variety of students, which allows for more robust subgroup analysis.

2.1.2. Scaling Up Programs

While high-quality, model preschool programs have been shown to have strong impacts on children's development (Schweinhart, 1997; Reynolds, 2000; Campbell, Ramey, Pungello, Sparling, & Miller-Johnson, 2002), not as much research is available to document the success with which model preschool programs can be brought to scale. Impact evaluations have shown mixed results on the ability of scaled-up programs to produce the same lasting impacts for children as model programs, with some studies showing gains fading out by early elementary (Puma, Bell, Cook, Heid & Lopez, 2010; Lipsey, Hofer, Dong, Farran, & Bilbrey, 2013;) and others showing sustained gains (Barnett, Jung, Youn, & Frede, 2013).

For an expansion to be successful, the scaled-up program must be "reasonably similar" to the model program (Schweinhart, 2007) so fidelity to the original model is important (Rohrbach & Dyal, 2015). Challenges to scale-up are adequate funding, as well as facility and leadership capacity (Lauter and Rice, 2008; Bumbarger & Perkins, 2008; Rohrbach & Dyal, 2015), but strong collaboration between school districts, school principals, and preschool providers can help scale-up efforts to achieve positive results for children (Lauter and Rice, 2008; Hayakawa, Englund, Candee, Lease, Sullivan, Warner-Richter, et al., 2015). Additionally, there are institutional and political factors that influence the capacity to scale, including cost, organizational commitment, and service fragmentation (Reynolds, Hayakawa, Ou, Mondi, Englund, Candee, et al., in press; Cooper, Slavin, & Madden, 1997; Domitrovich & Greenberg, 2000; Greenberg, 2010).

2.2. Theory

At its core, the CPC program focuses on increasing the school readiness skills of its participants, including an emphasis on cognitive skills, literacy and numeracy, as well as non-cognitive skills like socio-emotional learning. These skills not only manifest in the first year of program, but also make subsequent learning more effective. The acquisition of these skills is fundamentally a human capital model (Becker, 1962; Grossman, 2000, Almond and Currie, 2010, among others) that focuses on that acquisition of skills across time periods. Duncan and Magnuson (2013) outline how preschool programs are fundamentally human capital investments that see returns in the long-run, even if there appears to be fadeout of impacts on academic achievement. Heckman (2006) notes the importance of investing in the skills of economically-disadvantaged children (which the CPC program serves).

We base our analysis on the model of human capital presented by Cunha and Heckman (2007), where the capabilities of a student are a function of the genetic and environmental conditions of the child at preschool entry, θ_t, the human capital of the parents, h, and the investment in education, I_t, at any age t, such that:

1) $\theta_{t+1} = f_t(h, \theta_t, I_t)$

Thus the skills obtained at the end of the school year, time period t+1, are a function of the skills the student entered the school year with, the human capital skills of their parents, and the investment into human capital skills of the children that are made by the parents. We use this framework to help inform our model choice and to better understand the impacts of the CPC program. If we view attending CPC as increased investment in human capital skills, we should see an increase in those skills by the end of the preschool year, relative to the control sample and controlling for parent human capital and baseline human capital skills of the child. Similarly, we should be would expect the full-day group to outperform the part-day group when we examine the impacts within the CPC. Or, because:

2) $\partial f_t(h, \theta_t, I_t) / \partial I_t > 0$

where the delta term is a partial derivative and represents a predicted change in the measured skills resulting from a small change in the investment in education. Assuming that the magnitude of the education investment is described as:

3) $I_{CPC\ Full-Day,t} > I_{CPC\ Part-Day,t} > I_{Control,t}$,

then given equation (2) we should then see the outcomes varying as:

4) $\theta_{CPC\ Full-Day,t+1} > \theta_{CPC\ Part-Day,t+1} > \theta_{Control,t+1}$.

Moreover, we would also expect $f_t(h, \theta_t, I_t)$ to be increasing in h, the parent's human capital. If we are to isolate the impact of increased investment due to CPC, we must ensure that we account for any differences in h and θ_t, which include measures of parent education and fall baseline test scores, respectively. Without controls for those measures, we may incorrectly attribute increases in human capital from CPC investment to unrelated family or child characteristics.

2.3. Method

2.3.1. Sample

The total enrollment sample for the first year of the Midwest CPC scale-up program consists of 1,724 students in 16 Chicago CPCs and 868 students in 14 matched Chicago Public School preschools. While in the total sample 79 percent of the students were present for the entire school year, frequent mobility among the other students led us to define program participation as at least four months of attendance in a CPC site and enrolled in the program by January 1st. The administrative data received from Chicago Public Schools contained information on more students than could be physically accommodated in the preschool locations at any one point in time. By defining the sample with this rule, we ensure that students were present for a minimum of half the school year and that the enrollment matches the physical capabilities of the preschools.

Our sample includes 1,289 students in Chicago CPCs and 584 students in public preschool at matched control schools that had at least one valid TSGOLD score. We analyze the 10 existing and 6 the new CPC sites in this sample. The six additional sites were chosen to help broaden the target population of the intervention. Using all Chicago public preschools that were not part of the CPC program, matched control sites were chosen based on a propensity score of school participation, estimated on key demographic characteristics of the schools, including ethnic breakdown and 3rd grade test scores. Schools were matched individually based on these propensity scores, the neighborhood of the school and the schools' willingness to participate. All but one school agreed to participate and another control site was chosen in that school's place.

These matched sites create a set of schools from which individual control students were drawn and compared to the CPC students. Control students received half-day preschool programming that represent the typical Chicago Public preschool services including Head Start and the state pre-k program. All students, treatment and control, received preschool programming, but the CPC group received additional services above and beyond what the control group students received.

Table 2.1 presents the individual level characteristics of the sample, both at the beginning of the preschool year and the end of the year. There are significant differences in race, home language and the literacy baseline test score (adjusted for age, as the sample includes both 3- and 4-year old students), though this may be accounted for by the significant difference in percent Hispanic and ELL status. There is no significant difference in math scores or the summed total score of the six domains. Despite the attempt at school level matching, the significant differences at baseline must be addressed in order to draw casual inferences of the effectiveness of the MCPC Expansion.

2.3.2. Key Outcome Measures

To estimate the impact of the CPC program on school readiness, we used scores on the Teaching Strategies Gold Assessment System (TSGOLD; Lambert, Kim, and Burts, 2013a, 2013b, 2014). As a teacher-rated performance assessment measuring multiple domains of school readiness, TSGOLD ratings are completed three times during the school year (fall, winter, spring). They are routinely collected as part of the school district assessments. Based on observations of classroom performance, teachers rated students on a scale from 0 to 9 (low to high skill proficiency). For science, the items were rated from 0 to 2. Raw scores are summed to obtain subscale scores and whether students meet national norms is based on age and the norming population (Lambert et al., 2013a, 2013b; Soderberg, Stull, Cummings, 2014). Internal consistency reliabilities are high (> .90; Lambert et al., 2013a). Table 2.2 presents summary statistics of the domains, as well as sample items from each subscale.

The validity of performance assessments is well documented by the National Research Council (Snow & Van Hemel, 2008). The advantage of TSGOLD and similar assessments is that scores are based on greater knowledge of child behavior in the school context. Ratings, for example, occur after 4 to 6 weeks of observation in the classroom. The assessment is also aligned to district and state standards, covers all domains of learning, and is linked to the curriculum and opportunity to learn principles. In support of construct and convergent validity, TS GOLD scores are moderately correlated with standardized assessments, and are predictive of later learning (Joseph, McCutchen et al., 2011; Kim, Lambert, & Burts, 2013; Teaching Strategies, 2011).

We examined the impact of CPC on TSGOLD math, literacy, socio-emotional, and science domains, as well as a total score comprised of the six domains (math, literacy, language, socioemotional wellbeing, physical heath and cognitive development). We also analyze the impact of CPC on meeting the national norm standard. No national norm is available for science. Table 2.3 presents t-tests of the unadjusted mean differences in the outcome data by CPC and comparison, as well as by missing status. Before accounting for group differences, we see significantly higher raw test scores and percent at the national norm for the intervention group, suggestive of positive impacts of the CPC program. When looking at differences by attrition sample, the pattern is less clear. Part of this is due to the fact that few students had spring scores on one domain, but not all the domains. Of the sample, 75% of the students had TSGOLD scores for the five domains discussed in this paper, while 11% had none. Only 14 percent of the sample had between one and four TSGOLD spring scores. Those with all TSGOLD scores, prior to adjustment, outscored those missing at least one in math and science, but performed lower on socio-emotional learning. Below we discuss the use of a robust propensity score analysis to determine the impacts of the program, accounting for differences in the distribution of baseline characteristics and differences in attrition rates.

When we compare the results of the first year of the MCPC expansion program to the original CLS cohort that attended CPC in the 1980's, we cannot use the TSGOLD for the CLS cohort as it was not administered. However, at the beginning of the kindergarten year, the CLS students were tested on school readiness skills using the Iowa Test of Basic Skills (ITBS). The ITBS is a reliable and valid (Hildebrand, Hoover and Hildebrand, 1987) assessment that captures student ability on reading and math.

2.3.3. Analysis Plan

To estimate the impacts of the first year of the Midwest CPC program, we use inverse propensity score weighting with regression adjustment (IPWRA). Given the significant differences in the groups at baseline, our analysis focuses on propensity score analysis (Rosenbaum and Rubin, 1983) that can potentially reduce bias in nonexperimental study designs. As described in Wooldridge (2007) as a doubly-robust estimator, inverse propensity score weighting with regression adjustment involves a comparison of two regressions modeling the outcomes – one for the treatment group and the second for the comparison group. Inverse propensity score weights are used to estimate corrected regression coefficients. The estimated treatment effect in a regression adjustment framework is the difference between the weighted, predicted outcomes of the treatment and comparison group. We combine this approach to address differences in group characteristics with a similar approach to limit bias that may arise from differential attrition. We follow the approach laid out by Seaman and White (2013) where we combine the IPW weights for treatment selection with IPW weights for the probability of a student having a valid spring TSGOLD score, or that the student is a member of the recovery sample (R=1; if no spring score, 0). A recovery sample is determined for each domain analyzed. The weights representing treatment probability and attrition probability are multiplied and used to weight the regression adjustment framework to jointly control for differences that may arise from both groups.

We follow guidelines laid out in Caliendo and Kopeinig (2008) to estimate the probability of program participation, focusing on variables that only occur prior to program participation and are informed by prior CPC studies (Arteaga et al, 2014; Reynolds et al., 2011) and economic theory from the human capital model. Given the importance of the parent's investment in Heckman's human capital model, we include parent survey data in our CPC prediction model. We estimate the propensity for CPC participation in equation 4 using multiple specifications, controlling for various baseline characteristics.

5) $\Pr(CPC_i = 1|X) = \beta X_i + e_i$

Where the probability of attending a CPC preschool is a function of demographic characteristics of both the child and the family. Table 2.4 presents three probit models to predict CPC program participation. Model 1 uses only the administrative data available from the start of the preschool year. Model 2 includes several parent survey variables completed by families at the beginning of the school year, imputed using MVA methodology and demographic characteristics. Also included in this model is an indicator of whether or not a family completed the parent survey. It also includes an imputed fall TSGOLD assessment score and dummy indicating if the score was imputed. The final model is included for robustness testing and includes school level achievement of 3rd graders in the year the preschool students entered the school. We use model 2 for our analysis as the balance of variables post-weighting was best (see Appendix A), however, the estimation results did not fundamentally change across the three prediction models, see Appendix A for results. Figure 1 demonstrates the distribution of propensity scores. Next, we compute the inverse probability weights and estimate weighted regression models to obtain predicted outcomes for each treatment level, T_i.

6) $w_{1i} = \frac{T_i}{\Pr(T_i|X)} - \frac{1 - T_i}{1 - \Pr(T_i|X)}$

The estimated weight, w_i, favors treatment students with a lower probability of attending CPC, based on demographic characteristics, while weighting control students with a higher probability of attending CPC (despite attending a control site) more heavily. This weighting procedure creates a control group that is more similar to the treatment group and helps minimize bias that arises from differences in the distribution of observed covariates (Rosenbaum and Rubin, 1983**)**. The weights ranged from 1.04 to 18.9 and no observations were outside the overlap condition. We begin by estimating the probability of CPC participation or not, but extend the analysis to a multi-level weight for the probability of attending CPC full-day, CPC half-day or control preschool programming. We use a Poisson distribution for TSGOLD data and logistic distribution for the national norm dummy variables. We take the difference in the predicted, weighted outcomes by treatment status to estimate the Average Treatment Effect (ATE) to estimate the causal impact of the Midwest CPC program (Wooldridge, 2010). In the outcome model, we include all variables from the propensity score model as well as a measure of school-level reading achievement.

We then estimate a model to predict the recovery sample probability for each domain. Following the guidelines (Seaman and White, 2013) and methodology previously applied to CPC analysis in the CLS (Reynolds, et al, 2011), we use the set of demographic characteristics used to predict treatment participation, including fall test score for each domain, as well as CPC participation status, average attendance rates and a school level characteristics to derive a predicted probability of sample recovery:

7) $\Pr(R_i = 1|X, Z) = \beta_1 X_i + \beta_2 Z_i + e_i$

that is a function of student and school level characteristics. See Table 2.5.

We use model 3 to capture both student characteristics and school characteristics to predict attrition. We then compute the probability of sample recovery weight simply as

8) $w_{2i} = \frac{1}{\Pr(R_i|X,Z)}$

which, when applied, helps account for any differential attrition. Using this structure of weights, we weight the results of students that were less likely to have be in the recovery sample but have spring TSGOLD scores more heavily than those with a higher probability of sample recovery.

After weighting, there are significant differences in 3rd grade school level achievement and language spoken at home, however, the weighting procedure removed the significant differences in CPC participation rate and free lunch status. Of note, there was no difference before or after weighting between attrition status and fall test scores. Table 2.6 presents these results. Finally, we combine the weights to weight the regression adjustment to calculate the Average Treatment Effects of the first year of the MCPC expansion program such that the weight:

9) $w_{3,i} = w_{1i} * w_{2i}$

We estimate the impacts using both the treatment weight, w_{1i}, and the combined treatment and attrition weight, w_{1i}, to estimate the impacts and compare the results. We focus on IPWRA estimation for several reasons; first, the estimation strategy allows flexibility in estimating both the CPC participation model and the outcome model. This allows us to include variables that may influence the outcome, such as baseline test scores and school quality, but may not influence the probability of treatment participation, such as fall TSGOLD scores, for example. Another advantage of the estimation strategy is that IPWRA methods are doubly robust (Robins and Rotnitzky, 1995; Wooldridge, 2007) in that only the specification of either the treatment prediction model or the outcome needs to be correct to provide consistent estimate of the impacts of the CPC program. Finally, unlike propensity score matching, IPWRA methods use all available data without discarding observations and allow for multilevel treatment variables, so we can estimate the impact of differences in CPC full or half-day participation versus the control students.

2.4. Results

We begin by estimating the impacts of the first year of the CPC program on TSGOLD scores and national norm rates. By weighting the outcome regressions by the estimated weights obtained from the program participation model, we are able to create a more comparable control group, thereby reducing the standard errors of the coefficients. Table 2.7 describes these results.

CPC students score significantly higher on all school readiness domains, including the likelihood of meeting the national norm standard. CPC students have significantly higher scores (from 3.4 points higher in math to 6.1 points higher in literacy) on all TSGOLD domains tested, compared to similar students receiving the typical Chicago Public preschool programming. Like the original CPC program, the Midwest CPC Expansion program is effective in increasing school readiness among its participants.

2.4.1 Full-Day CPC vs Part-Day CPC:

Given the results in Reynolds et al (2014), it seems possible that that the impacts of the CPC program are being driven by the higher performance of students in the full-day programs and the impacts for CPC part-day students may be minimal. The IPWRA model allows us an intuitive method to analyze this question. We create a trichotomized treatment level variable, where 2 indicates attendance in a CPC full-day program, 1 indicates attendance in a CPC half-day program, and 0 indicates attendance at a control preschool. The IPWRA model allows us to estimate the probability of attending each of those levels and then weights the outcome model by those results. We use CPC part-day participation as the baseline level as this allows us to compare the CPC part-day students to the control group students, while also comparing the CPC full-day students to the CPC part-day students. This allows us to test for significant differences among intervention levels.

We find that the full-day preschool group outperforms the half-day preschool group in raw TSGOLD scores and meeting the math and total national norms, though there is no significant difference in the likelihood of meeting national of socio-emotional outcomes between the intervention groups, indicating the advantage in socio-emotional skills arises from the part-day programming and additional services above the part-day may not be effective in increasing SEM outcomes. There was no significant difference on the rates of meeting the math national norm. Otherwise, the part-day group consistently outperforms the control group in raw score and the percentage of students meeting the national norm for each domain. See Table 2.8. This demonstrates that the overall impacts of CPC are not exclusively driven by the full-day group, but there are positive, significant impacts of the part-day program as well. Students gain skills in the half-day program and continue to improve those skills with extended classroom time.

2.4.2 Subgroup Impacts

Given one of the goals of the Midwest CPC program was to evaluate the impacts of the program on different demographic groups, we investigate the impacts of the CPC program for two key subgroups: Spanish-speakers at home and free lunch eligible students. For each subgroup, we estimated a new propensity score model, limiting the sample to that particular subgroup. The first row of Table 2.9 limits the sample to only those students eligible for free or reduced price lunch. Given the both the CPC and control preschools serve communities with very high rates of poverty, the majority of the sample is eligible for at least some reduction in lunch price. We also estimate the impacts of CPC on those students not eligible for free or reduced lunch.

We find that regardless of free lunch status, CPC students outperform control preschool students of the same lunch status, except for meeting the national norm in literacy for students not eligible for free or reduced lunch. Thus, the results, especially the gains seen in raw TSGOLD test scores, are not driven exclusively by more economically advantaged families. The gains in raw TSGOLD scores are comparable between CPC subgroups, though those gains are more likely to translate into meeting the national norm for the students not eligible for free lunch. Those students have higher baseline test scores so gains are more likely to translate to meeting the national norm. Thus, while the gains from CPC may be similar across free lunch status, it is easier for the non-eligible students to reach the national norm threshold.

Table 2.10 compares the impacts of CPC for students who speak Spanish at home compared to those control students that speak Spanish at home. We also compare the impacts of CPC home Spanish-speakers to CPC students that spoke all other languages at home (97.5% of the non-Spanish speakers spoke English at home). Again, we limit the sample by language spoken at home and re-estimate the propensity scores.

We find similar results to the lunch eligibility subgroup. Regardless of language spoken at home, CPC preschool students outperform similar control preschool students, at least on the raw scores. When comparing the impacts of CPC for only those that spoke Spanish at home we see gains compared to the Spanish-speaking control students for both raw scores and those students meeting the national norms. Interestingly, home Spanish speakers typically had higher initial test scores in math and overall score, so comparable raw gains from CPC are more likely to translate to meeting the national norm in math and the overall score. CPC students that speak Spanish at home are more likely to meet the TSGOLD national norms, compared to control preschool students, by roughly 27 to 35 percentage points. While the mainly English speakers CPC students saw significantly higher raw scores in all domains, there was no significant difference in the rates of meeting national norms for the math domain or the overall score. English-speaking CPC students were more likely to meet the literacy national norm, but less likely to meet the socio-emotional learning national norm, compared to English-speaking control students.

2.4.3 Attrition Weighting

Given the concerns discussed previously, we also implement the attrition and treatment weighting procedure. To correct for possible bias arising from differential attrition, we repeat the analysis performed prior, weighting the regression results by both the inverse probability of treatment *and* the probability of not having a spring TSGOLD test score, following a similar strategy applied to the CLS by Reynolds et al (2011). Of note, there was no significant difference in fall TSGOLD scores for the attrition group by CPC status (223.1 vs 220.7, p-value 0.8767). To control for attrition, we multiply the attrition weight calculated for each missing spring score on each TSGOLD domain by the previously calculated treatment weight to apply the IPWRA specification. When weighting for treatment and attrition, we find the same results as the treatment weight results. See Table 2.11 for full results.

When controlling for attrition, we still find positive, significant impacts of the first year of the CPC program. Again, we turn to the impacts of CPC participation by treatment level. Full results are presented in Table 2.12.

While the CPC part-day compared to controls mirror the previous results, the comparison of full-day CPC to part-day CPC differs on some domains, particularly meeting the national norm. In particular, the full-day CPC students no longer have a significant advantage over the part-day CPC students in literacy or overall national norms, indicating that some of the results in the previous section may have been due to bias arising from differential attrition. However, there still are indications, especially in raw scores, that CPC full-day students are outperforming the part-day students, even adjusting for treatment and attrition probabilities.

Following the previous analysis, we investigate the impacts of the CPC program on various subgroups, again weighting for both treatment and attrition probabilities. Table 2.13 reports the results for all 4 four subgroups analyzed, by free lunch status and home language.

Again, there is a consistent pattern of significant effects of CPC impacts for these subgroups. Overall, while there were some differences in between the IPW treatment weights and the IPW treatment and attrition weights, a general pattern arises, CPC students outperform control students and end the preschool year at a higher level of school readiness.

Additional robustness testing used MVA imputation approaches to fill missing TSGOLD data to estimate the impacts of the program, see Appendix A for results, but are consistent with the results present above. This further reaffirms the robustness of the estimates. Missing data were imputed using the Expectation-Maximization (EM; Schafer & Olson, 1998) algorithm, which yields valid estimates under the assumption of data missing at random. Given the extensive baseline data available for children and families, this assumption was satisfied. The input variables included race, gender, age, free lunch status, parent information including single parent status, employment status and education level and all available TSGOLD data. The sample was limited to students with at least one valid TSGOLD score (1,873 students). There was no significant difference in the TSGOLD scores by the imputed or non-imputed sample, see Appendix A. The results are consistent with other estimates of the impacts of CPC using imputation strategies for the full-sample size (Reynolds, Richardson, Hayakawa, Englund and Ou, 2016 and Reynolds et al, 2014).

2.4.4 Comparison to the CLS model

An important aspect to this analysis is to compare the results of the first year of the scale up program to the results seen in the original CLS sample. With the MCPC sample, we have used TSGOLD scores at the end of the preschool year. The most consistent outcome measure in the CLS data is the Iowa Test of Basic Skills (ITBS) measured in the fall of kindergarten. IPWRA was used on the CLS data to understand if the effect size of the historic CLS model is similar to that of the CPC P-3 model. The CLS data contains a different set of covariates and does not include all the parent-level variables found in the i3 data. Instead of leaving out covariates to make the models match more closely, we tested the best possible model with the available data.

Using IPWRA to estimate the impacts of the CPC program on the total TSGOLD score, we find an estimate effect size of .38 of a standard deviation, certainly smaller than the impacts estimated in the CLS sample of students who attended CPC in the mid-1980s, however, the effect size of the full-day program approaches the impacts of the original implementation. This is especially important because only about 10 to 15 percent of the comparison group sample for the CLS implementation received preschool programming, while 100 percent of the comparison group for the MCPC group received state-funded preschool, either through the district or Head Start. See Table 2.14.

2.6. Conclusion

Study findings show that an expansion of the CPC program to new sites yields practically significant gains in school readiness skills. These gains occurred above and beyond those of students who attended the usual preschool programs in the Chicago Public School District, either Head Start or Chicago Public preschool programs. The results of this paper are not indicating the effectiveness of the preschool intervention, but rather the effectiveness of the intervention above and beyond existing, publicly provided preschool programs. Analyses of a range of state prek (Magnuson, Meyers, Ruhn, &

Waldfogel, 2004) and Head Start programs (Currie & Thomas, 1993; Puma, 2005) demonstrate that they can be effective in improving school readiness. Recent evidence on large-scale programs, including Tulsa and Boston, indicate the average effect sizes range from .18 to .63 (Yoshikawa, Weiland and Brooks-Gunn, 2016, Gormley, Gayer, Phillips & Dawson, 2005, Lipsey et al., 2013; Weiland & Yoshikawa, 2013; Wong, Cook, Barnett & Jung, 2008). With an effect size of about .40, the results of this study show that the innovations of the CPC program improve student performance significantly above already effective services. We hypothesize that these gains arise from the key requirements of CPC, which includes classes sizes of no more than 17, state-licensed teachers, a leadership team in each center, family support services, and professional development. These and other requirements were specifically designed to target and improve the school readiness of vulnerable populations.

Our findings were not exclusively driven by students with more learning time through full-day programming. Part-day preschool participants as well as their full-day counterparts outperformed the comparison group. Full-day participants made the largest gains, however. These benefits occurred for under-represented populations, including Hispanic children, and those from more diverse socioeconomic and ethnic contexts. Regardless of subgroup status, CPC students outperform the comparison students attending district programs. These results are important because the evidence of effectiveness for these subgroups has been limited in the original CLS evaluations and indicates a broader effectiveness of the program. This suggests that further scale-up of the CPC program is not only feasible but can produce larger effects than are typically found for publicly funded preschool programs (Camilli, Vargas, Ryan & Barnett, 2010). With evidence of effectiveness across multiple subgroups, including students that speak Spanish at home and by socio-economic status, these are indications of increased generalizability compared to the original CLS cohort.

In comparing the present study with CPC findings from the CLS, the larger effect size in the CLS sample is most likely due to the relative absence of preschool participation in the comparison group. Only 15% of the comparison group enrolled in publicly funded preschool whereas the entire comparison group in the present CPC enrolled in either state prekindergarten or Head Start. The .18 SD difference between the two estimates is roughly the impact of centered-based preschool versus home care in the ECLS. In addition, there were temporal differences in the school readiness measures. The TSGOLD measures were administered in the spring of the preschool year. The CLS school readiness measure was administered in the fall of kindergarten. Not only did students have additional months of development, but CLS students that attended CPC received at least one to two months of additional services. Finally, two major school district events occurred during the preschool year that may have deflated effect sizes. A nine-day strike occurred at the beginning of the year, which reduced the number of instructional days from which to contrast CPC versus usual preschool. Moreover, in the winter of the preschool year the district announced the planned closing or reconstitution of more than 50 schools, which were more concentrated among CPC schools. This process may have had a detrimental effect on the learning climate of the affected schools so important for school readiness skills. Consequently, our findings may be conservative.

Despite these caveats, the MCPC full-day students performed at a level comparable to the original implementation. This is evidence that the CPC scale-up program can be successful in contemporary educational contexts, but the program required key changes from the original model.

There are three notable limitations. First, there were significant differences in baseline characteristics between the treatment group and the control group, including race, home language and fall literacy TSGOLD assessment scores. However, using the doubly-robust propensity weighting methodology minimizes the bias that may arise from differences in the distribution of observed variables. It is also important to note that the positive impacts of CPC are still present across home language and racial subgroups.

The second limitation is the short-term nature of the study, as we only examine the impacts of the first year of the preschool program. Thus, we cannot rule out the presence of drop-off effects that have been found in some other preschool interventions (Takanishi and Kauerz, 2008). So while the first year of the scale-up program demonstrates evidence of effectiveness, determination of the implications for a wider scale-up require further investigation. The implementation of CPC is expensive, roughly $1,500 per student above and beyond the costs of publicly-provided preschool. The scalability of the program depends on the sustained gains in subsequent. However, previous research on the CPC program has demonstrated sustained effects (Reynolds et al., 2011) and the impacts of the current sample, especially the full-day programming, follow closely with the results from the CLS sample. In addition, the comprehensive set of educational, family, and professional learning services is greater than most other programs. This would be expected to help sustain gains. As data are available on the performance of students in subsequent school years, this analysis should be expanded to investigate the impact of CPC on school performance in kindergarten and the elementary grades. The use of other assessments besides TSGOLD also will address robustness across outcome measures. Future analysis will investigate the impacts of CPC on student mobility to see if the MCPC implementation, like the CLS implementation, can reduce student mobility within and across years so students can reap the full, potential benefits of an aligned preschool through third grade implementation.

Finally, while we hypothesize that the key program elements account for the observed gains, further research should investigate the impacts of these elements on student achievement. Some but not all of these elements are present in state prek, Head Start, and other center-based programs. They warrant further testing and inclusion in expansion efforts. The availability of full-day services is also a key CPC feature, and the increased learning time was linked to large gains in school readiness.

The positive impacts of the CPC program should be viewed within the context of the changes since the 1980s assessed in the CLS. In the present program, six major elements are emphasized: effective learning experiences, collaborative leadership, parent involvement and engagement, aligned curriculum, continuity and stability, and professional development. The previous model emphasized only the first three, and with less intensity. For example, enhanced elements of effective learning experiences include a curriculum balance of teacher-directed and child initiated activities, full-day preschool, and progress monitoring of instruction. The current Midwest expansion also has a professional development system of coaching, provides program support by site mentors, and implements curriculum alignment and parent involvement plans in collaboration with school principals. These and other elements are likely to contribute to the positive effects. The CPC benefits on school readiness are the added value of the six elements above and beyond that of the usual services. The typically implemented district preschool programs have some but not all of these elements, and at lower degrees to intensity. For example, typical preschool class sizes are 20 whereas CPC has a maximum of 17. A leadership team is also present in each site to manage the entire program and help establish a strong environment for learning. The precise influences of these and other elements, and their contribution to sustained gains, warrant further investigation.

In conclusion, we find evidence that the CPC program is effective in increasing school readiness amongst its participants, across subgroups examined. If we allow for spring TSGOLD scores to represent human capital skills, θ_{t+1}, we find that, despite the limitations of this study, there is evidence that CPC serves as an effective investment for increasing human capital skills.

Table 2.1: *Characteristics of CPC and Comparison Groups at Preschool Entry, 2012-2013*

Child/Family Characteristics**	CPC Group (N=1,724)	Comparison Group (N=868)	Original Sample* *p*-value	End of Year *p*-value
Female child, %	51.6	50.1	.67	.97
Black, %	64.1	44.5	<.01	<.01
Hispanic, %	34.1	54.8	<.01	<.01
Home language is Spanish, %	27.2	48.9	<.01	<.01
School-level proficiency at state assessment (grades 3-8; %)	62.4	60.8	.03	<.01
Age in months on Sept. 1, 2012 (mean)	48.4	48.6	.42	.68
Enrolled as 3-year-olds on Sept. 1, 2012, %	40.4	38.7	.40	.79
Special education status (IEP), % [a]	9.6	9.1	.67	.95
Child eligible for fully subsidized meals, % [a]	85.4	84.0	.33	.93
Single parent family status, % [a]	48.8	46.7	.52	<.01
Fall score on Literacy subscale, mean (SD)	33.7(15.3)	31.4(13.0)	<.01	<.01
Fall score on Math subscale, mean (SD)	22.6 (8.5)	23.2 (7.2)	.13	.56
School readiness, Fall total scale (SD)	192.1(58.8)	190.6 (49.0)	.50	.35

**Original sample was participants who enrolled in the program and comparison group. End-of-Year sample had valid values for one or more outcome indicators. P values show the significance of mean (or percentage) group differences for the program and comparison groups. The comparison group participated in the usual preschool programs in Chicago (Head Start and State PreK) and were matched on the school-level propensity to enroll in the program.*

***Data on child and family characteristics were collected from school administrative records with the exception of low-income status which was a combination of administrative records and parent reports.*

[a]Children have an Individual Education Plan under IDEA. N for single parent family status is 1,455 (parent survey).

[b] *Eligibility defined at <130% of the federal poverty level.*

Table 2.2: TSGOLD subscale sample items and means

Domain	Sample Items*	Fall Mean (SD) Spring Mean (SD)	Percent at/above National Norm
Literacy 12 items	Identifies and names letters Uses and appreciates books	33.3 (15.9) 57.2 (17.7)	10.5% 71.5%
Math 7 items	Counts Quantifies	23.0 (8.9) 36.3 (9.5)	8.5% 69.4%
Science 5 items	Uses scientific inquiry skills Demonstrates knowledge of the characteristics of living things	4.5 (2.2) 8 (2.3)	N/A
Socio-emotional 9 items	Manages feelings Balances needs and rights of self and others	40.6 (13.0) 55.4 (11.9)	10.8% 60.1%
Total Score	Sum of six domains: Literacy, Math, Cognitive Development, Socio-emotional and Physical Health	192.8 (60.0) 277.8 (61.0)	12.0% 64.1%

*Items are rated on a scale of 0 to 9 (Science 0 to 2). Scores at or above the national average in spring are as follows for 3- and 4-year-olds: Literacy (39, 56), Math (27, 37), Socioemotional (46, 57). Meeting the national norm in total score was defined as meeting the norm in at least 3 domains in the fall and 4 in the spring.

***Table 2.3**: Unadjusted mean differences in Spring TSGOLD scores, by treatment and missing status*

VARIABLES	CPC (n=993-1,073)	Control (n=503-526)	p-value	Missing No Spring Scores (n=1,419)	Missing 1 or More Spring Scores (n = 27-163)	p-value
Math	37.8	33.5	0.000	36.5	34.3	0.010
Percent at National Norm	74.7%	56.8%	0.000	69.6	58.0	0.006
Literacy	60.6	50.4	0.000	57.3	55.3	0.188
Percent at National Norm	78.0%	53.1%	0.000	69.8	66.2	0.2516
Socio-emotional	57.3	51.8	0.000	55.0	60.0	0.000
Percent at National Norm	67.0%	44.3%	0.000	58.1	71.1	0.001
Science	8.0	7.3	0.000	7.8	7.3	0.007
Total	287.8	255.2	0.000	276.8	274.9	0.439
Percent at National Norm	69.8%	47.5%	0.000	32.4	55.6	0.2349

Table 2.4: *CPC Participation Prediction Models*

VARIABLES	(1) cpc	(2) cpc	(3) cpc
Black	0.546***	0.533***	0.738***
	(0.171)	(0.190)	(0.197)
Hispanic	-0.309*	-0.306*	-0.430**
	(0.170)	(0.179)	(0.183)
Female	0.0308	0.00907	0.00539
	(0.0636)	(0.0668)	(0.0673)
Special Education Status	0.196	0.230*	0.185
	(0.120)	(0.127)	(0.129)
Age in Months	0.00409	0.0125**	0.0122*
	(0.00490)	(0.00636)	(0.00639)
Free Lunch Eligible	-0.198**	-0.334***	-0.293***
	(0.0976)	(0.106)	(0.108)
Mother High School Graduate		0.288***	0.273***
		(0.0942)	(0.0944)
Single Parent Household		0.103	0.109
		(0.0986)	(0.0992)
Mother Employed		0.188*	0.223**
		(0.0969)	(0.0976)
Missing Parent Survey Data		-0.984***	-1.022***
		(0.0767)	(0.0775)
Fall Total TSGOLD score		-0.00983**	-0.00878*
		(0.00474)	(0.00473)
Missing Fall TSGOLD score		-0.109	-0.0226
		(0.0748)	(0.0771)
School-level Reading Score (3rd grade)			0.0155***
			(0.00248)
Constant	0.261	0.249	-0.823**
	(0.296)	(0.342)	(0.388)
Observations	1,873	1,873	1,873

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: *Recovery Sample Prediction Models*

VARIABLES	(1) In Sample	(2) In Sample	(3) In Sample
CPC	-0.738***	-0.561***	-0.575***
	(0.156)	(0.161)	(0.163)
Black	0.261	0.437	0.466
	(0.349)	(0.349)	(0.355)
Hispanic	-0.00339	-0.0246	-0.0438
	(0.330)	(0.329)	(0.331)
Female	-0.0345	-0.0281	-0.0288
	(0.124)	(0.128)	(0.128)
Special Education Status	0.0395	-0.0359	-0.0424
	(0.237)	(0.241)	(0.241)
Age in Months	-0.0109	-0.0122	-0.0122
	(0.0116)	(0.0119)	(0.0119)
Free Lunch Status	0.699***	0.768***	0.774***
	(0.188)	(0.192)	(0.192)
Mother High School Graduate	0.0219	-0.0201	-0.0224
	(0.174)	(0.177)	(0.177)
Single Parent Household	-0.267	-0.172	-0.170
	(0.178)	(0.179)	(0.179)
Mother Employed	-0.579***	-0.504***	-0.498***
	(0.189)	(0.192)	(0.193)
Missing Parent Survey Data	-0.0715	0.120	0.111
	(0.144)	(0.151)	(0.152)
Fall Math TSGOLD score	-0.000347	-0.00833	-0.00832
	(0.00858)	(0.00878)	(0.00877)
Missing Math TSGOLD score	-0.545***	-0.569***	-0.562***
	(0.135)	(0.140)	(0.141)
Average Daily Attendance		4.703***	4.690***
		(0.539)	(0.539)
School-level Reading Score (3rd grade)			0.00220
			(0.00468)
Constant	2.612***	-1.514*	-1.642*
	(0.640)	(0.800)	(0.846)
Observations	1,873	1,873	1,873

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: *Baseline Characteristics pre- and post-weighting procedure using math domain.*

Child/Family Characteristics	Difference in baseline by Recovery Sample (n=1,873)	Unweighted p-value	Weighted p-value*
CPC participation, %	-12.4	0.000	0.158
Female child, %	-0.3	0.889	0.886
Black, %	1.8	0.536	0.547
Hispanic, %	-1.4	0.641	0.442
Home language is Spanish, %	3.4	0.224	0.008
School-level proficiency at state assessment (grades 3-8; %)	9.3	0.929	0.089
Age in months on Sept. 1, 2012 (mean)	-0.17	0.661	0.611
Enrolled as 3-year-olds on Sept. 1, 2012, %	3.3	0.255	0.162
Special education status (IEP), % [a]	0.5	0.744	0.996
Child eligible for fully subsidized meals, % [a]	6.4	0.001	0.162
Single parent family status, % [a]	-2.5	0.405	0.213
Fall score on Math subscale, mean (SD)	0.11	0.822	0.421

Table 2.7: *Adjusted, Weighted Impacts of CPC on school readiness*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC vs None	3.4***	8.2***	6.1***	13.0***	3.6***	7.4***	0.7***	20.8***	8.4***
	(0.306)	(0.0233)	(0.730)	(0.0272)	(0.368)	(0.0237)	(0.118)	(2.035)	(0.0274)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.8: *Adjusted, Weighted Impacts of Full- and Part-Day CPC Participation*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC Part-Day vs Control	2.7*** (0.318)	4.0 (0.0281)	5.2*** (0.749)	11.8*** (0.0320)	2.9*** (0.417)	7.1*** (0.0270)	0.6*** (0.127)	16.5*** (2.09)	5.8* (0.033)
CPC Full-Day vs CPC Part-Day	1.8** (0.821)	9.0 (0.0561)	4.3*** (1.612)	12.0*** (0.0381)	2.1** (0.868)	-1.9 (0.0569)	0.5*** (0.196)	13.9*** (3.948)	15.0*** (0.0491)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

***Table 2.9**: Adjusted, Weighted Impacts of CPC participation by free lunch eligibility*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC free/reduced lunch vs Control free/reduced lunch (n = 1,308 – 1,418)	3.6*** (0.326)	6.1*** (0.0268)	6.9*** (0.786)	13.9*** (0.0321)	3.7*** (0.391)	7.2** (0.0282)	0.7*** (0.129)	20.7*** (2.107)	6.6** (0.0318)
CPC non reduced lunch vs control non reduced (n=138 - 169)	2.2** (0.845)	22.3*** (0.072)	0.7 (1.666)	28.7*** (0.0821)	4.0*** (0.784)	29.3*** (0.073)	1.6*** (0.311)	13.6*** (4.536)	31.8*** (0.0877)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.10: *Adjusted, Weighted Impacts of CPC by language spoken at home*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC Spanish at home vs Control Spanish at home Lang (n=468-542)	4.0*** (0.571)	29.5*** (0.0436)	3.9*** (1.336)	27.0*** (0.0474)	5.4*** (0.902)	29.7*** (0.0514)	1.6*** (0.203)	26.3*** (4.455)	35.0*** (0.0458)
CPC all other languages vs control all other lagnuages (n=978-1,040)	3.1*** (0.428)	-2.4 (0.0283)	6.1*** (0.991)	7.4** (0.0361)	2.8*** (0.526)	-6.1** (0.0277)	0.6*** (0.171)	14.2*** (2.878)	-4.3 (0.035)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.11: *Adjusted, Weighted Impacts of CPC on school readiness*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC	3.7***	12.2***	6.1***	15.0***	4.2***	15.6***	1.0***	23.0***	10.4*
	(0.382)	(0.0304)	(0.803)	(0.0235)	(0.492)	(0.0279)	(0.279)	(3.582)	(0.0582)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. Attrition prediction model controlled for CPC status, 3rd grade school level reading achievement, race, sex, special education status, free lunch status, mother's education, marital and employment status and a dummy variable indicating completion of the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.12: *Adjusted, Weighted Impacts of Full- and Part-Day CPC Participation*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC Part-Day vs Control	3.3***	12.5***	5.3***	15.1***	4.2***	16.6***	0.8***	24.6***	10.9
	(0.483)	(0.0364)	(0.796)	(0.0287)	(0.687)	(0.0342)	(0.345)	(1.961)	(0.0753)
CPC Full-Day vs CPC Part-Day	2.9***	5.3	6.0***	4.2	2.7	-1.1	0.5*	22.8***	4.7
	(0.731)	(0.060)	(1.503)	(0.0415)	(1.765)	(0.0608)	(0.242)	(5.191)	(0.0396)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. Attrition prediction model controlled for CPC status, 3rd grade school level reading achievement, race, sex, special education status, free lunch status, mother's education, marital and employment status and a dummy variable indicating completion of the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.13*: Adjusted CPC impacts by subgroup, weighting by for treatment and recovery probabilities*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
Free/Reduced Lunch Eligible (n = 1,308 – 1,418)	3.8*** (0.368)	11.9*** (0.0326)	6.8*** (0.682)	15.7*** (0.0295)	4.2*** (0.512)	16.4*** (0.0315)	0.8** (0.319)	24.1*** (2.102)	8.3 (0.0568)
Not Eligible for Free/Reduced Lunch (n=138 - 169)	2.0** (0.762)	23.4*** (0.0309)	0.8 (1.501)	15.2*** (0.0440)	5.5*** (1.185)	25.1*** (0.0479)	1.7*** (0.216)	10.3 (7.288)	19.8** (0.0724)
Spanish Spoken at Home (n=468-542)	3.8*** (1.191)	15.4 (0.0926)	3.0** (1.378)	9.0* (0.0471)	5.8*** (0.744)	22.7*** (0.0696)	1.6*** (0.083)	25.3*** (5.753)	37.4*** (0.0768)
Non-Spanish Spoken at Home (n=978-1,040)	3.5*** (0.622)	10.2*** (0.0319)	7.4*** (1.578)	14.3*** (0.0413)	3.5*** (0.769)	5.6 (0.0378)	0.9** (0.424)	19.0*** (5.848)	-3.8 (0.0170)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

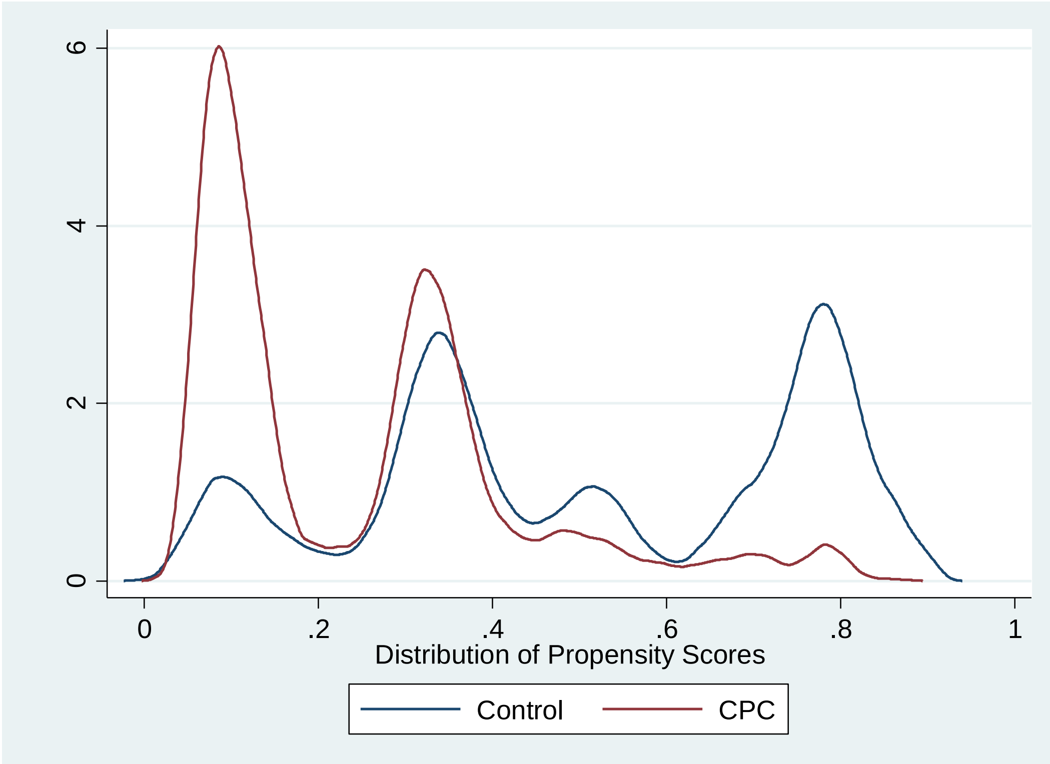
Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. Attrition prediction model controlled for CPC status, 3rd grade school level reading achievement, race, sex, special education status, free lunch status, mother's education, marital and employment status and a dummy variable indicating completion of the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.14: *Comparison of Effect sizes by implementation*

Program	MCPC overall	MCPC part-day	MCPC full-day	CLS sample
Effect Size (Treatment Weight)	.341	.271	.517	.565
Effect Size (Treatment*Attrition Weight)	.334	.294	.571	.591

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. The CLS model controlled for gender, Black, low-income neighborhood, single-parent family, mother's age under 18 at birth of child, mother did not complete high school, more than four children in the family, participate in AFDC, maternal employment status, eligibility for subsidized meals, low birth weight, home environment problems, and child welfare case history by age 4.

Figure 2.1: Distribution of predicted CPC participation propensity scores



CHAPTER 3: EVALUATING THE IMPACTS OF NEIGHBORHOOD CRIME ON PRESCHOOL ACHIEVEMENT: EVIDENCE FROM CHICAGO

3.1 Introduction

This chapter investigates the impacts of neighborhood crime near preschool sites on the academic achievement, health and attendance of preschool children in Chicago. Using data from a federally-funded expansion of Child-Parent Center (CPC) preschool slots starting in 2012 in public schools located in some of Chicago's poorest neighborhoods, this study examines the impacts of neighborhood crime on preschool children and investigates whether high-quality early education services can help offset some of the negative impacts of crime. In 2012, the year that the i3 Midwest Expansion of the CPC began, there were 500 homicides in the city of Chicago according to FBI reports. Those homicides were the most of any city that year (Federal Bureau of Investigation, 2012). Crime generally occurs in the highest poverty neighborhoods and often, at least in Chicago, disproportionally impact African Americans. Given that the majority of CPC sites in Chicago are located in high-poverty areas and have, historically, served a majority of African-American families, there is a fundamental overlap of children exposed to high neighborhood crime and those that attend the CPC program. This provides an opportunity to examine the impacts of neighborhood crime on preschool children, as well as investigate if high quality early education can help offset some of the negative impacts of crime.

The costs of crime are well-documented. In 2009, the University of Chicago Crime Lab estimated that that murders and non-fatal shootings alone imposed total social costs of $2.5 billion on its citizens (Ander, Cook, Ludwig and Pollack, 2009). Many of these costs are borne by citizens not directly impacted by a shooting and include tax-payer costs of trauma care, autopsies, forgone income and the subsequent tax revenue of the killed. Cullen and Levitt (1999) estimate that a murder in Chicago reduces the population of the city by 70 people as migration out of the city increases, while new arrivals fall. This research examines the impacts of neighborhood crime on the cognitive abilities, physical health and attendance of preschool children in Chicago. If neighborhood crime directly impacts achievement, the costs of crime may be much higher than previously estimated.

CPC students and families may be exposed to levels of crime that many preschoolers across the country are not. By collecting police data on crimes that occurred during the school year, we are able to evaluate the impacts of those crimes on the development of the children in those schools. While the impacts of neighborhood crime are well-studied, we begin by analyzing the impacts of crime on children who attend the typical Chicago Public School preschool serving low-income, inner-city children. Furthermore, we also estimate the impacts of crime on students attending a subset of preschools called the Child-Parent Centers. Can a high-quality early education intervention help off-set any of the negative impacts of crime on achievement?

3.2 Literature Review

Economists have long been interested in both the predictors and impacts of crime. Some key determinants include labor market opportunities, changes in demand for criminal goods like drugs (Freeman, 1999) or high income inequality (Kelly, 2000). Recent studies have investigated the importance of education as a determinant of crime. Lochner and Moretti (2004) find that as education rises, the probability of committing a crime falls. Increased levels of education had the biggest reductions on murder, assault and motor vehicle theft rates. Similarly, Machin, Marie and Vujic (2011) find that increases in the minimum dropout age and reductions of the share of the neighborhood population with no education can significantly reduce property crime. Even early education can have a significant impact on the likelihood an individual engages in crime. Reynolds, Temple, White, Ou and Robertson (2011) find that compared to control students, CPC students have higher economic standing, including higher rates of education attainment and occupational prestige. With higher economic standing comes higher opportunity costs of crime and Reynolds et al. (2011) find significantly lower rates of juvenile and adult crime amongst CPC students. Garces, Thomas and Currie (2000) find similar results for African-American Head Start students. These results reinforce Becker's (1968) economic theory of crime. Becker framed the decision of committing crime as a rational choice, where an individual will commit a crime if the costs of being caught are lower than the utility received from the crime. As income rises, the costs of being caught increase and, Becker (1968) theorizes, leads to people committing fewer crimes.

Economists often examine the impacts of crime in two ways: examining the impacts of neighborhood crime and looking at exposure to crimes as individual events. Kling, Ludwig and Katz (2005) investigate the impacts of the Move to Opportunity voucher program. The authors find, that relative to a control group, female youth who have moved to a lower crime and lower poverty neighborhood are less likely to engage in criminal activities themselves. For male youth, there is some reduction in the likelihood of violent crime, however, there is an increase in the less serious property crime. Damm and Dustmann (2014) use the quasi-random assignment of refugee families to neighborhoods in Denmark to find that male youth assigned neighborhoods with a higher share of the population of convicted criminals (but not actual number of crimes committed in the neighborhood) were significantly more likely to commit a crime and be convicted of a higher number of crimes later in life. This relationship did not hold for female youths.

Some researchers focus more specifically on estimating the consequences of shootings and homicides. Gershenson and Tekin (2017) examine the impacts of the 2002 Beltway Sniper attacks on school level proficiency rates of schools within five miles of an attack. Using a difference-in-difference framework, Gershenson and Tekin (2017) find significant and negative impacts on third grade reading test scores and fifth grade reading and math scores. Reductions in test scores were greater for schools that served higher proportions of racial minorities and more socio-economically disadvantaged students. An analysis of test scores in the two years following the shootings found no direct effects of the shootings. Gershenson and Tekin suggest that the reason that schools serving more disadvantaged students were more greatly affected was due to the schools and parents having fewer resources with which to help the students cope with the traumatic events. Aliprantis and Chen (2016) use survey responses from the National Longitudinal Survey of Youth 1997, where respondents indicate whether or not they had seen someone been shot before they turned 12 years old, to estimate the impacts of early exposure to gun violence. Using a series of linear regressions, they find associations of early (by 12 years old) exposure with increased likelihood of committing a crime, lower education status and, for African-Americans, higher mortality. The economic literature portrays a clear story, exposure to crime can negatively impact human capital skills, and may also increase the likelihood of committing a crime.

Other disciplines, including sociology and child development, are concerned with the impacts of crime. Sharkey (2010, 2012) investigated the impacts of a local homicide on test scores and impulse control in preschoolers. A homicide has negative impacts on vocab and reading skills, as well as attention and impulse control. They find that these impacts persist for several days, but may disappear in the long run. Other research (Bowen and Bowen, 1999) finds that neighborhood crime may have a negative impact on achievement in teens. In a study of Baltimore preschool children, Caughy, Hayslett-McCall and O'Campo (2007) find high neighborhood violent crime had a negative impact on cognitive scores, though the impacts of the surrounding neighborhood were insignificant. However, Weir, Etelson and Brand, 2006 find only a weak correlation of parent anxiety with regards to child safety from crime and the actual physical activity that child partakes in. Bowen and Bowen (1999) find that in adolescent students both being and victim of crime and the perception of high neighborhood crime are associated with lower attendance, trouble avoidance and grades. Bowen, Richman, Brewster and Bowen (1998) find that children in middle and high school that have a high perception of danger in schools may have lower engagement in school and lower confidence in their abilities to succeed at school-related activities. While these impacts are on older students with more agency (particularly those students in high school), there are studies that examine the impacts of parent perception of crime in younger students. Though much of the early research is dedicated to physical well-being. Burdette and Whitaker (2005) find no impacts of the perception of crime on children's BMI or outdoor playtime. However, the children of parents with high perception of crime are spend significantly more time watching television and are significantly more likely to watch more than two hours of television a day. There does appear to be a consistent theme in the literature, crime has negative impacts on cognitive skills in children. The duration of those impacts is less clear, however, though some evidence does point to a sustained effect. Even if the impacts of exposure to single event fade-out, persistent exposure to crime may have persistent effects.

3.3 Conceptual Framework

The general theoretical framework that motivates the empirical analyses in this study is the human capital model of the type proposed by Grossman (1972) for the study of the determinants of health outcomes. Currie and Almond (2011) use this framework for their model of human capital investment Currie and Almond adapt the Grossman model to focus on how children acquire human capital skills in response to early childhood investments. Heckman (2007) employs a similar model. To understand the impacts of early investments in children's educational attainment, a basic model of human capital accumulation can be written:

$$h = \mathrm{A}[\gamma I_1^{\phi} + (1-\gamma) I_2^{\phi}]^{1/\phi}$$

where h is human capital at the completion of childhood, I_t are human capital investments in time period $t = 1,2$ where the size of delta determines the age at which investments into human capital are the most effective. Although human capital investments can be made in many time periods, to simplify Currie and Almond assume that time period 1 refers to investments in human capital made between birth and age five of the child and time period two refers to investments made after age through the end of childhood. By including the coefficient delta, Currie and Almond allow for flexibility in the effectiveness of investments in different time periods. Delta greater than .5 implies that investments in time period 1 will be more effective than investments in time period 2. The coefficient A allows for flexibility in overall investments on human capital. If $\gamma A > 1$, then investments in human capital have a greater than one-to-one impact on human capital skills. The exponent gamma, an addition suggested by Heckman (2007) indicates the elasticity of substitution of investments between time periods 1 and 2. For gamma equal to one, there is perfect substitution between time periods.

These models are not only useful for understanding the impacts of an early childhood investment such as the CPC, but for understanding the impacts of changes in neighborhood contexts. To understand how to incorporate neighborhood shocks such as occurrences of local crime, we turn to the shocks model of Currie and Almond. Currie and Almond (2011) extend the analysis of estimating the effects of various inputs into the production of child outcomes in periods 1 and 2 to examine the impacts of exogeneous shocks, μ_g, that occur to investments in the first time period on the development of human capital skills at the end of childhood. Currie and Almond allow for positive or negative shocks, but we expect crime to be a negative shock, or at the very least, have no impact on human capital skills. In the two time period model, parents can change their investments patterns in response to the shock. In doing so, we can examine the impacts of the parental investment response. If parents decrease investment in human capital skills in response to an increase in crime, the impact of the investment response is reinforcing. If we assume a negative impact of crime on academic achievement, a reinforcing response may occur if parents divert resources away from building human capital skills in their children, instead, perhaps, invest those resources in keeping their children safe (Halpern, 1990, Scheinfeld, 1983). The impact is considered compensatory if parents instead increase investment into their children's human capital skills. With this framework in mind, we can examine the previous literature on the impacts of crime on human capital skills.

While Currie and Almond's (2011) framework typically describes childhood investments before and after age 5, to examine the impacts and responses of parents, we focus the two time period model to one school year, with t=1 representing the fall baseline and t=2 representing the end of the preschool year. The shock of neighborhood crime occurs between the measurements of these time periods. We allow for flexible responses to the shock of crime during the school year. We assume a high degree of substitutability between time periods, as the investments are incurring within one school year. As Heckman (2007) notes, with high substitutability, the optimal response to a shock is compensatory, so investment in time period 2 should increase if neighborhood crime does impact human capital skills *and* families have the means and opportunities to increase investment. If low-income families do not have the resources, capabilities or opportunities to increase investments, we may not see an increase in time period two investment. Currie and Hyson (1999) note that low-income families, like those served by the Chicago CPCs, may experience more long-term damage due to negative shocks. If the CPC program can help offset any impacts of neighborhood crime, it may be because children attending CPCs have a higher effective investment and may be less susceptible to a negative shock.

Using this framework, we can examine how the shock of crime impacts human capital skills (measured here using fall and spring teacher assessments) and if parent investment patterns change in response to crime, using parent involvement in the school to represent investment. If crime has a negative impact on human capital acquisition and parent involvement decreases in response, the change in investment would reinforce the impacts of the shock. This could occur, as Halpern (1990) notes, if parents spend less time investing in skills and focus more on the safety of their children. However, if parents increase investment in the second time period, these investments might help offset some of the negative impacts of crime.

3.4 Discussion of CPCs

As discussed previously, the CPC program has a strong focus on human capital skills, with an emphasis on literacy and mathematics. The CPC program emphasizes increased parent involvement, in home and at school, to further learning and help foster environments where human capital skills can be developed. The CPC are effective in increasing parent involvement, which in turn is associated with higher academic achievement (Hayakawa, Englund, Warner-Richter and Reynolds, 2013). The CPC model focuses on both increasing the skills of the parents (some Centers provide GED courses for parents that have not graduated high school or cooking and nutrition courses to exercise sessions focused on health) as well as teaching parenting and child education skills that focus on improving the quality of parent involvement in the home. At the beginning of the year, the primary parent involvement coordinator at each CPC site (the Parent Resource Teacher) distributes a Parent Need Assessment, where parents indicate what types of involvement events would be most beneficial to their family. Parents can indicate the relative importance of six key categories that are most important to them: Child development and parenting, health, safety and nutrition, volunteering in school, language, math and science, or career and education. Thus, parents have the opportunity to identify and reduce potential gaps in their own provision of human capital skills to their children.

3.5 Data and Sample

The sample for this study are students attending preschool in the first year of the Midwest CPC expansion program in Chicago. The total expansion enrollment sample includes 1,724 students attended preschool at a CPC, while 906 students attended preschool at a matched control site, a Chicago Public School preschool without the elements of the CPC program. Our sample consists of 1,403 students that had both fall and spring Teaching Strategies GOLD data for math, literacy, cognitive development and physical health. Administrative demographic and attendance data was collected from the district. Parent survey data on parent education, employment and single parent status was collected at the beginning of the preschool year, 62% of the sample had responses to parent survey questions. The missing data was imputed through missing value analysis using demographic and available parent survey responses.

Crime data were collected from the public records of the Chicago Police Department. All occurrences and locations of reported crimes between August 2012 and June 2013 were collected. Using GIS mapping, we identified which crimes occurred within half a mile of each preschool site, both CPC and control. We then calculate the total number of crimes experienced each student based on which site the student attended and the months an individual student was present at the site. We know which months each student was present at each school, but not individual days. Thus, each student has an individual measure of crimes experienced, though the measure will be the same for all students in a school that attended for the full school year, variation arises from variation in the total months present at the site of each student.

Previous research has investigated the impacts on parents and children when neighborhood crime is a pressing concern to the family. There are two main bodies of literature of relevance to this study. There are studies that estimate the impacts of the crime itself, either individual events or neighborhood-level, on student and families. These studies make the key assumption that a particular type of crime or the aggregated measure of crime has a direct impact on children and families. There are also studies that estimate the impacts of parents' perceptions of the neighborhood crime. While these two concepts are certainly related, many of the differences stem from the data available or the types of questions being asked. In this study, we combine parent perceptions with the number of crimes committed, so both bodies of literature are relevant. We combine these approaches by focusing on an aggregate measure of crime, but we take advantage of the results of a parent survey administered to the sample at the beginning of the preschool year. This allows us to avoid assumptions on the types of crime that matter, but also take advantage of the data on the actual crimes that occur. One of the questions in the parent survey asked parents to rate the safety of their neighborhood on a scale of 1 to 4. We correlated the types of crime with the responses to see which types of crime had the biggest relationship with parent perception of safety. If parents are making investment decisions regarding human capital and safety, then the types of crime that directly influence that perception may be more important than definitional types of crime (like violent or non-violent crime). The correlations between parent perception of safety and types of crime are presented in Table 3.1.

The parent safety measure of crime is an aggregate total of all z-scores of crimes significantly correlated with parents feeling less safe in their neighborhood (including assault, battery, burglary, criminal sexual assault, criminal damage, criminal trespassing, homicide, narcotics, offense involving children and weapons violations). Z-scores for each type of crime are used so as not to assume the impacts of a single murder are the same as the impacts of a single narcotics violation, but rather how far from the average each category of crime is. The Chicago preschool program did not provide busing and all but one school (Beasley Elementary, a magnet school) were neighborhood schools. Thus, regardless of the parent understand of “this neighborhood” either home or school, the responses should provide a good proxy for perceptions of school neighborhood safety. We also run a factor analysis on the types of crime, which maps well to the correlations with parent safety measured above, see Appendix B for full factor analysis. The only variables that did not uniquely load onto factor 1 but were correlated with the perception of parent safety, were burglary, criminal damage and weapons violation. These variables did load to factor one, but not uniquely. The only variable that loaded to factor one that was not identified by the parent perception variable was prostitution. The overlap between factors and parent perception of crime reinforce our total crime variable creation.

We then use attendance variable to determine the months that a particular student was attending a particular school. Combining this information, we create an individual measure of crime experienced based on the school site attended and the months present at that school. Thus, for each student within a school, the crime variables will be similar but not necessarily identical, depending on how long a student was present at that school. The aggregate crime variable and the responses to the parent survey variable had a negative, significant correlation of 0.195. Using the quartile measure of crime, we first compare the average number of crimes experienced, based on the types of crime identified by the parent survey, see Table 3.2. Students that attend preschool in the highest crime neighborhoods experience a dramatically higher amount of crime in the immediate vicinity of the preschool site. Of course, the students do not witness every crime committed, and may not see any, but the quartile dummy is indicative of neighborhood crime effects. Next, we compare demographic and baseline characteristics across several groups, including the full sample and the students that experience the top quartile of crime by CPC and Control. The baseline characteristics are presented Table 3.3.

Students that attended school in the highest quartile of the crime ranking are significantly more disadvantaged than students that are not. Students in the top quartile are more likely to be black, on free lunch, live in a poorer neighborhood and live with a single parent. While the high crime group is significantly more disadvantaged and experienced a higher number of crimes, neighborhood crime is not exclusive to the lower income neighborhoods. Figure 1 depicts the distribution of the sum of the z-score types of crime used to create our crime indicator by low income (bottom quartile of median household income) status. While the low-income neighborhoods had higher rates of crime, there is overlap between the two groups. Still we are sure to control for household income in our strategy to identify the impacts of neighborhood crime. Perhaps unsurprisingly, these students score significantly lower in the fall on teacher assessments. We must address these differences if we are to determine a causal impact of school neighborhood crime.

3.6 Outcome Measures:

We analyze the impacts of crime on student achievement, average attendance and parent involvement. Average daily attendance was collected administratively from the district. We also collect teacher assessment data, the Teaching Strategies GOLD (TSGOLD), a validated (Lambert, Kim and Burts, 2013a, 2013b, 2014 and Soderberg, Stull, Cummings, 2013) assessment that is administered by the teacher at least twice a year, at the beginning of the year and at the end. The students may also be assessed mid-year as well. The TSGOLD measures several domains, we focus our analysis on cognitive domains, including literacy, math, cognitive development and physical health. Students are rated on a scale of 0 to 9 on multiple items for each domain (for example, the literacy domain includes 12 items to be rated by the teacher, while math includes 7). See Table 3.4 for TSGOLD sample items and means. By necessity of our estimation strategy, we limit our sample to students that received a fall score and a spring score each domain.

Finally, we also use Parent Involvement data, collected by the teachers for each student, broken down into four categories: language, math and science events, health, nutrition and safety events, child development and parenting and career, education and personal development events. We also combine these activities for an overall Parent Involvement measure. As our outcome variable, we use the average number of activities for the months of January through June. Unfortunately, no parent involvement data was received from control sites, so the analysis of the impacts of crime on parent involvement will be limited to CPC students exclusively.

3.7 Methodology

Given the significant differences between the groups at baseline, we employ a weighting strategy that inherently balances the groups as part of the weighting process called entropy balancing (Hainmueller, 2012). Similar to inverse probability weighting, we weight particular observations of the control and treatment group to create groups that more comparable on observable characteristics. One key advantage of the entropy weighting methodology is the procedure ensures the groups are balanced across the covariates. As Austin and Stuart (2015) note, the balance of propensity score weighting is often not checked and results of unbalanced weighting should not be trusted. By ensuring our weighted sample balances, we can minimize the impacts of potential observed variable bias.

We create a weight for the likelihood of being in the top quartile of the crime variable predicted by neighborhood poverty, CPC status, race, gender, special education status, age, free lunch status, parent education, marital and employment status, an indicator of whether or not the family completed the parent survey and the fall baseline test score. We weight each control student according to Hainmueller (2012) with the following calculation:

$$w_i^* = \frac{q \exp(-\sum_{r=1}^{R} \lambda_r c_{ri}(X_i))}{\sum_{i|D=0} q_i \exp(-\sum_{r=1}^{R} \lambda_r c_{ri}(X_i))}$$

where R represents the number of balance constraints imposed on the weighting procedure, λ_r are Lagrangian multipliers that solve the loss function minimization problem to create the weights, w_i^*, and $c_{ri}(X_i)$ is the function of moments applied to the control group to balance covariates with the treatment group. Entropy balancing allows for weighting on covariates on multiple moments, including mean, variance and skewness (Hainmueller, 2012, Hainmueller and Xu, 2013). We weight according to mean for all covariates, but weight on mean and variance for fall TSGOLD scores. This helps create a control group that will have comparable growth trends so we can implement the second stage of the model, difference-in-differences.

For each TSGOLD domain, the covariates were balanced, with less than 10% standardized difference. Figure 1 shows the standardized difference of the covariates before and after the entropy weighting for the TSGOLD literature model. Balance models were estimated for each TSGOLD domain and attendance and each model balanced similarly.

While the entropy weighting approach can help minimize bias that arises from observed characteristics, it cannot address differences in unobserved characteristics. To help address those issues, we employ a difference-in-difference (DD) approach, as well as a triple difference approach (DDD). Difference-in-difference approaches can eliminate bias that arises from differences in treatment and control groups that are time-invariant (Wooldridge, 2002). The key assumption required for DD modelling is that outside of the intervention (in this case, high crime), the two groups would have similar growth patterns. This does not seem likely for the two groups, with significant differences in baseline score, marital status and race among others. We employ a difference-in-difference model that is weighted by the estimated entropy balancing weights in equation 1, a method that has been applied to examine the economic impact of graduating with honors (Freier, Schumann and Siedler, 2015). We begin by estimating the following equation, separately for CPC and control students:

$$y = \beta_0 + \beta_1 Crime_i + \beta_2 Spring_i + \beta_3 Crime_i * Spring_i + \beta_i X_i + \mu_i$$

where β_1 represents differences between the students in high and low crime neighborhoods prior to the school year. β_2, a dummy indicating the assessment at the end of the year, captures any growth in TSGOLD scores that would occur regardless of exposure to crime. We expect this term to be positive as most students tend to score higher in the spring assessment of the TSGOLD. A collection of control variables, including demographic characteristics of the student, parent, a measure the third-grade achievement and neighborhood income are included in the model in X_i. The difference-in-difference estimator is β_3, the interaction term between our crime variable and the time variable. This coefficient captures the change in TSGOLD scores in response to the change in attending a high crime preschool site, or:

$$\beta_3 = \left(\hat{y}_{Crime=1,Spring} - \hat{y}_{Crime=1,Fall}\right) - \left(\hat{y}_{Crime=0,Spring} - \hat{y}_{Crime=0,Spring}\right)$$

which gives the causal estimate of the impacts of high neighborhood crime in the immediate vicinity of the preschool site, for both CPC and control students separately. By combining the entropy balance and the difference-in-difference approaches, our goal is to reduce any bias that arises from time-invariant omitted variables and reduce the bias that arises from differences in the distributions of the covariates between the treatment and control groups (Heckman, Ichimura and Todd, 1997, Abadie, 2005).

We also want to test the difference in the estimates for CPC and control students. We can do this using a triple difference (DDD) model, again weighted by the entropy balance weights outline above. While we could subtract the estimates from the control group from the estimates of the CPC group, we estimate the model to test for significant differences between the group. This will determine if CPC is able to offset any negative impacts of crime on achievement or attendance. We estimate the following model:

$$y = \beta_0 + \beta_1 Crime_i + \beta_2 Spring_i + \beta_3 Control_i + \beta_4 Crime_i * Spring_i + \beta_5 Crime_i * Control_i + \beta_6 Control_i * Spring_i + \beta_7 Crime_i * Spring_i * Control_i + \beta_i X_i + \mu_i$$

where again, the coefficient of interest is the interaction term between the three key indicators, crime, time and CPC treatment status, β_7. This tells us if there are any significant differences of the impacts of crime between CPC and non-CPC students:

$$\beta_7 = \left(\hat{y}_{Crime=1, Spring, Control} - \hat{y}_{Crime=1, Fall, Control}\right) - \left(\hat{y}_{Crime=0, Spring, Control} - \hat{y}_{Crime=0, Spring, Control}\right) - \left(\hat{y}_{Crime=1, Spring, CPC} - \hat{y}_{Crime=1, Fall, CPC}\right)$$

We address key differences in the growth trends first by combining the entropy weighting approach described above and by adding the DDD model, which allows us to test for significant differences in the impacts of crime by CPC status, allowing us to test the question if the CPC program can help offset any potential negative impacts of crime. We cluster the standard errors at the school level to help account for school-level differences.

3.8 Results

We begin by estimating the weighted difference-in-difference models separately for CPC students and non-CPC students. For the control students, the students that attended a non-CPC public preschool, either Head Start or district-run, we find significant, negative impacts of crime on TSGOLD literacy, math and cognitive scores, though no significant impacts on physical well-being or attendance rates. Though students at control sites did receive formal prekindergarten, the services provided were limited compared to those at the CPC sites and represent the impacts of crime on a student receiving the standard preschool education. The full results are presented in Table 3.5. Students that had the highest number of crimes in the immediate vicinity to their preschool scored 12 points lower on literature and cognitive development teacher assessments and 5 points lower on math assessments. There were no significant differences on the physical health assessment or attendance rates. It appears that for the control students, neighborhood crime functions as a negative shock in the Currie and Almond (2011) human capital model.

However, when we estimate the model separately for CPC students, we do not see any significant impacts of crime for students on TSGOLD assessments or attendance. Results are presented in Table 3.6. These results indicate that CPC students are less susceptible to the impacts of high neighborhood crime that the control students. Perhaps this is due, in part, to the fact that CPC students outperform the control students, so they have a higher initial I_1, which as Currie and Almond note, may make CPC students less impacted by negative shocks.

Another possible theoretical explanation arises from the concept of reactive investments. If parents identify the negative impacts of the neighborhood crime, but are able to increase investment in the second time period, they may be able to offset the negative impacts of high neighborhood crime. To test this, we examine the impacts of neighborhood crime on parent involvement amongst CPC families. We use the same weighted difference-in-differences approach to estimate the impacts of high neighborhood crime on attendance of various types of parent involvement activities offered in the CPC sites. CPC families completed a needs assessment, where parents indicated the types of activities that would be most beneficial to them. This allowed the CPC sites to tailor parent involvement activities to the needs of their families. To test this hypothesis, we proceed again with the entropy balancing, difference-in-difference approach, this time applied to parent involvement. The sample was limited to all CPC students with valid parent involvement data during the year. The results of this estimation are presented in Table 3.8. CPC families in high crime neighborhoods attend significantly more health, safety, and nutrition events that CPC families in all other neighborhoods, though there is no significant impact on other types of parent involvement, including language, math or science activities. With a limited sample size, we estimate the impacts of high neighborhood crime on parent involvement only on the families that had valid observations for each type of activity. Results are presented in Table 3.9. With the caveat of small sample size, this estimation strategy makes no assumption that all types of activities were offered when the family was present in the school. By using this strategy, we know that all included observations attended at least one of the type of activity analyzed. Amongst that sample, families that attend CPCs in high crime neighborhoods see no significant difference in in school activities, child development activities, or career or education activities. However, they do attend significantly more health, safety or nutrition events and language, math and science events. The specific types of activities that occur in the language, math and science and health, safety and nutrition tend to be more concrete activities that parents can participate with their children later on at home. For example, learning a new recipe to cook with their children or worksheets or activities that focus on math learning at home. If a parent was concerned about the impacts of crime, these types activities may help offset those impacts and increased attendance in those represent a reactive, compensatory investment in time period two. This may help explain why we see no significant differences between the performance of high and low neighborhood crime within CPC students.

Finally, we run a triple difference (DDD) estimate to test if there is a significant difference of the impacts of crime between CPC and non-CPC students. We find a significant, negative difference for literacy, math and cognitive development scores. The difference between the DD models, when estimated jointly, is significant, indicating that CPC preschool may help offset the negative cognitive impacts of attending preschool in a high crime neighborhood. These results indicate that high neighborhood crime has negative impacts on the human capital skills of preschoolers, unless the preschool program provides opportunities for families to increase their investment in their children's human capital skills.

3.9 Robustness Testing:

We begin our robustness testing by estimating the difference-in-difference model without the entropy weights. We find no differences from our previous estimation, see Table 3.10. CPC students have no significant impacts, while control students see significant, negative impacts in literature, math and cognitive development test scores. There is no significant difference in physical health or attendance rates for the control group. Like the results in Table 3.6, the triple difference estimate is significant for the three domains as well. These results are consistent with the weighted results presented earlier. Appendix B presents results by various weighting estimations. All models tested were consistent. We also examine the impacts of high neighborhood crime defined as above the mean on the z-score distribution of the parent crime variable. To balance the model, we used percent of the neighborhood in poverty, rather than median household income. The entropy weights would not balance using median neighborhood household income. Results are presented in Table 3.11. Results remained relatively the same for the CPC students, except for a positive, significant impact on physical development. However, while the pattern of results was similar for control students (negative for literature, math and cognitive development) the results are no longer significant. This indicates that perhaps only the highest amounts of neighborhood crime may have negative impacts on human capital skills.

Finally, in accordance with the parent perspective literature, we test the impacts of feeling unsafe in the neighborhood. Here we do not use the actual crime data, simply the responses on the parent survey. We use a difference-in-difference approach to test for the difference between those answering that they disagree most strongly with the statement: this neighborhood is a safe place for me and my children. While the sample size is much smaller, we do see negative impacts of feeling unsafe the control group (though in this estimate it is for physical health and attendance), again these are offset for CPC students. Full results presented in Table 3.12.

3.10 Conclusion

Using the Currie and Almond (2011) human capital model as a framework, we are able to determine that high crime in the immediate vicinity of a preschool program serves a negative shock to human capital skills, measured here by year end teacher assessments. The results here are consistent with impacts found by previous studies (Gershenson and Tekin, 2017, Aliprantis and Chen, 2016) in that neighborhood crime can have an impact on student achievement, even if they did not personally witness the crimes or their aftermath. However, a high quality early education program, the Chicago CPC, with a focus on menu-based parent involvement may help parents react accordingly to the negative shock of crime. By allowing parents agency in the types of involvement opportunities offered, parents can identify which involvement opportunities will be most beneficial for their children. This allows parents to change their human capital investments accordingly to help offset the shocks caused by neighborhood crime. This analysis represents the first analysis to investigate the relationship between early education and crime, to the best of our knowledge.

While we examined the impacts on school readiness, we are unable to assess the impacts of crime on other well-being, including mental health. It seems possible, if not likely, that while CPC programs in high crime neighborhoods may help offset the negative impacts of crime academically, those same CPC programs may have to shift limited resources away from other programs or activities whose benefits do not manifest in kindergarten school readiness. Further examination of outside impacts is required. Similarly, some preschool programs see a fadeout in academic achievement by later years but still see positive gains in other adult outcomes (Currie and Thomas, 1993, Garces, Thomas and Currie, 2002, Reynolds et al, 2011), we are unable to analyze the impacts of sustained exposure to crime, though as subsequent data is gathered, this becomes more feasible.

A continued analysis of the impacts of multiple years of exposure to high neighborhood crime compared to multiple years of program comparison would help identify the impacts of crime more robustly and serves as the main future direction of this study. As subsequent test score data becomes available, we will able to determine if dosage effects of both CPC participation and exposure to crime have compounding impacts. The limited time frame serves as one key limitation to this study. Several previous studies (Sharkey 2010, 2012) have identified that the impacts of crime may fade out after several years. There are also significant group differences between the groups exposed to the highest number of crimes. While we control for neighborhood income in both our matching model and our outcome model, crime is certainly associated with neighborhood quality. By combining matching strategies and differences-in-differences approaches, our methodology minimizes, but does not eliminate, concerns that we are capturing the impacts of neighborhood quality rather than crime. However, given the groups are matched and unchanging neighborhood quality is differenced out, those concerns are small. Finally, we may be understating the true impacts of exposure to crime, as all the students analyzed experienced some level of crime in the immediate vicinity of the school.

There are important implications of this work. First, previous studies have studied the costs of crime in Chicago (Ander, Cook, Ludwig and Pollack, 2009) and the results are staggering. However, it is possible that with an impact on school readiness, the costs of crime are understated. This provides an additional argument for implementing policies intent on the reduction of crime. This study also highlights the importance of high-quality early education and offering menu-based involvement approaches that helps parents counter act negative shocks by providing opportunities for investment in different subject areas. We believe this approach could be applied to combat the negative impacts of external factors beyond neighborhood crime.

Table 3.1: Correlations between aggregate crime variable and parent survey responses to opinions on neighborhood safety

Assault[1]	-0.1791*	Narcotics[1]	-0.1319*
Battery[1]	-0.1885*	Offense involving children[1]	-0.1717*
Burglary	-0.0908*	Other narcotics	0.0322
Criminal sexual assault[1]	-0.1572*	Prostitution[1]	0.0361
Criminal damage	-0.0986*	Robbery[1]	-0.1847*
Criminal trespass[1]	-0.0819*	Sex offense	-0.0367
Homicide[1]	-0.1142*	theft	-0.0294
Kidnapping	-0.0027	Weapons violation	-0.2109*
Motor vehicle theft	-0.024		

*denotes significant correlation, 1 denotes unique factor 1 loading.

Table 3.2: Differences in average crime by type, by quartile

	Top Quartile (n=309)	All Other (n=1063)	p-value	CPC only p-value	Control only p-value
Assault	146.3	61.6	0.000	0.000	0.000
Battery	519.7	178.6	0.000	0.000	0.000
Burglary	113.4	52.3	0.000	0.000	0.000
Criminal Damage	42.5	11.3	0.000	0.000	0.000
Criminal Sexual Assault	8.6	3.3	0.000	0.000	N/A
Criminal Trespass	37.9	25.0	0.000	0.000	0.000
Homicide	3.5	1.8	0.000	0.000	0.000
Narcotics	596.9	79.1	0.000	0.000	0.000
Offense involving children	15.4	5.6	0.000	0.000	0.000
Robbery	109.7	41.7	0.000	0.000	0.000
Weapons Violation	42.5	11.3	0.000	0.000	0.000

Table 3.3: Baseline Characteristics by crime level and CPC treatment status, 2012-2013 school year

	Top Quartile (n=309)	All Other (n=1063)	p-value	CPC only p-value (n=946)	Control Only p-value (n=426)
CPC	79.6	65.9	0.000	N/A	N/A
Neighborhood Median Household Income	37,540.97	43,822.41	0.000	0.000	0.0000
Black	95.2	49.1	0.000	0.000	0.0000
Hispanic	5.5	49.6	0.000	0.0000	0.0000
Female	51.5	53.0	0.641	0.3548	0.4574
Special Education	2.9	8.4	0.001	0.0001	0.8901
Age in Months	48.4	49.2	0.043	0.2744	0.0807
Eligible for Free or Reduced Lunch	98.1	87.3	0.000	0.0000	0.1422
Mom HS Grad	83.5	79.5	0.1189	0.0922	0.7337
Mom Employed	76.7	81.8	0.044	0.2282	0.4544
Single Parent	71.8	45.2	0.000	0.0000	0.0000
Fall Lit TSGOLD score	33.1	35.5	0.039	0.0000	0.0000
Fall Math TSGOLD score	23.3	23.8	0.391	0.0076	0.0000

***Table** 3.4: TSGOLD subscale sample items and means*

Domain	Sample Items*	Fall Mean (SD) Spring Mean (SD)	Percent at/above National Norm
Literacy 12 items	Identifies and names letters Uses and appreciates books	33.3 (15.9) 57.2 (17.7)	10.5% 71.5%
Math 7 items	Counts Quantifies	23.0 (8.9) 36.3 (9.5)	8.5% 69.4%
Science 5 items	Uses scientific inquiry skills Uses tools and other technology to perform tasks	4.5 (2.4) 7.7 (2.4)	N/A N/A
Physical 5 items	Demonstrates balancing skills Demonstrates fine-motor strength and coordination	26.0 (6.7) 33.8 (61)	54.8 69.6

*Items are rated on a scale of 0 to 9 (Science 0 to 2). Scores at or above the national average in spring are as follows for 3- and 4-year-olds: Literacy (39, 56), Math (27, 37), Socioemotional (46, 57). Meeting the national norm in total score was defined as meeting the norm in at least 3 domains in the fall and 4 in the spring.

Table 3.5: Weighted Difference-in-Difference Impacts of High Neighborhood Crime for Control Students Only, TSGOLD scores

VARIABLES	(1) Literature	(2) Math	(3) Cognitive	(4) Physical	(5) Attendance

			Development	Health	
Crime Dummy	-36.74**	-8.206	-2.581	-2.564	0.0830***
	(13.00)	(8.807)	(1.601)	(2.455)	(0.0230)
Spring Time Dummy	20.47***	11.20***	14.14***	7.530***	-0.0275
	(0.793)	(0.649)	(1.298)	(0.759)	(0.0196)
DD Estimate	-12.04***	-5.295***	-12.43***	-0.673	-0.0480
	(0.793)	(0.649)	(1.298)	(3.983)	(0.0510)
Neighborhood Household Median Income	-0.00141**	-0.000405	-9.39e-05**	-8.22e-05**	-1.90e-07
	(0.000389)	(0.000257)	(3.43e-05)	(3.17e-05)	(7.86e-07)
Months Present	9.088***	3.118***	1.024	0.0649	0.0359
	(1.338)	(0.500)	(1.925)	(0.512)	(0.0296)
School level achievement	-1.406**	-0.393	-0.292***	-0.110***	0.00127
	(0.413)	(0.290)	(0.0271)	(0.0175)	(0.000864)
Fall Assessment Date	-5.511***	-2.537*	-3.216***	-2.399***	0.0754**
	(1.046)	(1.052)	(0.658)	(0.657)	(0.0276)
Black	15.21**	8.608	10.14*	3.831**	-0.0220
	(4.910)	(4.533)	(5.163)	(1.114)	(0.0170)
Hispanic	14.26***	9.241*	8.996*	3.678***	0.0260
	(2.965)	(3.599)	(3.937)	(0.664)	(0.0223)
Female	3.356***	1.800***	1.849	0.594	0.0210
	(0.754)	(0.313)	(1.279)	(0.355)	(0.0251)
Special Education Status	-21.26***	-9.683***	-13.14***	-5.084***	0.0102
	(3.544)	(1.548)	(1.624)	(0.659)	(0.00958)
Age in Months	1.622***	0.852***	1.005***	0.451***	-0.00169*
	(0.0342)	(0.0610)	(0.106)	(0.0383)	(0.000849)
Free lunch eligible	2.382	-1.445***	0.186	-1.954**	-0.0271
	(2.676)	(0.294)	(1.260)	(0.569)	(0.0294)
Mother HS Grad	4.742***	1.579	1.949**	0.363	0.0248
	(1.170)	(0.911)	(0.729)	(0.847)	(0.0172)
Mother Employed	-0.215	-1.073	-1.077	-0.504	-0.0104
	(4.432)	(1.945)	(1.984)	(0.696)	(0.00899)
Missing Parent Survey	-0.844	-0.540	-0.103	0.494	-0.0468*
	(1.610)	(0.297)	(0.447)	(0.493)	(0.0202)
Single Parent Status	1.018	0.480	-0.995	-0.193	-0.000277
	(1.882)	(1.497)	(1.352)	(0.216)	(0.00512)
Constant	-5.183	-13.77	-2.597	13.16***	0.522
	(39.50)	(32.34)	(16.73)	(3.725)	(0.299)
Observations	349	372	381	426	422
R-squared	0.713	0.721	0.713	0.657	0.226

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey. Standard errors are clustered at the school level.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Weighted Difference-in-Difference Impacts of High Neighborhood Crime for CPC students only, TSGOLD scores

	(1)	(2)	(3)	(4)	(5)

VARIABLES	Literature	Math	Cognitive Development	Physical Health	Attendance
Crime Dummy	-1.992	-0.589	-1.558	-0.153	0.00154
	(2.269)	(1.205)	(1.998)	(1.011)	(0.0106)
Spring Time Dummy	26.43***	15.30***	17.61***	9.122***	-0.104***
	(2.423)	(1.010)	(1.196)	(0.745)	(0.00981)
DD Estimate	2.506	0.0438	1.158	-0.789	-0.00541
	(3.632)	(1.565)	(1.707)	(0.863)	(0.0192)
Neighborhood Household	-2.07e-05	-2.55e-05	-3.37e-06	2.92e-05	4.49e-08
Median Income	(9.10e-05)	(3.94e-05)	(5.20e-05)	(6.49e-05)	(2.94e-07)
Full-Day Class	-1.237	1.078	0.636	0.119	0.0608***
	(3.718)	(1.877)	(2.055)	(1.449)	(0.0106)
Months Present	0.0778	-0.492	2.619**	0.809**	0.000882
	(1.744)	(0.787)	(1.150)	(0.325)	(0.00740)
School level achievement	-0.108*	-0.0400	-0.0257	0.0195	0.00108***
	(0.0584)	(0.0299)	(0.0620)	(0.0318)	(0.000190)
Fall Assessment Date	7.083*	3.794**	6.945***	3.136***	0.0128
	(3.437)	(1.323)	(1.502)	(0.973)	(0.0120)
Black	7.261	1.303	2.653	-0.0648	-0.0627**
	(4.371)	(1.961)	(3.670)	(2.331)	(0.0263)
Hispanic	6.946	4.911*	5.483	1.364	-0.0390
	(5.159)	(2.357)	(3.372)	(1.992)	(0.0377)
Female	2.340*	1.103**	1.007*	0.446**	0.00134
	(1.224)	(0.462)	(0.528)	(0.167)	(0.00553)
Special Education Status	-7.498	-4.226	-6.966**	-3.516**	0.00890
	(4.594)	(2.471)	(3.020)	(1.483)	(0.0299)
Age in Months	1.534***	0.746***	0.986***	0.464***	0.000363
	(0.0906)	(0.0881)	(0.106)	(0.0612)	(0.000781)
Free lunch eligible	-6.107**	-2.917***	1.731	1.626	0.00269
	(2.034)	(0.749)	(1.563)	(1.187)	(0.0331)
Mother HS Grad	3.815	1.728	1.201	0.721	0.0360**
	(2.249)	(1.383)	(1.346)	(0.546)	(0.0160)
Mother Employed	-1.882	-1.318*	-2.452*	-1.030*	-0.0128*
	(1.510)	(0.628)	(1.344)	(0.493)	(0.00654)
Missing Parent Survey	-0.0865	0.107	0.691	0.486	-0.0281**
	(1.883)	(0.860)	(1.420)	(0.665)	(0.0109)
Single Parent Status	1.702	0.823	2.114	0.507	0.0108
	(1.479)	(0.639)	(1.315)	(0.509)	(0.0102)
Constant	-44.11*	-7.279	-42.82**	-11.19*	0.845***
	(24.55)	(10.25)	(15.00)	(5.752)	(0.129)
Observations	780	792	829	941	943
R-squared	0.648	0.643	0.620	0.596	0.203

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey. Standard errors are clustered at the school level.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Weighted Difference-in-Difference Impacts of High Neighborhood Crime for CPC students only on Parent Involvement

VARIABLES	(1) Language, Math or Science Activity	(2) Health, Safety or Nutrition	(3) ACTM	(4) Child Development Activity	(5) Career and Education Programs
Crime Dummy	-0.948	-0.671***	-0.632	-0.390	-0.498**
	(0.749)	(0.212)	(0.995)	(0.462)	(0.192)
Spring Time Dummy	-0.682	-0.722***	-0.879*	-0.389	-0.146
	(0.639)	(0.205)	(0.440)	(0.499)	(0.170)
DD Estimate	0.704	0.655***	0.337	0.357	0.279
	(0.656)	(0.207)	(0.790)	(0.505)	(0.272)
Neighborhood Household	-1.06e-05	7.91e-06*	0.000106*	2.54e-05**	1.56e-05*
Median Income	(1.03e-05)	(4.37e-06)	(5.19e-05)	(9.02e-06)	(7.58e-06)
Full-Day Class	-0.0588	-0.0266	1.390***	0.199**	-0.0151
	(0.0630)	(0.0639)	(0.440)	(0.0704)	(0.0784)
Months Present	-0.150	0.0523	1.244**	0.0497	-0.0990
	(0.121)	(0.0422)	(0.547)	(0.0504)	(0.0981)
School level achievement	-0.0126	-0.00179	0.0334	-0.00218	-0.0124
	(0.00742)	(0.00181)	(0.0193)	(0.00192)	(0.00925)
Fall Assessment Date	-0.474*	-0.239**	-0.961	-0.270	-0.626**
	(0.243)	(0.0927)	(0.664)	(0.427)	(0.248)
Black	0.0559	-0.281**	-1.040*	-0.292	-0.548***
	(0.182)	(0.120)	(0.514)	(0.373)	(0.174)
Hispanic	0.00303	-0.00664	0.155	0.0213	-0.0579
	(0.0410)	(0.0518)	(0.174)	(0.0444)	(0.0936)
Female	0.0362	-0.0583	-0.270	0.0109	0.111
	(0.151)	(0.0847)	(0.453)	(0.0536)	(0.247)
Special Education Status	-0.00947	-0.000194	0.00878	-0.00470*	-0.0235
	(0.00697)	(0.00376)	(0.0128)	(0.00227)	(0.0195)
Age in Months	-0.102	0.0697	0.453	0.0202	-0.141
	(0.0848)	(0.0845)	(0.619)	(0.0440)	(0.0806)
Single Parent Status	0.0661	0.100	0.418	0.0135	0.143
	(0.0456)	(0.0711)	(0.305)	(0.0516)	(0.0876)
Mother Employment Status	0.110*	-0.0502	0.804**	0.0620	0.0718
	(0.0565)	(0.119)	(0.363)	(0.0476)	(0.0697)
Missing Parent Survey	-0.154	-0.0612	-1.056*	-0.0222	-0.208
	(0.0897)	(0.0784)	(0.528)	(0.0704)	(0.131)
Mom HS Grad	0.0795	0.0463	0.995***	0.0885	0.217*
	(0.0572)	(0.0485)	(0.198)	(0.0778)	(0.123)
Constant	4.549	0.229	-18.00**	-0.543	3.350
	(2.885)	(0.415)	(6.416)	(0.790)	(2.651)
Observations	564	564	564	564	564
R-squared	0.199	0.125	0.199	0.198	0.107

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey. Standard errors are clustered at the school level.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Weighted Difference-in-Difference Impacts of High Neighborhood Crime for CPC students only on Parent Involvement

VARIABLES	(1) Language, Math or Science Activity	(2) Health, Safety or Nutrition	(3) In-School Activity	(4) Child Development Activity	(5) Career and Education Programs
Crime Dummy	-2.464***	-1.514***	-0.173	-1.390	-0.0480
	(0.277)	(0.384)	(0.930)	(0.886)	(0.202)
Spring Time Dummy	-2.219***	-1.531***	-0.846*	-1.745	0.437
	(0.233)	(0.430)	(0.430)	(1.002)	(0.496)
DD Estimate	1.867***	1.235**	0.290	1.234	-0.402
	(0.269)	(0.454)	(0.814)	(0.979)	(0.480)
Neighborhood Household Median Income	-5.92e-06	-8.30e-06	0.000110*	3.20e-05***	3.25e-06
	(1.15e-05)	(7.01e-06)	(5.15e-05)	(7.73e-06)	(1.68e-05)
Full-Day Class	-0.372*	-0.334**	1.448***	-0.0221	-0.0626
	(0.180)	(0.137)	(0.402)	(0.0974)	(0.202)
Months Present	-0.00906	-0.178	1.001*	0.142***	-0.539
	(0.122)	(0.180)	(0.486)	(0.0479)	(0.399)
School level achievement	-0.00882	-0.00641	0.0385*	-0.00855*	-0.0263
	(0.00567)	(0.00422)	(0.0190)	(0.00437)	(0.0157)
Fall Assessment Date	-0.595*	-0.297**	-1.620**	-1.654***	-0.819**
	(0.327)	(0.112)	(0.622)	(0.227)	(0.373)
Black	0.0253	-0.409*	-1.013	-0.531**	-0.157
	(0.251)	(0.199)	(0.619)	(0.201)	(0.354)
Hispanic	-0.0890	-0.0308	0.195	0.107	-0.207
	(0.146)	(0.105)	(0.168)	(0.114)	(0.199)
Female	-0.159	-0.192	-0.462	0.0907	0.0578
	(0.172)	(0.321)	(0.376)	(0.104)	(0.662)
Special Education Status	-0.0336	0.00281	0.0110	-0.00436	-0.0661
	(0.0204)	(0.00595)	(0.0125)	(0.00602)	(0.0496)
Age in Months	0.0358	0.321	0.538	0.291	-0.158
	(0.0897)	(0.228)	(0.691)	(0.322)	(0.204)
Single Parent Status	0.276	0.229*	0.355	-0.0947	0.409**
	(0.167)	(0.110)	(0.299)	(0.161)	(0.152)
Mother Employment Status	-0.0780	-0.313	0.877**	0.0628	0.210
	(0.129)	(0.226)	(0.360)	(0.143)	(0.173)
Missing Parent Survey	-0.125	-0.0611	-1.127*	0.156	-0.214
	(0.135)	(0.125)	(0.535)	(0.0912)	(0.124)
Mom HS Grad	0.180**	0.0332	0.994***	0.0856	0.638**
	(0.0704)	(0.0991)	(0.188)	(0.106)	(0.237)
Constant	5.927**	4.846**	-15.55**	1.350	10.97
	(2.554)	(2.255)	(6.187)	(1.299)	(7.432)
Observations	237	176	519	185	225
R-squared	0.437	0.368	0.206	0.526	0.198

Difference-in-difference model controls for neighborhood income, full-class room, months present in site,

school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother’s employment, education and marital status and a measure of incomplete parent survey.
Standard errors are clustered at the school level.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Weighted Difference-in-Difference-in-Differences estimates of the impacts of high neighborhood crime

VARIABLES	(1) Literature	(2) Math	(3) Cognitive Development	(4) Physical Health	(5) Attendance
Crime Dummy	-1.787	-0.523	-1.510	-0.0417	-0.00220
	(2.194)	(1.163)	(1.931)	(0.988)	(0.0130)
Spring Dummy	26.43***	15.30***	17.61***	9.122***	-0.103***
	(2.394)	(0.997)	(1.180)	(0.737)	(0.00955)
Crime*Spring	2.506	0.0438	1.158	-0.789	-0.00552
	(3.587)	(1.546)	(1.684)	(0.853)	(0.0190)
Control Dummy	1.125	2.313*	5.341***	1.319	-0.0248
	(2.775)	(1.182)	(1.436)	(1.207)	(0.0158)
Crime*Control	13.08***	6.669***	6.998***	1.092	0.0873***
	(2.692)	(1.408)	(2.122)	(2.760)	(0.0188)
Control*Spring	-5.959**	-4.100***	-3.469*	-1.592	0.0749***
	(2.505)	(1.166)	(1.700)	(1.031)	(0.0205)
DDD estimate	-14.55***	-5.338***	-13.58***	0.116	-0.0416
	(3.662)	(1.660)	(2.081)	(3.878)	(0.0518)
Neighborhood Household Median Income	-4.79e-05	-2.89e-05	5.84e-06	2.72e-05	-9.39e-08
	(6.00e-05)	(2.49e-05)	(4.15e-05)	(4.01e-05)	(3.54e-07)
Full-Day Class	-1.203	1.043	0.618	0.0921	0.0604***
	(3.691)	(1.813)	(2.016)	(1.471)	(0.0131)
Months Present	-0.167	-0.557	2.509**	0.566	0.00832
	(1.379)	(0.664)	(1.116)	(0.355)	(0.00758)
School level achievement	-0.0923	-0.0351	-0.0274	0.0283	0.000685**
	(0.0551)	(0.0277)	(0.0561)	(0.0252)	(0.000258)
Fall Assessment Date	6.495**	3.588***	6.777***	2.583***	0.0311**
	(3.062)	(1.188)	(1.315)	(0.810)	(0.0115)
Black	8.594**	2.878	4.746	1.503	-0.0811***
	(4.098)	(2.083)	(3.177)	(1.806)	(0.0241)
Hispanic	6.882*	4.731**	4.095	0.641	-0.0163
	(3.948)	(1.806)	(2.875)	(1.512)	(0.0315)
Female	2.587**	1.209**	1.162**	0.510***	0.00432
	(1.087)	(0.425)	(0.498)	(0.149)	(0.00646)
Special Education Status	-9.722**	-4.991**	-7.788**	-4.146***	0.0126
	(4.332)	(2.208)	(2.814)	(0.982)	(0.0176)
Age in Months	1.546***	0.758***	0.990***	0.475***	-0.000123
	(0.0792)	(0.0771)	(0.0949)	(0.0538)	(0.000611)
Free lunch eligible	-3.493	-2.550***	1.516	0.265	-0.00302
	(2.724)	(0.718)	(1.420)	(1.108)	(0.0218)
Mother HS Grad	4.104**	1.776	1.492	0.737	0.0350**
	(1.945)	(1.201)	(1.212)	(0.490)	(0.0130)
Mother Employed	-1.437	-1.195*	-2.216*	-0.914**	-0.0120**
	(1.447)	(0.607)	(1.200)	(0.403)	(0.00553)
Missing Parent Survey	-0.294	0.0101	0.495	0.350	-0.0329***
	(1.694)	(0.742)	(1.223)	(0.534)	(0.0100)

Single Parent Status	1.573	0.767	1.857	0.432	0.00798
	(1.313)	(0.583)	(1.193)	(0.418)	(0.00869)
Constant	-46.15**	-9.261	-44.14***	-9.544*	0.834***
	(19.48)	(8.536)	(14.47)	(4.776)	(0.114)
Observations	1,129	1,164	1,210	1,367	1,354
R-squared	0.649	0.645	0.621	0.596	0.204

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey.
Standard errors are clustered at the school level.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Unweighted estimates, CPC Difference-in-Differences, Control Difference-in-Differences, Triple Difference

VARIABLES	(1) Literature	(2) Math	(3) Cognitive Development	(4) Physical Health	(5) Attendance
CPC Difference in Difference	3.590	0.489	1.671	-0.700	-0.0164
	(3.174)	(1.489)	(1.472)	(0.706)	(0.0189)
Control Difference in Difference	-13.55***	-5.502***	-10.55***	0.0757	-0.0243
	(0.510)	(0.452)	(0.901)	(3.946)	(0.0495)
DDD	-17.14***	-5.991***	-12.22***	0.776	-0.00752
	(3.171)	(1.530)	(1.682)	(3.812)	(0.0509)
Observations	1,129	1,164	1,210	1,367	1,354

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey. Standard errors are clustered at the school level.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table *3.11: Weighted estimates, CPC Difference-in-Differences, Control Difference-in-Differences, Triple Difference, Top Two quartiles of Crime variable*

VARIABLES	(1) lit	(2) math	(3) cog	(4) phys	(5) attendance
CPC Difference in Difference	-3.308	0.272	-0.280	2.300***	0.00419
	(2.582)	(1.168)	(1.694)	(0.773)	(0.0135)
Control Difference in Difference	-7.793	-2.880	-3.274	0.628	-0.0576
	(5.909)	(2.659)	(4.298)	(1.898)	(0.0330)
DDD	1.711	0.272	-0.280	2.300**	0.00450
	(3.091)	(1.182)	(1.718)	(0.782)	(0.0136)
Observations	1,129	1,164	1,210	1,367	1,354

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey. Standard errors are clustered at the school level.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.12: Weighted Difference-in-Difference Impacts of Crime, using Parent Survey Results indicating unsafe neighborhoods

VARIABLES	(1) Literature	(2) Math	(3) Cognitive Devel.	(4) Physical Health	(5) Attendance
CPC Difference in Difference	1.487	0.264	0.101	-0.301	-0.00505
(n=557 – 1,088)	(2.433)	(0.372)	(1.032)	(0.561)	(0.0190)
Control Difference in Difference	-1.572	1.166	-1.121	-3.547*	-0.0576**
(n= 126 – 273)	(1.813)	(2.051)	(1.469)	(1.585)	(0.0250)
DDD	-3.059	0.902	-1.222	-3.246*	-0.0508
	(2.922)	(1.915)	(1.703)	(1.587)	(0.0319)

Difference-in-difference model controls for neighborhood income, full-class room, months present in site, school level reading achievement, fall assessment date, race, gender, special education, age, free lunch eligibility, mother's employment, education and marital status and a measure of incomplete parent survey.
Standard errors are clustered at the school level.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 3.1: Distribution of crime types by neighborhood income level.

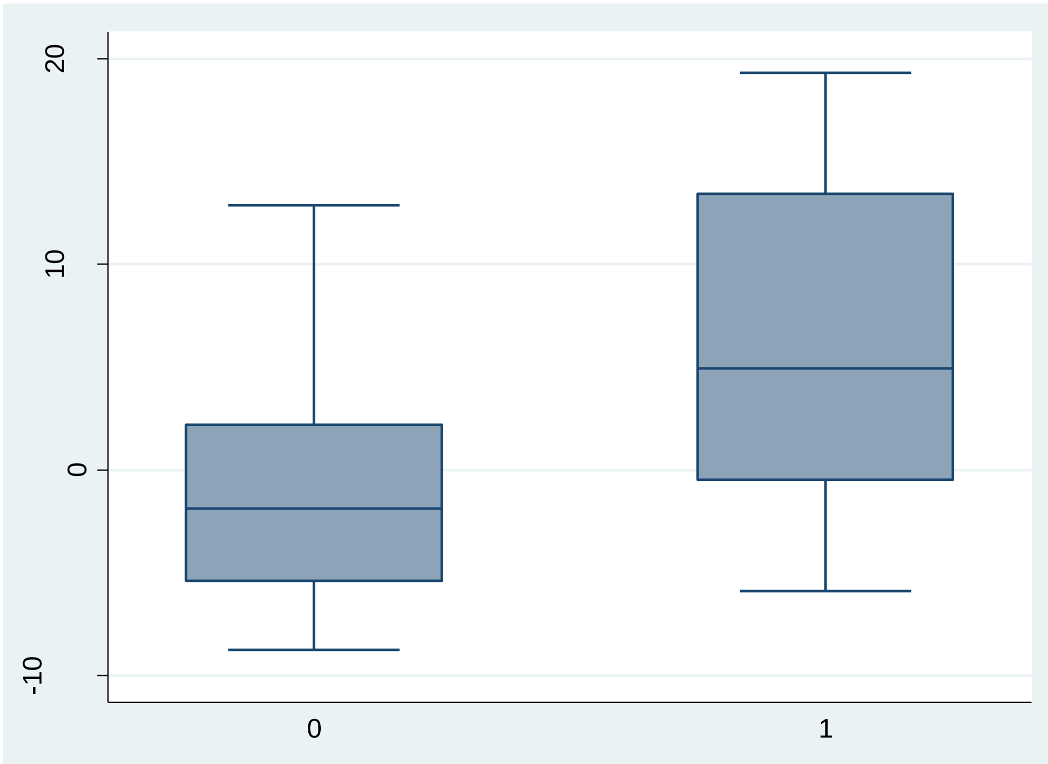
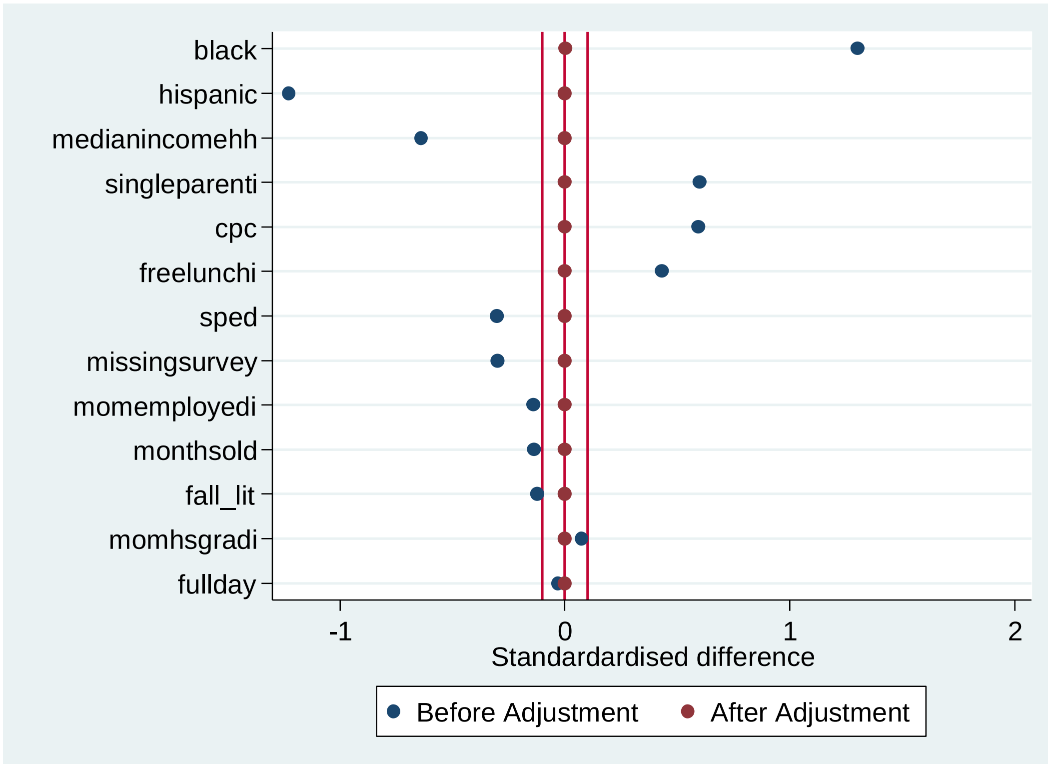


Figure 3.2: Standardized Differences of Baseline Characteristics Before and After Weighting Procedures



CHAPTER 4: ESTIMATING THE IMPACTS OF CPC PARTICIPATION ON HIGH SCHOOL CHOICE

4.1 Introduction:

This research investigates the impact of a high quality early education intervention for children and parents on subsequent schooling decisions. In this study, we focus specifically on whether students who lived in high-poverty neighborhoods and participated in an educational program called the Child-Parent Centers beginning in preschool later attend better quality high schools. Significant options for school choice now exist in urban school districts in the U.S. due to concern about poor academic outcomes for many poor urban youth. However, observers have noted that the realization of benefits from school choice depends heavily on the capability of students and their parents to make informed choices.

Our research uses data on a cohort of urban minority youth who have been followed from preschool through age 30 in the Chicago Longitudinal Study (CLS). The cohort of 1,500 students was comprised of entire kindergarten classrooms of children in approximately 25 Title 1-funded public schools located in the poorest neighborhoods in the city. About two-thirds of the CLS sample participated in a high-quality early educational intervention spanning preschool through second or third grade. By tenth grade, the CLS students were enrolled in a number of private schools and almost every public high school in Chicago. The quality of these public schools varied widely from selective magnets to high schools with extremely high rates of truancy and dropout.

Given the availability of public and private school choice in Chicago, students had a wide range of options for high school. We ask whether participation in an early education intervention targeted toward students and parents residing in high-poverty neighborhoods influences decisions about what high school to attend. Framing the question as a utility maximization problem, we analyze what characteristics of students, families and schools influence the school choice decision for high school. We use information on school attendance areas to investigate whether students who participated in the Child-Parent Center intervention were more likely to opt out of attending their neighborhood school and whether these students were more likely to attend different types of schools including private, magnet or high-quality public schools. Inverse probability weighting is used to better compare the high school choice decision of students who attended the CPC intervention with similar students who did not attend the early intervention program. Importantly, we use bounding strategies (Oster, 2015) to test the robustness of our CPC prediction model to omitted variable bias. One form of school choice is whether students remain in their neighborhood to attend the local public school or whether they opt-out of this choice and go elsewhere. We find no evidence that CPC participation increases the likelihood a student will opt out of their designated neighborhood school. However, the reasons for opting out differ between the treatment and control groups indicating CPC may influence the preferences for types or quality of high school. While we find no evidence that CPC students are more likely to attend a career academy or are less likely to attend a poor quality public high school, CPC students are also more likely than comparison group students to attend a private, magnet or high quality public school.

4.2 Related Studies:

The importance of school quality has been studied by a number of researchers including Card and Krueger (1992) and dozens of more recent studies. Card and Krueger

(1992) examined the impact of school quality on later wages using state-level information on pupil/teacher ratios and relative teacher wages as indicators of school quality. Card and Krueger found that higher school quality is not only associated with increased wages and years of schooling. Other studies examine the relationship between school spending and subsequent earnings or educational attainment. Card and Krueger's (1996) review of that literature suggests that a 10 percent increase in school spending is associated with a one to two percent increase in later earnings. An updated, more critical review of the school quality/adult earnings literature is found in Speakman and Welch (2006). High school quality also has been found to reduce the probability of unemployment (Eide and Showalter, 2005).

In addition to the research that suggests that school quality has important effects on later economic outcomes, school quality also has been examined in terms of its contribution to the sustainability of the effects of early intervention. Given the recent interest by economists in the role of early childhood education in human capital models of skill formation, various researchers have investigated whether school quality in the years after preschool can explain the persistence or fadeout of preschool benefits in terms of test scores. As Currie and Thomas (2000) and Lee and Loeb (1995) suggest, school quality can help explain some of the fadeout effects in test scores that are frequently found for the federal preschool program Head Start. Currie and Thomas (2000) find that although black and white students make gains in the Head Start program, black students are more likely to then attend lower-quality schools compared to similarly disadvantaged black non-Head Start children. They argue that this helps explain why some of the benefits of Head Start fade out for black children. Lee and Loeb (1995) find similar results for Head Start students in general. Students who had participated in Head Start are more likely to attend middle schools with lower average grade point averages, lower student SES, and a worse academic climate. Johnson and Jackson (2017) investigate the impact of school quality subsequent to attending Head Start using nationally representative data from 1968 to 2007, by exploiting variation in Head Start rollout and increases in kindergarten through 12th grade spending increases. Similar to the Currie and Thomas (2000) and Lee and Loeb (1995), Johnson and Jackson (2017) find that when Head Start children attend schools with higher per pupil spending in the adolescent years, students have higher educational attainment, economic outcomes and lower likelihood of incarceration. The impacts of Head Start followed by increases in spending in the kindergarten through 12th grade years are larger than the sum of the impacts of either Head Start or increased spending in isolation. These results highlight the importance of school choice following an early education intervention to help combat possible fadeout effects and increase the overall effectiveness of the intervention.

Another strand of the literature investigates the consequences of school choice for education attainment decisions. Using data on students throughout the Chicago school district from a similar time period, Cullen, Jacob and Levitt (2005, 2006) examine the impacts of school choice on educational attainment and find that despite open school choice, there is likely no causal relationship between opting out of a neighborhood school and higher achievement, unless students are opting to attend a career academy.

Various forms of school choice have flourished for several decades in urban areas in the U.S. as parents, students, and policy-makers seek solutions to low-educational attainment in high-poverty urban school districts (Gamoran, 1996). Other studies, including examinations of school choice in Milwaukee and Texas, present evidence that expanded options for school choice may lead to higher educational attainment. School choice may make a difference in student outcomes if decisions about the school choice are based primarily on educational attainment. However, as Cullen et al. (2005) note, not every school decision is made with educational attainment as the end goal. Students and parents derive utility from school choice based on other factors including peers, school safety and transportation (e.g., Gennetian et al., 2012). If parents and students are not adequately informed on the quality of schools in their choice sets, they cannot make a decision that will improve educational outcomes. If the decision is made for reasons other than academic, we cannot expect improved educational outcomes. School choice appears to be a complicated decision with multiple driving factors.

The determinants of school choice decisions in the United States have been examined in a number of studies. Long and Toma (1988) use family and school characteristics to determine how families choose public or private school. Demographic characteristics such as family income, race and religious background were significant across all models, while the impact of school-level characteristics varied by model. Glazerman (1998) analyzes elementary school choice in Minneapolis, Minnesota. He finds evidence that race, distance to school and SES all play an important role in kindergarten choice, while school and neighborhood quality variables had little to no impact. Determinants of school choice internationally have also been studied, including studies in Egypt (Hanushek, Lavy and Hitomi, 2006), Chile (Gallego and Hernando, 2008), Canada (Bosetti, 2004) and Australia (Le and Miller, 2003). Surveying parents of elementary students in Alberta, Canada, Bosetti (2004) finds that 60% of parents choose the private school because of small class size, the leading response to the survey. On the other hand, parents of children in public school most frequently responded that proximity to home was the most important factor in school choice. Both sets of parents valued academic reputation and teaching style, while parents of private school students reported choosing schools based on shared beliefs and values. Public school parents also noted that teachers and principals were important in their elementary school decision.

Given that the literature finds some potential benefit to school choice and the wide variety of factors that influence school choice, we investigate if an early educational intervention with a strong emphasis on parent involvement can influence later schooling decisions. In this paper, we do not treat school quality as an exogenous factor affecting the sustainability of the gains of early intervention. Instead, we seek to understand the high school choice decision and investigate whether an intervention with a strong focus on parental involvement in the early years of schooling can affect later school choice decisions. The students in the Chicago Longitudinal study (CLS), as well as those in many urban districts across the country, had significant opportunities for school choice.

4.3 Chicago Child-Parent Centers:

The early childhood intervention examined in this research is the Chicago Child-Parent Center program, a federally-funded program offered in schools located in high poverty neighborhoods in Chicago. The Chicago Child-Parent Center (CPC) program is an early education intervention targeted at students between the ages of 3 and 9 living in some of the poorest neighborhoods in Chicago. The CPC program offers a high-quality preschool program staffed by teachers with four-year degrees and small class sizes of 8 or 9 children per teacher or teacher's aide. Started in the 1960s and still ongoing today, the CPC program is a comprehensive, educational intervention with an intensive parental involvement component. Students may enroll in the CPC preschool program for one or two years and then continue in the elementary school component of the program that offers small class sizes, field trips, and a modest amount of additional classroom resources.

The name "Child-Parent Center" indicates that parental involvement is an important component of the program. Parents are expected to volunteer at least one-half day each week at the center in various capacities. Each CPC site has a dedicated parent-resource teacher who encourages parent participation and a parent resource room that provides a location for parent program activities. This is an important component, because high school choice is likely influenced by both parent and student. With the extensive parent component, the CPC program instills the value of education in both parties.

Although the Child-Parent program has been in existence for 50 years, studies of the CPC are especially important in light of the recent strong national interest by policymakers in Prek-3 education. As described by Shore (2009) and Takanishi (2011), concern about the perceived lack of persistence of the benefits of preschool, especially for economically-disadvantaged children, has focused attention on the need for early programs that last longer, perhaps through third grade. In recent years, additional funding has been targeted toward expanding the Child-Parent center program. In 2011, the U.S. Department of Education awarded an Investment In Innovation (i3) grant of $15 million to expand the CPC program in Chicago and other Midwestern cities. More recently, the CPC program in Chicago received $16 million from Goldman Sachs to expand access to unserved children as part of an experiment in social impact borrowing whereby private investors pay for an expansion in certain preventative social or educational service offerings but expect to be paid back by the government cost savings arising from the interventions (Temple and Reynolds, 2015).

4.4 Chicago Longitudinal Study of the Child-Parent Centers:

The Chicago Longitudinal Study (CLS) follows 1,539 students born in 1979 and 1980 that attended schools in Chicago. The majority (989) of the students attended kindergartens in 20 different CPC sites that offered the preschool program. The CLS sample consists of entire kindergarten classes in the 20 CPC sites as well as a comparison group of 550 students without CPC preschool drawn from 5 randomly-selected public kindergartens in schools with similar high-minority, high poverty populations. The quasi-experimental study design of the CLS allows for the comparison of program participants with nonparticipants who are well matched on observed characteristics.

In the current study, we focus on a subsample of 1,154 students (75% of the original sample) for whom we have information elementary school and high school attendance. Table 4.1 illustrates the comparability of the treatment and control groups of this sample.

We also examine a sample of students with 8^{th} grade school attendance and high school attendance, 1,136 students. At the time that the original study of the CPC program began, the matching of CPC students with nonparticipants was undertaken using a small number of school-level characteristics (such as race and percent eligible for subsidized lunch) to create the matched study design. In more recent years, CLS researchers obtained data from public birth records providing additional student or family-level characteristics measured at birth. These additional variables help control for differences across students in family socioeconomic risk. They include indicators of whether the mother was a teenager or a single parent at the child's birth, how many other children the mother already had, and whether the mother had completed high school. Additional information from the birth reports and other administrative data sources provide information on the mother and family during the child's age from birth to age 3. These additional factors representing family socioeconomic risk include AFDC (now TANF) participation, unemployed status of mother, whether the family's income would qualify for subsidized lunch at school, and whether reports on child neglect or mistreatment has been filed with the Department of Family Services. An overall risk index was created that was the sum of indicator variables for each of these eight individual risk factors. An additional variable reflects the poverty rate in the elementary school attendance area. Table 4.1 indicates that, overall, there is no significant difference on overall level of risk factors. Comparing students who participated in the CPC preschool to those who did not, the CPC students are more likely to be female, come from poorer neighborhoods and are more likely to have mothers that completed high school although both groups have 50-

60% of students who have mothers that did not complete high school. Interestingly, the difference in maternal education exists for boys only. While the construction of the high school quality variable is described in greater detail below, there was no significant difference in the average quality of the neighborhood high schools based on the location of the 8th grade school that the student attended, though there was a significant difference on the quality of high schools based on the kindergarten location.

The quasi-experimental matched study design is useful because it reduces both observed and unobserved heterogeneity in differences between treatment groups. Simple single-equation models may generate unbiased estimates of the CPC effects. However, given the nonrandomized nature of the study design, we will implement more refined techniques in order to obtain stronger causal inferences to better understand the impact of CPC participation on high school choice.

4.5 School quality measures:

We use three measures in an attempt to capture school quality. The first is a dummy variable that indicates whether or not a student attended a magnet or private high school. While the literature on the effectiveness of magnet schools is mixed (Gamoran, 1996, Blank, 1989, Ballou et al, 2006, Neild 2004), previous work using the CLS study has indicated that CPS magnet schools may be effective in improving academic achievement (Ou and Reynolds, 2008). Given the positive impacts on academic outcomes found in Cullen et al (2005), we also analyze attendance in career academies.

The other measure is a composite variable of school characteristics that attempt to capture the quality of the school. Due to both data limitations on within district variation in school resources and concerns expressed by Hanushek (2006), we do not use data on school resources, but student behavior variables. This creates an index that captures the overall quality of the school but does not necessarily provide guidance for improving schools or policy implications. The composite variable is the sum of z-scored variables measuring a particular school's graduation rate, the mobility rate, the truancy rate, the attendance rate, the percent of low-income students and the percent of students at or above grade level on the Tests of Achievement and Proficiency (TAP) math standardized test. We sharpen comparisons by creating dummy variables indicating the top and bottom quartile schools in the school quality index. Because the study concerns high school choice, we limit the sample to those students that attended high school in Chicago schools for which we have data, comprising 75 percent of the original sample. Table 4.2 illustrates school attendance rates of different types of schools by treatment status.
It appears that CPC attendance is correlated with higher rates of attendance of good schools, but not correlated with lower rates of attendance of poorer schools. This is likely because the CPC sites are generally located in the poorest neighborhoods in the city in which poorer quality high schools are more likely to be located. Students that attended CPC and do not elect to opt out of their neighborhood school are likely to be tracked to a lower-performing school. This must be accounted for when determining the impacts of CPC attendance.

4.6 School Choice Decision:

Beyond the decision of which school to attend, we are also interested in how students and parents make their schooling decisions. Given that Chicago Public Schools had open school choice, students were generally free to attend a public school of their choice, though public schools gave preference to students living within the attendance boundaries. Private and magnet schools often had admission requirements that limited attendance to those schools. CPS did not provide transportation, however, so school choice may be constrained by the ability of the students to secure transportation to the school.

We begin by analyzing the decision to opt out of the local high school. Unlike Cullen et al (2005), the CLS data do not include individual addresses so we cannot use a distance measure in our analysis of choice. However, we know which elementary school each student attended in kindergarten and 8th grade school the student attended. Using data from the SABINS project from 1999, which identifies school attendance boundaries in 13 metropolitan areas, including Chicago, we were able to overlay high school attendance boundaries with the attendance boundaries of the kindergarten and eighth grade schools the students attended. CPC students started attending high school in the 1994-1995, at the earliest. There was no data available from Chicago Public schools regarding attendance zones or school tracking for those years. The SABINS project represents the best available data for the school attendance zones. Using GIS, we overlaid the attendance boundaries of the kindergarten and 8th grade sites with the attendance boundaries for the Chicago Public high schools. The overlap in attendance boundaries was used to identify the local high schools, based on the area of the overlap of the attendance boundaries. For example, 60 percent of the students attended a kindergarten that had an attendance boundary that was completely within the attendance boundary of a single high school. That school was considered to be their local high school. For the remaining 40 percent of the sample, the attendance boundary was within the attendance boundaries of two high schools. If the student attended any school other than one of the two, that student was considered to have opted out of their local school. When we examine the relationship between the attendance boundaries between the eighth-grade site and the high school site, 43 percent of the sample had a single high school with overlapping attendance boundaries. The remaining sample had 2 or 3 total high schools as possible local high schools. Figures 1 and 2 demonstrate the overlap of attendance boundaries between kindergarten and high school sites, and middle school and high school sites, respectively.

Table 4.3 presents simple, unadjusted means of opt-out rates by CPC participation, gender and risk status. There is no significant difference in opt-out rates by treatment status, based on either the kindergarten location or the 8th grade location. Female students were more likely to opt-out than their male counterparts at the 8th grade site, while higher risk students were significantly less likely to opt-out of their neighborhood high school. Overall, the opt out rates for this sample were in-line with the citywide average for the time period (Chicago Consortium of School Research).

4.7 School Choice Model:

Families may opt out of their neighborhood school in several ways. Each incurs a cost. Given that schools in Chicago give enrollment precedence to students in the neighborhood attendance boundaries, families may opt to re-locate to neighborhoods with higher quality schools. This incurs the cost of moving, and may incur higher housing costs if school quality, as Epple and Romano (2003) suggest, is a function of per student expenditures. Our school choice model follows Epple and Romano (2003) and Nechyba (2000, 2003). We assume that families are rational, utility-maximizing decision-makers with regards to school choice. Where utility is defined by the quality of the high school attended, *E*, and consumption of a composite good, *Z*. We analyze the school choice decision according to the following model:

$$U_i = Z^{\alpha} E^{\beta}$$

where:

$$\frac{\partial U}{\partial Z} \geq 0$$

and,

$$\frac{\partial U}{\partial E} \geq 0$$

which allows families to choose a school that maximizes their utility subject to their budget constraint:

$$BC = p_1 Z + p_2 E$$

where we assume that $p_{2,opt\text{-}out} > p_{2,neighborhood}$, that price to the family of opting out is greater than that of attending the neighborhood school. The price increase from opting out of the local school arises from the additional cost of moving to a neighborhood with a higher quality school or increased transportation costs to a school other than the neighborhood school. Families choose a school that will maximize utility from their choice set, which includes all public schools in Chicago, as well as all private and magnet schools that the student met admission requirements. It is possible, even likely, that some students and families would have maximized their utility by attending a private school, but were unable to, given admission or tuition requirements. Given the infeasibility of attending these schools, they would not be included in the choice set for the student and family.

Our hypothesis is that attending the CPC early education program increases the utility derived from a student attending a high quality high school later in life. Because the program has elements for both the student and the family, the utility derived is at the household level, which is important because there is likely a range of influence on school choice in the household, from entirely on the student to entirely on the head of household. We do not formalize in which way the preferences for education are changed, though we believe it arises, at least in part, through the extensive parent programming of the CPC. With a strong parent component that encourages participation in developing education skills in their children, as well as offering educational courses for the parents themselves, including GED courses, the parents of CPC students have more exposure to the benefits of education. In addition, Heckman's (2007) human capital model proposes that investments in early education are worthwhile because higher human capital early in life makes the acquisition of human capital skills later in life easier. With the demonstrated effectiveness of CPC in raising human capital skills (Reynolds et al, 2011), perhaps families identify that the performance of the student may continue to improve with a higher quality educational experience. Regardless of the root cause, which may differ within the participant family, we expect to see:

$$\frac{\partial U_{CPC}}{\partial E} > \frac{\partial U_{Control}}{\partial E} \geq 0$$

where the value of high quality high school leads to higher utility for CPC families, even at the expense of decreased consumption of other goods. We allow for the possibility that there is no utility gained from a higher quality high school for the control students.

4.8 Methodology:

To estimate the impacts of CPC on school choice, we begin by estimating the relationship between CPC participation and the opt-out decision and CPC participation and school type. To determine the influence of the CPC program, we estimate the following regression:

1) 1) $Y = \beta X + W_1 + \varepsilon$

Where Y represents the school choice decision, X is a dummy variable indicating CPC participation, and W_1 is a vector of control variables of demographic characteristics of the child and the mother, as well as a measure of neighborhood school quality. Despite the quasi-experimental nature of the study, we are concerned about potential bias from two sources; the existence of significant group differences at baseline and the possibility of omitted variables that influence both the probability of CPC participation and the school choice decision.

4.8.1 Inverse Probability Weighting

To help address concerns of group differences between CPC participants and control students, we use Inverse Probability Weighting (IPW) as an estimation strategy. IPW can help minimize the bias caused by differences in observed characteristics. Given that our sample is not randomly drawn and we see significant differences in several baseline characteristics, IPW methodology may provide more efficient estimates of the impacts of the CPC program than the linear probability model. Of particular concern, CPC students had a significantly smaller percentage (52% to 59%) of mothers who did not graduate from high school. If the school choice decision is a household decision with influence from both parent and student, families with a higher education level may be more inclined to send their child to a higher quality high school, regardless of preschool experiences. To account for this, we estimate the propensity (Rosenbaum and Rubin, 1983) for CPC participation given characteristics of the student and family prior to program selection, focusing on variables that only occur prior to program participation and are informed by prior CPC studies (Arteaga, 2014) and economic theory (Heckman and Navarro-Lozano, 2004) for matching in economic choice models. The models we use to estimate the impacts of CPC participation include the baseline covariates described above, as well as the measure of neighborhood high school quality. Table 4.4 presents the propensity score models, where model 1 uses the kindergarten location for the school quality variable, while model 2 uses the eighth-grade school attended. The predicted propensity scores for each model satisfied the overlap condition and no score needed trimming. The distributions of the scores for each model are presented below.

We then estimate outcome equations by weighting the results by the inverse of the propensity score, using Oster's notation, calculated as:

$$z_i = \frac{X_i}{\Pr(X_i|W_1)} - \frac{1 - X_i}{1 - \Pr(X_i|W_1)}$$

so CPC students who were less likely to receive the treatment were weighted more heavily, while control students who were more likely to receive the treatment were also weighted more heavily.

To test the robustness of the weighting strategy, we examine the standardized differences between treatment and control groups for the weighted and unweighted samples. As Austin and Stuart (2015) note, studies frequently assume that the weighting procedure has reduced bias in observable characteristics, without running balance diagnostics. One potential diagnostic is to ensure that the standardized difference for all variables included is less than 0.10. Table 4.5 presents the standardized differences before and after weighting. Prior to weighting, we see differences in gender and mother's education larger than the 0.10 threshold. Following weighting, those differences have been effectively reduced. We see a reduction in the standardized differences in all variables included. With our inverse probability weighting strategy, we have weighted the treatment and control groups to be more comparable on baseline characteristics and minimize concerns of bias arising from differences in treatment groups.

4.8.2 Coefficient Bounding

While the IPW approach help minimize the potential bias arising from significant differences between the treatment and control groups, the validity of our estimation results are threatened in the presence of unobserved, endogeneous variables. To address this, we test the sensitivity of our coefficient estimates using methodology proposed by Oster (2015), based on work by Altonji, Elder and Taber (2005). Altonji, Elder and Taber investigate the impacts of Catholic high school attendance on education achievement. However, lacking a good instrument, the authors are concerned that a variable that is correlated with both Catholic high school attendance is also correlated with academic achievement. If this is true, Altonji et al may inaccurately attribute the impacts on academic achievement to Catholic school attendance, when in fact they should be attributed to family characteristics. This is a concern for our paper as well. If we omit a variable that is correlated with CPC attendance *and* correlated with school choice, our previous estimates may be in accurate. To test for the impact of selection on unobservables, Altonji et al make assumptions about the relationship between the observable variables and the unobservable variables. By beginning with the unadjusted means of the outcome variables (for example, 18 percent attendance in high quality schools for CPC students compared to 12 percent attendance for control students), Altonji et al examine how much the difference changes as additional covariates are added to the model. They assume that unobserved variables will behave in a similar way and construct a bound based on coefficient movements. They assume that the observable covariates, in our model W_1, are chosen at random from the full set of variables at determine the outcome, Y. They also assume that the number of variables that are observable and unobservable are large, and that the variation in the outcome variable that is related to observable variables has the same relationship with the endogeneous variable as the variation in the outcome that is related to the unobservable variables. Altonji et al note that while these are strong assumptions, they are not prohibitive in estimating the bounds. In applying these bounds, Altonji et al find significant, positive impacts on academic achievement, namely high school graduation, despite the presence of omitted, endogeneous variables.

Oster (2015) proposes a bounding method based on the methodology of Altonji et al, with more relaxed assumptions. Namely, Oster does not require the assumption that difference in the outcome variable necessarily has the same relationship to the unobserved and observed variables. To accomplish this, Oster's bounds analyze changes in both coefficients and the R^2 as additional variables are added to the regression model to determine the sensitivity of the coefficient estimates to missing variables. Oster uses a regression model:

2) $Y = \beta X + W_1 + W_2 + \varepsilon$

Where β is the coefficient on the treatment variable (CPC participation, in our case), W_1 is a vector of observed control variables and their coefficients and W_2 are unobserved. To determine the relationship between the treatment variable and unobserved measures, we can use information from the relationship between the treatment and observed measures, which requires making assumptions about the proportionality between observed and unobserved (the proportional selection assumption). Technically:

3) $\frac{Cov(X,\varepsilon)}{Var(\varepsilon)} = \delta \frac{Cov(X,W_1)}{Var(W_1)}$,

where δ is the degree of proportionality between the observed and unobserved confounders. Because W_2 cannot be included in the regression model, estimating the true model is impossible. What is possible is estimating two regressions, once exclusively controlling for the treatment variable on the outcome and a second regression that controls for the treatment variable and observed covariates. The differences in the coefficient on the treatment variable *X* and the differences between r-squared values between the two regression models serve as inputs into the bias calculation Oster outlines. This also requires assumptions regarding the degree of proportionality and the maximum achievable r-squared. Oster assumes that $\delta>0$. If $1> \delta>0$, the observed variables have more influence on the estimate than unobserved variables. In practice, Oster recommends assuming $\delta=1$, equal importance between observed and unobserved variables.

We also need to determine a value of the maximum R^2 value for our school choice models, R_{max}. Oster recommends using a maximum value of 1, if the variation in school choice can be completely explained by covariates, or 1.3 times the R^2 value of the estimated linear probability model. Oster find that using a value of 1.3 times the R^2 allows for 90 percent of the results of 65 papers using randomized control trials to remain significant.

By combining the results from the controlled and uncontrolled regressions, assumptions of the R^2 and the degree of proportionality, δ, we are able to estimate a bound on our estimate of the treatment effects. If the results from the linear probability model are significant and the bound estimated following the Oster procedure do not include 0, we can conclude the estimates are robust to omitted variable bias. An additional robustness approach that we employ includes holding the estimated β=0 and calculating the size of the δ. This approach allows us to estimate how important the unobserved factors would need to be relative to the observed factors to reduce our treatment estimate to 0. A larger δ indicates a more robust result.

4.9 Analysis:

We begin by investigating factors that influence students' decisions to opt out of their local high schools. Table 4.6 provides multiple regression and inverse probability weighting results on the opt-out decision based on both the kindergarten site and the 8^{th} grade site. We control for the set of risk factors outlined in previous CPC studies. We also include a variable that is the mean of school quality index for the local high schools (maximum two for the kindergarten site, three for the eighth-grade site) that had overlap with the attendance boundaries of the attended kindergarten site or eighth grade site, depending on the analysis. The same model was used in the IPW estimation. We find no significant impact of CPC participation on the decision to opt-out of attending the local high school. This is consistent across estimation strategy and the year the neighborhood school was identified. This results is perhaps unsurprising, given the high rates of opt-out in our sample that are consistent with estimates of opt-out rates in Chicago Public Schools at the time.

We then analyze the opt-decision separately for CPC students and control students to determine the impact of neighborhood school quality plays in the opt-out decision. Again, we implement multiple regression models controlling for the same covariates. Given the outcomes are binary, we also estimate probit models and calculate marginal effects for comparison. Finally, since variable of interest is continuous, we implement the Oster (2015) bounds for robustness. Table 4.7 presents the results for CPC students, using the kindergarten school as a reference. We find that the higher the neighborhood high school ranks on the quality measure, the less likely a student is to opt out of attending that school. Because the treatment coefficients are significant and the bounds do not include 0, the results are robust to omitted variable bias. When holding β=0, we find the degree of proportionality required for this to be true is 37.3, further reaffirming the significance of the results. However, the r-squared value for this regression model is fairly small, only about 10% of the variation is explained. For robustness, we set R_{max} equal to 1, making the assumption that our true model could explain 100 percent of the opt out decision. Even with this stringent requirement, our results are significant. This indicates that local school quality plays an important role in the opt out decisions of CPC students. When we analyze the opt out decision based on the eighth-grade school attended, the results are similar. See Table 4.8. Again, local school quality plays a significant role in the opt out decision for CPC students. While this result is robust to the suggested 1.3 times r-squared as suggested by Oster (2015), it does not continue to hold if we assume our true model can fully explain the opt-out decision.

The story changes when we estimate the role of neighborhood school quality on the opt out decision for control students. There are no significant impacts, either using the kindergarten sites or the 8th grade sites, of neighborhood school quality on the opt-out decision. While the bounds do not include 0, the degrees of proportionality are very small, indicating the effect size of local school quality is effectively null. Results by location are presented in Tables 4.9 and 4.10. As school quality increases, CPC students are less likely to opt-out of their neighborhood high school. This relationship does not hold for non-CPC students. It appears that CPC students and families take into account the quality of the local high school in their choice decision. These results are consistent with our hypothesis that the CPC program is effective in increasing the value of education amongst the participants and their families. Choosing a higher quality school increases the utility of the household, even if it imposes a higher cost on the family.

We continue to analyze school choice by predicting the type of high school a student elected to attend. We begin by creating a variable that indicates what type of high school a student attended. Model 1 estimates the probability a student attended a magnet or private school. Model 2 analyzes attendance at a career academy, given the positive outcomes found in Cullen, et al (2005). Models 3 and 4 the probability a student attended a school that scored in the top or bottom quartile of the quality variable, respectively. We begin with a linear probability model to estimate the impacts of CPC on type of school attended. Again, we estimate Oster (2015) bounds to test the responsiveness of the estimate to omitted variable bias. Finally, we estimate the model using inverse probability weighting to minimize any bias that may arise from differences in observed characteristics. Table 4.11 presents the results. A consistent pattern emerges. CPC students are significantly more likely to attend a magnet or private school, or a public school that rates in the top quartile of schools based on the school quality variable. These results are consistent across estimation strategy. Similarly, there is no significant impact on attendance at career academies and CPC does not help students avoid schools that rank in the bottom quartile of our school quality variable.

4.10 Conclusions

Using several strategies, including propensity score weighting, we are able to analyze the high school choice decision of Chicago area students, mostly those that attended the Chicago Child Parent Centers. Given that the program had a significant parent component we expected that families with CPC students would have a higher value of education, which would manifest in the type of high school the student attended. While CPC students were not more likely to opt out of attending their local high school, the quality of those schools played a significant role for CPC students only, indicating that CPC may change the preferences for education consumption among the participating families. Along those lines, we find evidence that students who attend the CPC program are more likely to attend a magnet, private or high-quality public school. Again, reinforcing the idea that the CPC program instills a higher value of education among its participants. However, it is possible that rather than changing the preference for education, the CPC program provides the parents the skills to identify a high-quality school, where control families do not have that capability. Given that CPC families are more likely to attend a magnet or private school, clear signals of high school quality, even if the literature on the effectiveness of those schools are mixed, we believe the results shown in this study are indicative of a change in preferences.

This study does not answer what the longer-term implications of school choice are and this drives the future directions. There was no evidence that CPC participation influences the likelihood of attending a high school linked with student outcomes as found by Cullen et al (2005). CPC students were no more likely to attend a career academy than the control group. The next stage of the analysis is to link in academic and adult outcomes to see if the types of schools students chose to attend impacted their academic achievement or economic well-being in the years following school. This data is readily available and more is currently being collected of the sample at age 35. While changing preferences for education is an important result, it would be worthwhile to see demonstrable impacts of this change.

There are several key limitations of this study. Our study is explaining a relatively small amount of the school choice decision and while our methodology indicates robust results of the impact of CPC and neighborhood school quality, it is hard to determine what other factors drive school choice. We are also unable to determine by how much the preference for educational quality changes or how much the costs incurred by opting out of the local high school influence the school choice decision. Finally, another limitation is the amount of missing data from the original sample of 1,539 students. Our sample includes 1,154 students with valid high school data, the roughly 25 percent of students had no high school data. The 25 percent missing is a higher percentage than the percentage of students that attended any particular high quality school category analyzed in this paper. Of the total sample, 6 percent attended high school outside of Chicago, while the remaining either did not attend high school or left the sample prior to high school. Data was not available on the type or quality of the school attended outside the district. Future directions of this research include linking the choice of school to academic outcomes, including high school graduation and college attendance.

Table 4.1: Baseline equivalence of treatment and control groups

VARIABLES	CPC (n=766)	Control (n=388)	Difference
Black	93.99%	93.04%	0.01
Female	54.96%	48.71%	0.06**
Mother younger than 18 at child's birth	16.19%	16.24%	0.00
60% or greater poverty in school area	78.33%	72.68%	0.06**
Single Parent Status, Child 0-3 years	79.50%	76.03%	0.03
Mother did not complete high school, Child 0-3 years	52.61%	59.54%	-0.07**
4 or more children in household, Child 0-3 years	15.93%	18.04%	-0.02
AFDC participation, Child 0-3 years	65.27%	63.66%	0.02
Eligible for free lunch, Child 0-3 years	85.38%	84.54%	0.01
Any child welfare case histories, Child 0-3 years	2.87%	4.12%	-0.01
Total number of risk variables	4.59	4.53	0.06
Average High School Quality, based on k location	-3.45	-3.85	0.40*
Average High School Quality, based on 8^{th} grade	-2.0	-2.2	0.2

*** p<0.01, ** p<0.05, * p<0.1

Table 4.2: High school attendance rates by type and by treatment status

VARIABLES	Any Prek (n=766)	Control (n=388)	Difference
Magnet or Private School	18.28%	13.66%	0.0462**
Career Academy	23.72%	22.34%	0.0138
Top Quartile School	18.54%	12.40%	0.0614***
Bottom Quartile School	36.29%	38.24%	-0.0195

*** p<0.01, ** p<0.05, * p<0.1

Table 4.3: Opt Out of Neighborhood High School Rates by Group

	Any Prek	Control	Female	Male	4 or More Risk	3 or Fewer Risk
Opt out based on Kindergarten school	70.76%	72.68%	71.97%	70.77%	65.80%***	79.74%***
Opt out based on 8th grade school	62.88%	61.52%	65.18%**	59.29%**	56.54%***	71.18%***

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.4: CPC Participation prediction models by kindergarten and eighth grade sites

VARIABLES	(1) CPC participation (kindergarten site)	(2) CPC participation (8^{th} grade site)
Neighborhood School Quality	0.0219*	0.0107
	(0.0124)	(0.0113)
60% or greater poverty in school area	0.149	0.157*
	(0.0918)	(0.0920)
Black	-0.0107	-0.00964
	(0.165)	(0.168)
Female	0.148*	0.151*
	(0.0775)	(0.0782)
Single Parent Status, Child 0-3 years	0.119	0.121
	(0.101)	(0.102)
Mother younger than 18 at child's birth	0.0427	0.0295
	(0.114)	(0.115)
Mother did not complete high school, child 0-3 years	-0.202**	-0.212**
	(0.0855)	(0.0861)
4 or more children in the household, child 0-3 years	-0.0545	-0.0554
	(0.109)	(0.109)
AFDC participation, child 0-3 years	-0.0425	-0.0356
	(0.108)	(0.109)
Mother unemployed, child 0-3 years	0.123	0.102
	(0.105)	(0.106)
Eligible for free lunch, child 0-3 years	0.0311	0.0320
	(0.116)	(0.118)
Any child welfare case histories, child 0-3 years	-0.234	-0.229
	(0.210)	(0.209)
Missing any risk variable, child 0-3 years	-0.280**	-0.292**
	(0.137)	(0.139)
Constant	0.296	0.253
	(0.206)	(0.206)
Observations	1,154	1,136

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.5: Standardized difference of treatment and control groups pre- and post-weighting

VARIABLES	Pre-weighting	Post-weighting
Black	0.0387	-0.0096
Female	0.1252	0.0102
Mother younger than 18 at child's birth	-0.0013	-0.0007
60% or greater poverty in school area	0.1315	-0.0009
Single Parent Status, Child 0-3 years	0.0835	0.0014
Mother did not complete high school, Child 0-3 years	-0.1397	-0.0062
4 or more children in household, Child 0-3 years	-0.0563	-0.0059
AFDC participation, Child 0-3 years	0.0337	0.0027
Eligible for free lunch, Child 0-3 years	0.0235	-0.0026
Any child welfare case histories, Child 0-3 years	-0.0681	-0.0014
Average High School Quality, based on k location	-0.0512	-0.0385

*** p<0.01, ** p<0.05, * p<0.1

Table 4.6: OLS and IPW estimates on opt-out decision

VARIABLES	OLS Opt out k	OLS Opt out 8	IPW Opt out k	IPW Opt out 8
Any CPC PreK	-0.00246	0.0135	-.0067552	.0107103
	(0.0272)	(0.0300)	(.0278028)	(.0303811)
Neighborhood School Quality	-0.0247***	-0.0150***		
	(0.00372)	(0.00400)		
60% or greater poverty in school area	-0.149***	-0.0310		
	(0.0329)	(0.0341)		
Black	0.00168	0.0700		
	(0.0546)	(0.0597)		
Female	0.0126	0.0543*		
	(0.0257)	(0.0282)		
Single Parent Status, Child 0-3 years	-0.0201	0.0208		
	(0.0341)	(0.0371)		
Mother younger than 18 at child's birth	0.0130	0.0166		
	(0.0378)	(0.0416)		
Mother did not complete high school, Child 0-3 years	-0.0598**	-0.0649**		
	(0.0285)	(0.0311)		
4 or more children in household, Child 0-3 years	-0.0985***	-0.0923**		
	(0.0352)	(0.0392)		
AFDC participation, Child 0-3 years	-0.108***	-0.128***		
	(0.0362)	(0.0395)		
Mother Unemployed, Child 0-3 years	0.0632*	0.0560		
	(0.0351)	(0.0388)		
Eligible for free lunch, Child 0-3 years	-0.0480	-0.0575		
	(0.0405)	(0.0436)		
Any child welfare case histories, Child 0-3 years	0.0302	0.175**		
	(0.0720)	(0.0842)		
Missing any risk variable	0.0887*	0.0111		
	(0.0495)	(0.0513)		
Observations	1,154	1,136	1,154	1,136

IPW estimates control for neighborhood school quality, neighborhood income, race, gender, Mother's age at birth, single parent, educational, employment status. Also included were measures of the number of children in the household, AFDC participation, free lunch eligibility and any child welfare case history. A

dummy variable indicating if any of these variables was imputed was also included. Standard errors were clustered at the school level. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.7: Impacts of neighborhood school quality on Opt Out Decision based on kindergarten site, CPC students

VARIABLES	Probit MFX	CPC Opt out k OLS	Bound (1.3*R^2)	δ (1.3*R^2)	Bound (R^2=1)	δ (R^2=1)
School quality	-0.0310***	-0.02983***	-0.0306	37.3	-0.1552	3.251
	(0.0279)	(0.00850)				
Observations		766				
R-squared		0.101				

Estimates control for neighborhood school quality, neighborhood income, race, gender, Mother's age at birth, single parent, educational, employment status. Also included were measures of the number of children in the household, AFDC participation, free lunch eligibility and any child welfare case history. A dummy variable indicating if any of these variables was imputed was also included. Standard errors were clustered at the school level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.8: Impacts of neighborhood school quality on Opt Out Decision based on 8th grade site, CPC students

VARIABLES	Probit MFX	CPC Opt out k OLS	Bound (1.3^*R^2)	δ (1.3^*R^2)	Bound (R^2=1)	δ (R^2=1)
School quality	-0.0204***	-0.0203***	-0.02027	22.5	0.0134	0.9514
	(0.00746)	(0.00850)				
Observations		757				
R-squared		0.0619				

Estimates control for neighborhood school quality, neighborhood income, race, gender, Mother's age at birth, single parent, educational, employment status. Also included were measures of the number of children in the household, AFDC participation, free lunch eligibility and any child welfare case history. A dummy variable indicating if any of these variables was imputed was also included. Standard errors were clustered at the school level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.9: Impacts of neighborhood school quality on Opt Out Decision based on kindergarten site, Control students

VARIABLES	Probit MFX	CPC Opt out k OLS	Bound ($1.3*R^2$)	δ ($1.3*R^2$)	Bound (R^2=1)	δ (R^2=1)
School quality	0.004	0.003	0.0137	-0.3742	1.223	-0.009
	(0.01580)	(0.00612)				
Observations		388				
R-squared		0.079				

Estimates control for neighborhood school quality, neighborhood income, race, gender, Mother's age at birth, single parent, educational, employment status. Also included were measures of the number of children in the household, AFDC participation, free lunch eligibility and any child welfare case history. A dummy variable indicating if any of these variables was imputed was also included. Standard errors were clustered at the school level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.10: Impacts of neighborhood school quality on Opt Out Decision based on 8th grade site, Control Students

VARIABLES	Probit MFX	CPC Opt out 8th OLS	Bound (1.3*R^2)	δ (1.3*R^2)	Bound (R^2=1)	δ (R^2=1)
School quality	-0.002 (0.00748)	-0.0023 (0.00612)	-0.0007	1.41	0.581	0.0195
Observations		379				
R-squared		0.043				

Estimates control for neighborhood school quality, neighborhood income, race, gender, Mother's age at birth, single parent, educational, employment status. Also included were measures of the number of children in the household, AFDC participation, free lunch eligibility and any child welfare case history. A dummy variable indicating if any of these variables was imputed was also included. Standard errors were clustered at the school level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.11: School Choice Decision, Regression, Bounds, IPW

	Magnet or Private			Career Academy			Top Quartile School			Bottom Quartile School		
VARIABLES	OLS	Bound	IPW	OLS	Bound	IPW	OLS	Bound	IPW	OLS	Bound	IPW
CPC (k site)	.0546**	0.0575	0.0544**	0.0133	0.014	0.0125	0.0544**	0.0526	0.0587***	0.0001	0.007	-0.00079
	(0.02318)		(0.0217)	(0.0264)		(0.0263)	(0.02247)		(0.0212)	(0.02880)		(0.0287)
Observations	1,154			1,118			1,154			1,154		
R-squared	0.0417			0.0594			0.0895			0.117		
CPC (8th grade)	0.0599**	0.0619	0.0675***	0.0048	0.003	0.00449	0.0577***	0.0553	0.0602***	0.002	0.0065	0.00175
	(0.0232)		(0.0221)	(0.0276)		(0.0267)	(0.0223)		(0.0234)	(0.0288)		(0.0287)
Observations	1,136			1,101			1,136			1,136		
R-squared	0.0411			0.0613			0.1115			0.1292		

Estimates control for neighborhood school quality, neighborhood income, race, gender, Mother's age at birth, single parent, educational, employment status. Also included were measures of the number of children in the household, AFDC participation, free lunch eligibility and any child welfare case history. A dummy variable indicating if any of these variables was imputed was also included. Standard errors were clustered at the school level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 4.1: Overlap of Kindergarten and High School Attendance Boundaries

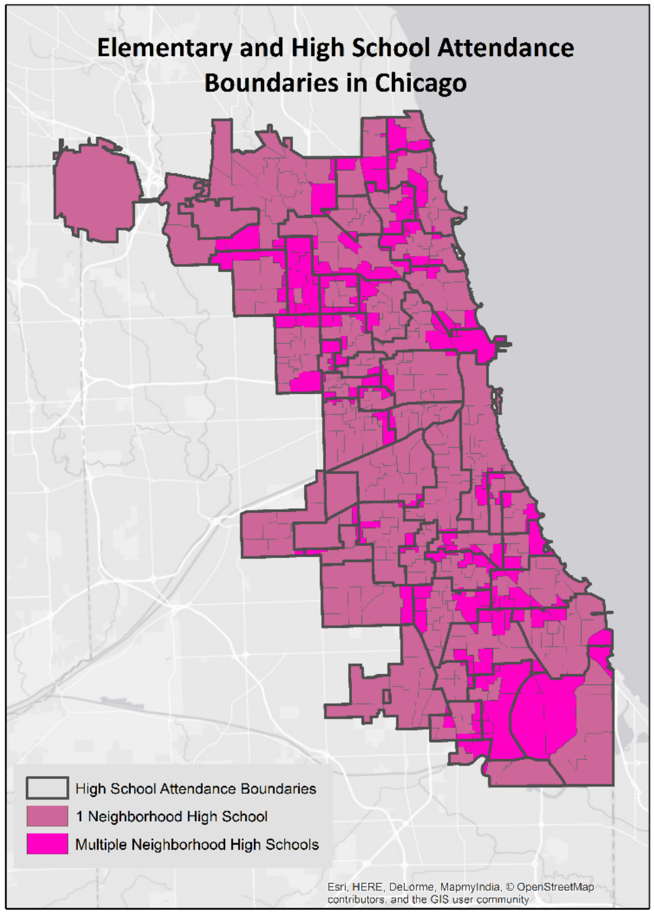


Figure 4.2: Overlap of Eighth Grade and High School Attendance Boundaries

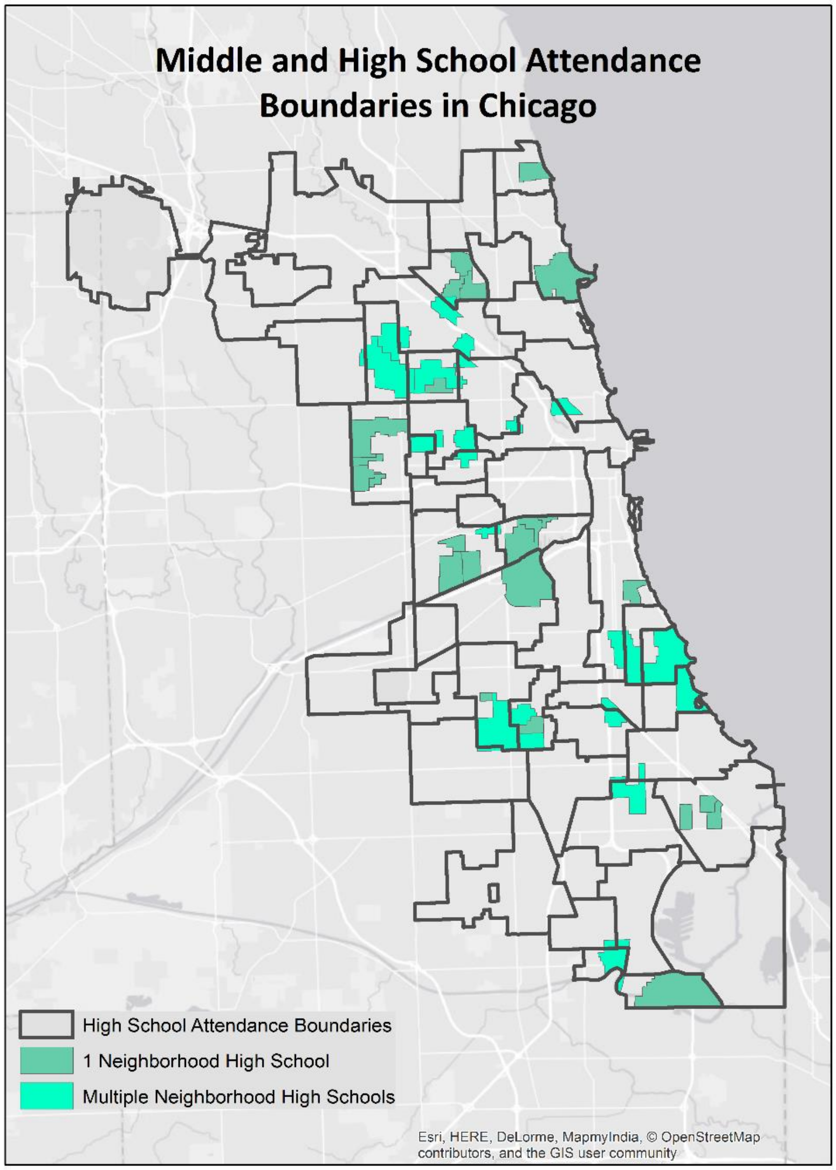


Figure 4.3: Distribution of Propensity Scores (Kindergarten site)

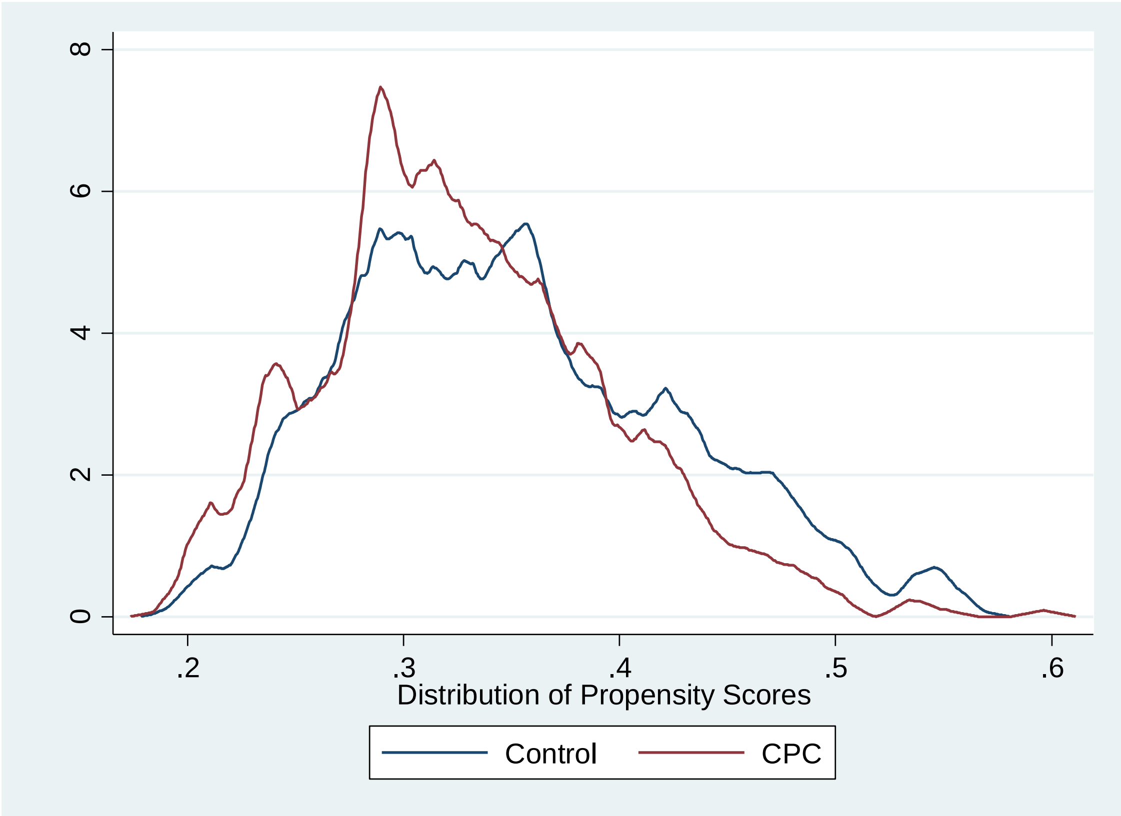
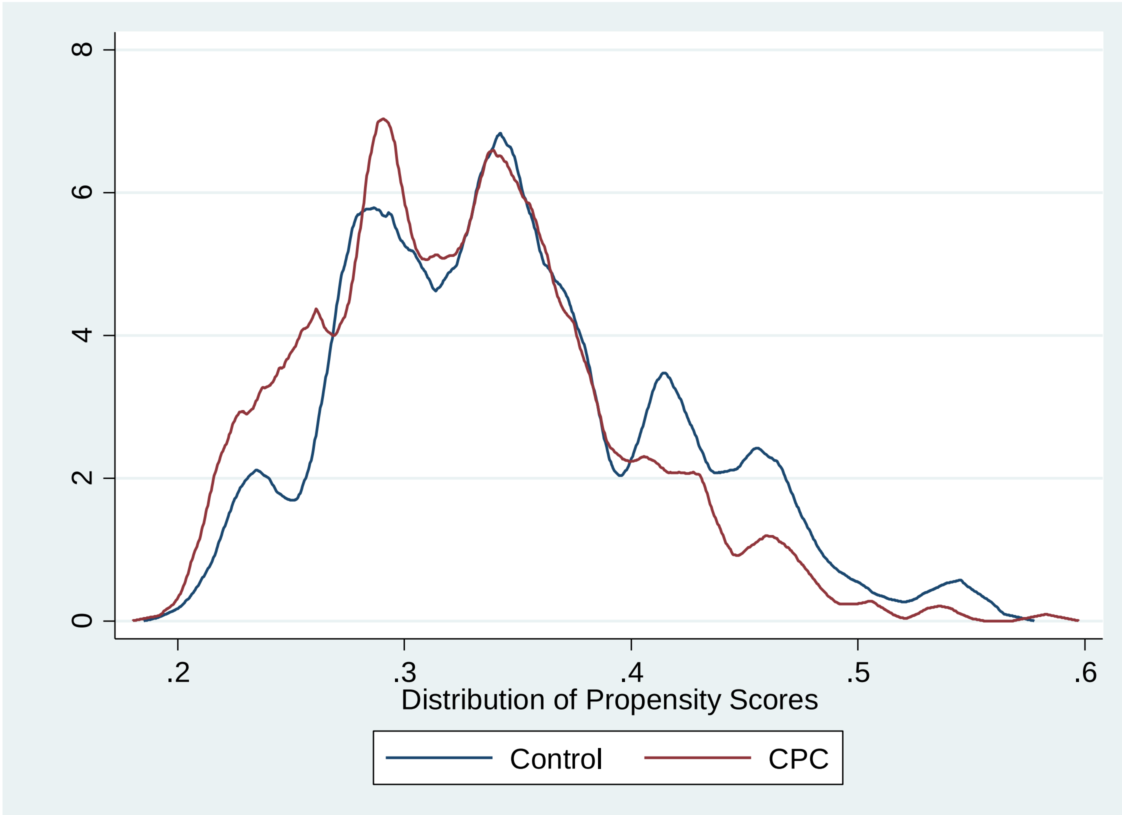


Figure 4.4: Distribution of Propensity Scores (8th Grade site)



CHAPTER 5: BIBLIOGRAPHY

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies*, *72*(1), 1-19.

Aliprantis, D., & Chen, A. (2016). The Consequences of Exposure to Violence during Early Childhood. *Economic Commentary*, (May).

Ander, R., Cook, P., Ludwig, J., & Pollack, H. (2009). Gun violence among school-age youth in Chicago. *Chicago, IL: The University of Chicago Crime Lab*.

Austin, P. C., & Stuart, E. A. (2015). Moving towards best practice when using inverse probability of treatment weighting (IPTW) using the propensity score to estimate causal treatment effects in observational studies. *Statistics in medicine*, *34*(28), 3661-3679.

Arteaga, Irma (2010) Three essays on the economics of child development. Doctoral dissertation, Department of Applied Economics, University of Minnesota.

Arteaga, I., Humpage, S., Reynolds, A. J., & Temple, J. A. (2014). One year of preschool or two: Is it important for adult outcomes? *Economics of education review*, *40*, 221-237.

Ballou, Dale, Goldring, E. and Liu, K. (2006). "Magnet Schools and Student Achievement," Working Paper. Vanderbilt University.

Barnett, W. S., Jung, K., Youn, M., & Frede, E. C. (2013). Abbott preschool program longitudinal effects study: Fifth grade follow-up. New Brunswick, NJ: National Institute for Early Education Research, Rutgers-the State University of New Jersey.

Becker, G. S. (1962). Investment in human capital: A theoretical analysis. *The journal of political economy*, 9-49.

Becker, G. S. (1968). Crime and punishment: An economic approach. In *The Economic Dimensions of Crime* (pp. 13-68). Palgrave Macmillan UK.

Blank, Rolf K. (1989). "Educational Effects of Magnet High Schools," Paper presented to the Conference on Choice and Control in American Education. National Center on Effective Secondary Schools.

Booker, Kevin, Sass, Tim, Gill, Brian, Zimmer, Ron. (2011) "The Effects of Charter High Schools on Educational Attainment" Journal of Labor Economics, Vol 29 (2), 377-415.

Bosetti, L. (2004). Determinants of school choice: Understanding how parents choose elementary schools in Alberta. *Journal of Education Policy*, *19*(4), 387-405.

Bowen, N. K., & Bowen, G. L. (1999). Effects of crime and violence in neighborhoods and schools on the school behavior and performance of adolescents. *Journal of Adolescent Research*, *14*(3), 319-342.

Bowen, G. L., Richman, J. M., Brewster, A., & Bowen, N. (1998). Sense of school coherence, perceptions of danger at school, and teacher support among youth at risk of school failure. *Child and adolescent social work journal*, *15*(4), 273-286.

Bumbarger, B., & Perkins, D. (2008). After randomised trials: Issues related to dissemination of evidence-based interventions. *Journal of Children's Services*, 3(2), 55-64.

Burdette, H. L., & Whitaker, R. C. (2005). A national study of neighborhood safety, outdoor play, television viewing, and obesity in preschool children. *Pediatrics*, *116*(3), 657-662.

Caliendo, M., & Kopeinig, S. (2008). Some practical guidance for the implementation of propensity score matching. *Journal of economic surveys*,*22*(1), 31-72.

Camilli, G., Vargas, S., Ryan, S., & Barnett, W. S. (2010). Meta-analysis of the effects of early education interventions on cognitive and social development. *Teachers College Record*, *112*(3), 579-620.

Campbell, F. A., Ramey, C. T., Pungello, E., Sparling, J., & Miller-Johnson, S. (2002). Early childhood education: Young adult outcomes from the Abecedarian Project. *Applied Developmental Science*, *6*(1), 42-57.

Card, D and Krueger, A.B. (1992). "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States" Journal of Political Economy, 100, 1-40.

Card, D and Krueger, A.B. (1996). "Labor Market Effects of School Quality: Theory and Evidence" Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success. Brookings Institution Press, Washington D.C.

Caughy, M. O. B., Hayslett-McCall, K. L., & O'campo, P. J. (2007). No neighborhood is an island: Incorporating distal neighborhood effects into multilevel studies of child developmental competence. *Health & place*, *13*(4), 788-798.

Cooper, R., Slavin, R. E., & Madden, N. A. (1997). Success for All: Exploring the Technical, Normative, Political, and Socio-Cultural Dimensions of Scaling Up. Report No. 16.

Council of Economic Advisors. (2015) *The Economics of Early Childhood Investments*. Washington, D.C.

Cullen, J. B., Jacob, B. A., & Levitt, S. D. (2005). The impact of school choice on student outcomes: an analysis of the Chicago Public Schools. *Journal of Public Economics*, *89*(5), 729-760.

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

Cullen, J. B., & Levitt, S. D. (1999). Crime, urban flight, and the consequences for cities.

Cunha, F., & Heckman, J. (2007). *The technology of skill formation* (No. w12840). National Bureau of Economic Research.

Currie, J., & Almond, D. (2011). Human capital development before age five. *Handbook of labor economics*, *4*, 1315-1486.

Currie, J., & Hyson, R. (1999). *Is the impact of health shocks cushioned by socioeconomic status? The case of low birthweight* (No. w6999). National bureau of economic research.

Currie, J., & Thomas, D. (1993). *Does Head Start make a difference?* (No. w4406). National Bureau of Economic Research.

Currie, J. and Thomas, D. (2000). "School Quality and the Longer-Term effects of Head Start." Journal of Human Resources, 35, 755-774.

Damm, A. P., & Dustmann, C. (2014). Does growing up in a high crime neighborhood affect youth criminal behavior?. *The American Economic Review*, *104*(6), 1806-1832.

Domitrovich, C. E., & Greenberg, M. T. (2000). The study of implementation: Current findings from effective programs that prevent mental disorders in school-aged children. *Journal of Educational and Psychological Consultation*,*11*(2), 193-221.

Duncan, G. J., & Magnuson, K. (2013). Investing in preschool programs. *The Journal of Economic Perspectives*, *27*(2), 109-132.

Eide, Eric and Mark H. Showalter (2005) "Does improving school quality reduce the probability of unemployment?" Contemporary Economic Policy, vol. 23, pp. 578-584.

Epple, D. N., & Romano, R. (2003). Neighborhood schools, choice, and the distribution of educational benefits. In *The economics of school choice* (pp. 227-286). University of Chicago Press.

Freedman, David and Sekhon Jasjeet (2010). "Endogeneity in Probit Response Models." Political Analysis, Vol 18 (2): 138-150.\

Freeman, R. B. (1999). The economics of crime. *Handbook of labor economics*, *3*, 3529-3571.

Freier, R., Schumann, M., & Siedler, T. (2015). The earnings returns to graduating with honors—Evidence from law graduates. *Labour Economics*, *34*, 39-50.

Fuerst, J.S. (1993) “Chicago experience with an early childhood program: The special case of the Child Parent Center program,” Urban Education, vol.28: 69-96.

Gallego, F. A., Hernando, A. E., Flabbi, L., & Tartari, M. (2008). On the determinants and implications of school choice: Semi-structural simulations for chile [with comments]. *Economía*, 197-244.

Gamoran, Adam. (1996). “Student Achievement in Public Magnet, Public Comprehensive, and Private City High Schools,” Educational Evaluation and Policy Analysis, *Vol. 18* (1): 1-18.

Garces, E., Thomas, D., & Currie, J. (2002). Longer-term effects of Head Start. *The American Economic Review*, *92*(4), 999-1012.

Gaylor, E., Kutaka, T., Ferguson, K., et al. (2016, April). *Evaluation of Kindergarten Readiness in Five Child-Parent Centers*. Report for 2014-15. Chicago: Illinois Facilities Fund.

Glazerman, S. M. (1998). School Quality and Social Stratification: The Determinants and Consequences of Parental School Choice.

Gennetian, L. A., Sanbonmatsu, L., Katz, L. F., Kling, J. R., Sciandra, M., Ludwig, J. & Kessler, R. C. (2012). The long-term effects of Moving to Opportunity on youth outcomes. *Cityscape*, 137-167.

Gershenson, S., & Tekin, E. (2017). The effect of community traumatic events on student achievement: Evidence from the beltway sniper attacks. *Education Finance and Policy*.

Gilliam, W. S., & Zigler, E. F. (2001). A critical meta-analysis of all evaluations of state-funded preschool from 1977 to 1998: Implications for policy, service delivery and program evaluation. *Early Childhood Research Quarterly*, *15*(4), 441-473.

Gormley Jr, W. T., Gayer, T., Phillips, D., & Dawson, B. (2005). The effects of universal pre-K on cognitive development. *Developmental psychology*, *41*(6), 872.

Greenberg, M. T. (2010). School-based prevention: current status and future challenges. *Effective Education*, *2*(1), 27-52.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political economy*, *80*(2), 223-255.

Grossman, M. (2000). The human capital model. *Handbook of health economics*, *1*, 347-408.

Hainmueller, J. (2012). Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis*, *20*(1), 25-46.

Hainmueller, J., & Xu, Y. (2013). Ebalance: A Stata package for entropy balancing.

Halpern, R. (1990). Poverty and early childhood parenting: Toward a framework for intervention. *American Journal of Orthopsychiatry*, *60*(1), 6.

Hanushek, Eric. (2006). "School Resources," Handbook of the Economics of Education, Volume 2.

Hanushek, E. A., Lavy, V., & Hitomi, K. (2006). *Do students care about school quality? Determinants of dropout behavior in developing countries* (No. w12737). National Bureau of Economic Research.

Hayakawa, M., Englund, M. M., Candee, A., Lease, E., Sullivan, M., Warner-Richter, M., & Reynolds, A. J. (2015). Effective Partnerships in School Reform: Lessons Learned from the Midwest Child-Parent Center Expansion. *Journal of Education for Students Placed at Risk (JESPAR)*,*20*(4), 263-280.

Hayakawa, M., Englund, M. M., Warner-Richter, M. N., & Reynolds, A. J. (2013). The longitudinal process of early parent involvement on student achievement: A path analysis. *NHSA dialog*, *16*(1), 103.

Heckman, J. J. (2006). Skill formation and the economics of investing in disadvantaged children. *Science*, *312*(5782), 1900-1902.

Heckman, J. J., Ichimura, H., & Todd, P. E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The review of economic studies*, *64*(4), 605-654.

Heckman, J., & Navarro-Lozano, S. (2004). Using matching, instrumental variables, and control functions to estimate economic choice models. *Review of Economics and statistics*, *86*(1), 30-57.

Heckman, J. J., Moon, S. H., Pinto, R., Savelyev, P. A., & Yavitz, A. (2010). The rate of return to the High Scope Perry Preschool Program. *Journal of Public Economics*, *94*(1), 114-128.

Human Capital Research Collaborative (2016). *CPC P-3 Program Manual*. Minneapolis, Minnesota: University of Minnesota.

Johnson, R. C., & Jackson, C. K. (2017). *Reducing Inequality Through Dynamic Complementarity: Evidence from Head Start and Public School Spending* (No. w23489). National Bureau of Economic Research.

Joseph G. E., McCutchen, D., et al. (2013). Inter-rater Reliability and Concurrent Validity Study of the Washington Kindergarten Inventory of Developing Skills (WaKIDS). Center for Research and Professional Development, University of Washington, Seattle, WA. http://depts.washington.edu/cqel/IRCV.php

Karoly, L. A., & Auger, A. (2016). *Informing investments in preschool quality and access in Cincinnati: Evidence of impacts and economic returns from national, state, and local preschool programs*. Santa Monica, CA: RAND.

Kelly, M. (2000). Inequality and crime. *Review of economics and Statistics*, *82*(4), 530-539.

Kim D., Lambert R. G., & Burts, D. C. (2013). Evidence of the Validity of Teaching Strategies GOLD Assessment Tool for English Language Learners and Children with Disabilities. *Early Education and Development.* 24(4): 574-595.

Kim, D. H., Lambert, R. G., & Burts, D. C. (2013). Validating a developmental scale for young children using the Rasch model: applicability of the teaching strategies GOLD assessment system. *Journal of applied measurement*, *15*(4), 405-421.

Kling, J. R., Ludwig, J., & Katz, L. F. (2005). Neighborhood effects on crime for female and male youth: Evidence from a randomized housing voucher experiment. *The Quarterly Journal of Economics*, *120*(1), 87-130.

Lambert, R, Kim, D, & Burts, D. *Technical Manual for the Teaching Strategies Gold Assessment System*. 2nd ed. Charlotte: University of North Carolina Center for Educational Measurement & Evaluation; 2013a.

Lambert, R., Kim, D., & Burts, D. (2013). Evidence for the association between scores from the Teaching Strategies Gold Assessment System and Information from direct assessments of child progress. *Manuscript under review*.

Lambert, R. G., Kim, D. H., & Burts, D. C. (2014). Using teacher ratings to track the growth and development of young children using the Teaching Strategies GOLD® assessment system. *Journal of Psychoeducational Assessment*, *32*(1), 27-39.

Lee, Valerie E and Loeb, Susanna, (1995). "Where Do Head Start Attendees End up? One Reason Why Preschool Effects Fade out." Educational Evaluation and Policy Analysis, Vol. 17 (1), 62-82.

Lauter, N., & Rice, C. (2008). Scaling Up Quality in Early Childhood Programs: New Jersey's Story. *The New Educator*, *4*(1), 64-82.

Lipsey, M. W., Hofer, K. G., Dong, N., Farran, D. C., & Bilbrey, C. (2013). Evaluation of the Tennessee voluntary prekindergarten program: Kindergarten and first grade follow-up results from the randomized control design. *Nashville, TN: Peabody Research Institute*.

Lochner, L., & Moretti, E. (2004). The effect of education on crime: Evidence from prison inmates, arrests, and self-reports. *The American Economic Review*, *94*(1), 155-189.

Long, J. E., & Toma, E. F. (1988). The determinants of private school attendance, 1970-1980. *The Review of Economics and Statistics*, 351-357.

Nechyba, T. J. (2000). Mobility, targeting, and private-school vouchers.*American Economic Review*, 130-146.

Nechyba, T. J. (2003). Introducing school choice into multidistrict public school systems. In *The economics of school choice* (pp. 145-194). University of Chicago Press.

Neild, Ruth Curran. (2004). "The Effects of Magnet Schools on Neighborhood High Schools: An Examination of Achievement Among Entering Freshman," Journal of Education for Students Placed at Risk, Vol. 9 (1): 1-21.

Machin, S., Marie, O., & Vujić, S. (2011). The crime reducing effect of education. *The Economic Journal*, *121*(552), 463-484.

Magnuson, K. A., Meyers, M. K., Ruhm, C. J., & Waldfogel, J. (2004). Inequality in preschool education and school readiness. *American Educational Research Journal*, *41*(1), 115-157.

Ou, S. & Reynolds, A. J. (2008). "Predictors of Educational Attainment in the Chicago Longitudinal Study." School Psychology Quarterly, *23*(2), 199-229.

Puma, M., Bell, S., Cook, R., Heid, C., & Lopez, M. (2005). Head Start Impact Study: First Year Findings. *Administration for Children & Families*.

Reynolds, A. J. (1999). Educational success in high-risk settings: Contributions of the Chicago Longitudinal Study. *Journal of School Psychology*. *37* (4), 345-354.

Reynolds, A. J. (2000). *Success in early intervention: The Chicago child parent centers*. U of Nebraska Press.

Reynolds, A. J. Hayakawa, M., Ou, S., Mondi, C. F., Englund, M. M., Candee, A. J. & Smerillo, N. E. (In press) Scaling and Sustaining an Effective Early Childhood Intervention through School-Family-University Collaboration. *Child Development.*

Reynolds, A. J., & Ou, S. R. (2010). Early childhood to young adulthood: An introduction to the special issue. Children and Youth Services Review, 32(8), 1045-1053.

Reynolds, A. J. & Ou, S. (2011). “Paths of effects from preschool to adult well-being: A confirmatory analysis of the Child-Parent Center Program.” Child Development. *82*(2), 555-582.

Reynolds, A. J., Richardson, B. A., Hayakawa, M., Lease, E. M., Warner-Richter, M., Englund, M. M., Ou, S-R & Sullivan, M. (2014). Association of a full-day vs part-day preschool intervention with school readiness, attendance, and parent involvement. *JAMA*, *312*(20), 2126-2134.

Reynolds, A. J., & Temple, J. A. (2008). Cost-effective early childhood development programs from preschool to third grade. *Annual Review of Clinical Psychology, 3*, 109-139.

Reynolds, A. J., Temple, J. A., & Ou, S. R. (2010). Preschool education, educational attainment, and crime prevention: Contributions of cognitive and non-cognitive skills. Children and Youth Services Review, 32(8), 1054-1063.

Reynolds, A. J., Temple, J. A., Robertson, D. L., Mann, E. A. (2001). “Long-term effects of an early childhood intervention on educational achievement and juvenile arrest- A 15-year follow-up of low-income children in public schools.” Journal of American Medical Association, 285 (18), 2339-2346.

Reynolds, A. J., Temple, J. A., Ou, S. R., Arteaga, I. A., & White, B. A. (2011). School-based early childhood education and age-28 well-being: Effects by timing, dosage, and subgroups. *Science*, *333*(6040), 360-364.

Reynolds, A. J., Temple, J. A., Robertson, D. L., & Mann, E. A. (2002). Age 21 cost-benefit analysis of the Title I Chicago Child-Parent Centers. *Educational Evaluation and Policy Analysis, 24*(4) 267-303.

Reynolds, A. J., Temple, J. A., White, B. A., Ou, S. R., & Robertson, D. L. (2011). Age 26 cost–benefit analysis of the child-parent center early education program. *Child development*, *82*(1), 379-404.

Richardson, B. A., Reynolds, A. J., Temple, J., & Smerillo, N. (2017). School readiness in the Midwest child-parent center expansion: A propensity score analysis of year 1 impacts. *Children and Youth Services Review*.

Robins, J. M., & Rotnitzky, A. (1995). Semiparametric efficiency in multivariate regression models with missing data. *Journal of the American Statistical Association*, *90*(429), 122-129.

Rohrbach, L. A., & Dyal, S. R. (2015). Scaling up evidence-based preventive interventions. Prevention science in school settings (pp. 175-197) Springer.

Rosenbaum, P. R., & Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, *70*(1), 41-55.

Schafer, J. L., & Olsen, M. K. (1998). Multiple imputation for missing-data problems: A data analyst's perspective. *Multivariate Behavioral Research, 3*3, 545-571.

Scheinfeld, D. R. (1983). Family relationships and school achievement among boys of lower-income urban Black families. *American Journal of Orthopsychiatry*, *53*(1), 127.

Schweinhart, L. (2007). How to take the High/Scope Perry preschool to scale. Paper presented at the National Invitational Conference of the Early Childhood Research Collaborative.

Schweinhart, L. J., Barnes, H. V., & Weikart, D. P. (1993). *Significant benefits: The High/Scope Perry Preschool study through age 27*. Ypsilanti, MI: High/Scope Educational Research Foundation.

Schweinhart, L. J., & Weikart, D. P. (1997). The High/Scope preschool curriculum comparison study through age 23. *Early childhood research quarterly*, *12*(2), 117-143.

Sharkey, P. (2010). The acute effect of local homicides on children's cognitive performance. *Proceedings of the National Academy of Sciences*, *107*(26), 11733-11738.

Sharkey, P. T., Tirado-Strayer, N., Papachristos, A. V., & Raver, C. C. (2012). The effect of local violence on children's attention and impulse control. *American journal of public health*, *102*(12), 2287-2293.

Seaman, S. R., & White, I. R. (2013). Review of inverse probability weighting for dealing with missing data. *Statistical methods in medical research*, *22*(3), 278-295.

Shore, R. (2009). The case for investing in PreK-3rd education: Challenging myths about school reform. *Policy to Action Brief*, (1).

Snow C. E., & Van Hemel S. B. (Eds.). (2008). Early childhood assessment: Why, what, and how. National Research Council, National Academies Press; Washington

DC.Soderberg, J. S., Stull, S., Cummings, K., Nolen, E., McCutchen, D., & Joseph, G. (2013). Inter-rater reliability and concurrent validity study of the Washington Kindergarten Inventory of Developing Skills (WaKIDS). *Unpublished report prepared for the State of Washington Office of Superintendent of Public Instruction*.

Soderberg, J. S., Stull, S., Cummings, K., Nolen, E., McCutchen, D., & Joseph, G. (2013). Inter-rater reliability and concurrent validity study of the Washington Kindergarten Inventory of Developing Skills (WaKIDS). *Unpublished report prepared for the State of Washington Office of Superintendent of Public Instruction*.

Speakman, R., & Welch, F. (2006). Using wages to infer school quality. *Handbook of the Economics of Education*, *2*, 813-864.

Temple, J. & Reynolds, A.J. (2015) Using cost-benefit analysis to scale up early childhood programs through Pay for Success financing. *Journal of Benefit-Cost Analysis,* 6, 628-653.

Temple, J. A. & Reynolds, A. J. (2007). "Benefits and costs of investments in preschool education: Evidence from the Child-Parent Centers and related programs." Economics of Education Review, 26 (1), 126-144

Takanishi, R. (2011) "Transforming America's primary education system for the 21st century: integrating K-12 education with prekindergarten," in Zigler, Giliam, and Barnett, eds. The Pre-K Debates. Brookes Publishing.

Teaching Strategies, Inc. (2011). *Teaching Strategies GOLD Assessment System: Technical summary*. Summary Findings of a Study Conducted by The Center for

Educational Measurement & Evaluation, University of North Carolina at Charlotte.

Weiland, C., & Yoshikawa, H. (2013). Impacts of a prekindergarten program on children's mathematics, language, literacy, executive function, and emotional skills. *Child Development*, *84*(6), 2112-2130.

Weir, L. A., Etelson, D., & Brand, D. A. (2006). Parents' perceptions of neighborhood safety and children's physical activity. *Preventive medicine*, *43*(3), 212-217.

Wong, V. C., Cook, T. D., Barnett, W. S., & Jung, K. (2008). An effectiveness-based evaluation of five state pre-kindergarten programs. *Journal of policy Analysis and management*, *27*(1), 122-154.

Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data*. MIT press.

Wooldridge, J. M. (2007). Inverse probability weighted estimation for general missing data problems. *Journal of Econometrics*, *141*(2), 1281-1301.

Yoshikawa, H., Weiland, C., & Brooks-Gunn, J. (2016). When Does Preschool Matter?. *The Future of Children*, *26*(2), 21-35.

CHAPTER 6: APPENDICES

Appendix A: Appendix A presents additional results from Chapter 2 of this dissertation emphasizing robustness of the estimates using various, alternative estimation techniques. These estimates affirm the results presented in Chapter 2.

Table A.1: Baseline Characteristics of CPC students by full-day classroom

Child/Family Characteristics**	CPC Full-Day (N=409)	CPC Part-Day (N=1,315)	*p*-value
Female child, %	52.8	51.2	0.28
Black, %	88.8	56.4	0.00
Hispanic, %	7.6	42.4	0.00
Home language is Spanish, %	3.7	34.5	0.00
School-level proficiency at state assessment (grades 3-8; %)	55.5	64.5	0.00
Age in months on Sept. 1, 2012 (mean)	51.6	47.4	0.00
Enrolled as 3-year-olds on Sept. 1, 2012, %	14.2	48.6	0.00
Special education status (IEP), %	4.7	11.2	0.00
Child eligible for fully subsidized meals, % [a]	89.7	84.1	0.00
Single parent family status, %	65.1	43.9	0.00
Fall score on Literacy subscale, mean (SD)	42.1	33.0	0.00
Fall score on Math subscale, mean (SD)	26.7	22.4	0.00
School readiness, Fall total scale (SD)	220.8	187.1	0.00

Table A.2: Baseline Characteristics of CPC students by new or existing CPC at the start of the MCPC Expansion.

Child/Family Characteristics**	New CPC (n=718)	Existing CPC (n=1,006)	*p*-value
Female child, %	49.6	53.0	0.08
Black, %	21.2	94.7	0.00
Hispanic, %	73.8	5.8	0.00
Home language is Spanish, %	62.7	1.9	0.00
School-level proficiency at state assessment (grades 3-8; %)	71.9	55.6	0.00
Age in months on Sept. 1, 2012 (mean)	48.5	48.3	0.29
Enrolled as 3-year-olds on Sept. 1, 2012, %	39.0	41.5	0.15
Special education status (IEP), %	17.5	4.0	0.00
Child eligible for fully subsidized meals, % [a]	72.4	94.7	0.00
Single parent family status, %	26.6	77.1	0.00
Fall score on Literacy subscale, mean (SD)	37.3	35.1	0.04
Fall score on Math subscale, mean (SD)	24.0	23.5	0.22
School readiness, Fall total scale (SD)	206.5	195.9	0.04

Table A.3: *Adjusted, Weighted impacts of CPC using three different CPC participation prediction models*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC vs None,	3.4***	8.5***	5.9***	15.4***	3.8***	9.1***	0.7***	20.7***	9.1***
Model 1	(0.311)	(0.0230)	(0.711)	(0.0282)	(0.354)	(0.0255	(0.120)	(1.954)	(0.0279)
CPC vs None,	3.4***	8.2***	6.1***	13.0***	3.6***	7.4***	0.7***	20.8***	8.4***
Model 2	(0.306)	(0.0233)	(0.730)	(0.0272)	(0.368)	(0.0237)	(0.118)	(2.035)	(0.0274)
CPC vs None,	3.3***	8.3***	6.1***	13.3***	3.6***	7.3***	0.8***	20.6***	8.5***
Model 3	(0.303)	(0.0233)	(0.742)	(0.0271)	(0.368\4)	(0.0236)	(0.115)	(2.034)	(0.0276)
Observations	1,531	1,531	1,496	1,496	1,571	1,571	1,582	1,446	1,446

Model 1 predicted CPC participation using only demographic admin data, including race, gender, special education status and free lunch eligibility. Model 2 was used in the main body of the analysis and included the same variables as Model 1, as well as parent survey information including Mother's education, single parent and employment status as well as imputed fall TSGOLD scores for each domain. A missing variable dummy was included for fall test scores and parent survey information that had to be filled via imputation. Model 3 included all the previous covariates and a measure of 3rd grade reading achievement at the school level.
*** p<0.01, ** p<0.05, * p<0.1

*Appendix A.4**:** Non-imputed versus Imputed, mean differences in Spring TSGOLD scores*

VARIABLES	(1) Non-imputed Sample (n=1,446 – 1,582)	(2) Imputed Sample Control (n=1,873)	(3) p-value
Math	36.32	36.36	0.9108
Literacy	57.14	57.26	0.8485
Socio-emotional	55.46	55.39	0.8686
Science	7.75	7.76	0.8903
Total	276.72	277.95	0.5676

Table A.4: *Weighted, Adjusted impacts of CPC on school readiness: Imputed Model*

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC vs None	3.6***	11.9***	6.4***	18.4***	4.0***	16.6***	0.7***	22.2***	13.0***
	(0.242)	(0.0248)	(0.543)	(0.0250)	(0.292)	(0.0269)	(0.0969)	(1.472)	(0.0247)
Observations	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Weighted, Adjusted Impacts of Full- and Part-Day CPC Participation, Imputed model

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotion al	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC Part-Day vs Control	2.9***	9.3***	5.2***	17.0***	3.4***	15.8***	0.5***	18.2***	11.2***
	(0.249)	(0.0261)	(0.559)	(0.0260)	(0.315)	(0.0277)	(0.106)	(1.530)	(0.0259)
CPC Full-Day vs CPC Part-Day	2.4***	11.2***	4.8***	6.3	1.9**	-5.8	0.4*	13.1***	8.8**
	(0.625)	(0.0427)	(1.348)	(0.0395)	(0.827)	(0.0514)	(0.161)	(3.212)	(0.0435)
Observations	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, free lunch eligibility, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Weighted, Adjusted Impacts of CPC participation by free lunch eligibility, imputed Model

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC free/reduced lunch vs Control free/reduced lunch (n = 1,660)	3.86***	11.0***	7.30***	17.1***	4.134***	16.0***	0.638***	24.1***	10.7***
	(0.262)	(0.0274)	(0.594)	(0.0275)	(0.315)	(0.0302)	(0.104)	(1.585)	(0.0270)
CPC non reduced lunch vs control non reduced (n=213)	2.31***	21.5***	1.80	35.9***	3.996***	34.2***	1.413***	15.0***	37.9***
	(0.736)	(0.070)	(1.433)	(0.0763)	(0.639)	(0.068)	(0.250)	(3.337)	(0.0719)
Observations	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Weighted, Adjusted Impacts of CPC by language spoken at home, imputed model

VARIABLES	(1) Math	(2) % at national norm	(3) Literacy	(4) % at national norm	(5) Socio-emotional	(6) % at national norm	(7) Science	(8) Total	(9) % at national norm
CPC Spanish at home vs Control Spanish at home Lang (n=622)	3.839***	26.3***	3.91***	29.1***	5.55***	34.7***	1.473***	23.32***	35.5***
	(0.571)	(0.0436)	(0.956)	(0.0424)	(0.647)	(0.0424)	(0.177)	(2.906)	(0.0400)
CPC all other languages vs control all other lagnuages (n=1,251)	3.52***	5.63*	6.81***	12.4***	3.34***	2.82	0.486***	20.0***	0.53
	(0.289)	(0.0312)	(0.676)	(0.0315)	(0.372)	(0.0305)	(0.119)	(1.845)	(0.0287)
Observations	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873	1,873

Notes: Propensity score model controls for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status and an indicator if the family did not complete the parent survey. The propensity score weighted outcome model controlled for race, gender, special education status, age of the student in months, Mother's education and employment status, single parent status, an indicator if the family did not complete the parent survey, 3rd grade school level reading scores, fall baseline TSGOLD score and the month the student was assessed in the fall. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix B: Appendix B provides supplemental material for Chapter 3. This includes additional information on the construction and demographic information on the crime variable, as well as additional estimation models to analyze the impacts of neighborhood crime on preschool student achievement, including results from different weighting models. Models presented in this appendix weight for fall test score, median neighborhood income, age, CPC status, full-day status, race, gender, free lunch status and special education status.

Table B.1: *Factor Analysis of Individual Crime types mapped to two factors.*

Pattern Matrix[a]

	Factor	
	1	2
ASSAULT	.903	.119
BATTERY	.851	.239
ARSON	.273	.590
BURGLARY	.344	.654
CRIM SEXUAL ASSAULT	.778	.023
CRIMINAL DAMAGE	.480	.610
CRIMINAL TRESPASS	.508	-.205
HOMICIDE	.454	.087
KIDNAPPING	-.138	.723
MOTOR VEHICLE THEFT	.109	.671
NARCOTICS	.720	.202
OFFENSE INVOLVING CHILDREN	.831	.046
OTHER NARCOTIC VIOLATION	-.037	-.178
PROSTITUTION	.365	.036
ROBBERY	.799	.129
SEX OFFENSE	.037	.569
THEFT	.633	.022
WEAPONS VIOLATION	.697	.327

Extraction Method: Maximum Likelihood.

Rotation Method: Oblimin with Kaiser Normalization.

a. Rotation converged in 7 iterations.

Table B.2: Weighted, Difference in Difference impacts of crime on student achievement, Control Students only

VARIABLES	(1) Literature	(2) Math	(3) Cognitive	(4) Physical	(5) Attendance
Crime Dummy	-36.91**	-6.946	-3.192*	-2.620	0.0889**
	(13.25)	(10.11)	(1.556)	(2.521)	(0.0273)
Spring Time Dummy	20.72***	11.27***	14.15***	7.606***	-0.0277
	(0.667)	(0.605)	(1.176)	(0.748)	(0.0203)
DD Estimate	-12.29***	-5.368***	-12.44***	-0.749	-0.0478
	(0.667)	(0.605)	(1.176)	(3.981)	(0.0512)
Neighborhood Household Median Income	-0.00142**	-0.000370	-0.000123**	-8.57e-05**	1.03e-07
	(0.000395)	(0.000297)	(3.32e-05)	(3.45e-05)	(8.32e-07)
Months Present	9.327***	3.279***	1.651	0.141	0.0352
	(1.269)	(0.393)	(1.888)	(0.564)	(0.0273)
School level achievement	-1.407**	-0.345	-0.300***	-0.111***	0.00136
	(0.423)	(0.334)	(0.0257)	(0.0199)	(0.000891)
Fall Assessment Date	-5.296***	-2.255*	-3.579***	-2.360**	0.0779**
	(1.011)	(1.094)	(0.803)	(0.731)	(0.0281)
Black	15.84**	8.906	10.61*	4.000**	-0.0252
	(4.863)	(4.539)	(5.208)	(1.170)	(0.0181)
Hispanic	14.25***	8.950*	9.176*	3.706***	0.0242
	(3.143)	(3.655)	(3.971)	(0.761)	(0.0228)
Female	3.411***	1.893***	1.930	0.646	0.0210
	(0.719)	(0.356)	(1.313)	(0.374)	(0.0269)
Special Education Status	-20.50***	-9.420***	-12.57***	-5.030***	0.0113
	(3.317)	(1.373)	(1.497)	(0.634)	(0.0114)
Age in Months	1.614***	0.852***	0.998***	0.453***	-0.00181*
	(0.0335)	(0.0572)	(0.0996)	(0.0375)	(0.000802)
Free lunch eligible	2.477	-1.141**	0.0343	-1.883**	-0.0271
	(2.337)	(0.339)	(1.325)	(0.587)	(0.0288)
Mother HS Grad	4.692***	1.529	1.856**	0.188	0.0285
	(1.115)	(0.813)	(0.758)	(0.854)	(0.0201)
Mother Employed	-0.00486	-1.104	-1.028	-0.591	-0.00896
	(4.515)	(2.039)	(2.055)	(0.761)	(0.00931)
Missing Parent Survey	-0.857	-0.658*	-0.107	0.477	-0.0494*
	(1.583)	(0.258)	(0.559)	(0.514)	(0.0236)
Single Parent Status	0.565	0.257	-1.184	-0.164	0.00218
	(1.624)	(1.369)	(1.274)	(0.233)	(0.00578)
Constant	-7.326	-20.62	-7.299	12.40**	0.514
	(40.56)	(37.23)	(15.92)	(4.424)	(0.282)
Observations	349	372	381	426	422
R-squared	0.718	0.720	0.710	0.655	0.226

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.

Table B.3: Weighted, Difference in Difference impacts of crime on student achievement, CPC Students only

VARIABLES	(1) Lit	(2) Math	(3) Cognitive	(4) Physical	(5) Attendance
Crime Dummy	-2.041	-0.521	-1.637	-0.128	0.00164
	(2.371)	(1.222)	(1.986)	(0.999)	(0.00973)
Spring Dummy	26.61***	15.34***	17.70***	9.151***	-0.101***
	(2.269)	(0.983)	(1.109)	(0.717)	(0.00929)
DD estimate	2.321	3.94e-05	1.070	-0.818	-0.00784
	(3.531)	(1.548)	(1.646)	(0.840)	(0.0189)
Median Household Income	-2.53e-05	-2.60e-05	-7.83e-07	2.29e-05	1.35e-08
	(9.29e-05)	(3.89e-05)	(5.31e-05)	(6.31e-05)	(2.78e-07)
Full-Day	-1.010	1.174	0.861	0.157	0.0591***
	(3.682)	(1.873)	(2.057)	(1.451)	(0.0103)
Months Present	0.146	-0.549	2.751**	0.833**	0.00152
	(1.729)	(0.817)	(1.189)	(0.325)	(0.00740)
3rd Grade reading achievement	-0.106*	-0.0389	-0.0254	0.0191	0.00112***
	(0.0596)	(0.0303)	(0.0635)	(0.0317)	(0.000195)
Fall assessment	6.766*	3.707**	6.928***	3.155***	0.0112
	(3.461)	(1.343)	(1.487)	(0.966)	(0.0107)
Black	7.899*	1.621	2.884	0.396	-0.0643**
	(4.252)	(1.869)	(3.542)	(2.118)	(0.0251)
Hispanic	7.345	5.121**	5.620	1.630	-0.0440
	(5.006)	(2.239)	(3.299)	(1.857)	(0.0371)
Female	2.353*	1.094**	1.034**	0.473***	0.00101
	(1.142)	(0.398)	(0.472)	(0.157)	(0.00514)
Special Education Status	-7.742	-4.483	-6.844**	-3.297**	0.0129
	(4.721)	(2.587)	(3.065)	(1.427)	(0.0294)
Age in Months	1.534***	0.742***	0.974***	0.462***	0.000395
	(0.0918)	(0.0882)	(0.107)	(0.0614)	(0.000772)
Free lunch status	-6.105**	-2.950***	1.454	1.567	0.00496
	(2.050)	(0.732)	(1.550)	(1.220)	(0.0325)
Mother HS grad	3.638	1.572	1.004	0.578	0.0417**
	(2.269)	(1.417)	(1.419)	(0.593)	(0.0179)
Mother employed	-2.009	-1.317**	-2.280*	-1.068**	-0.0109
	(1.429)	(0.574)	(1.239)	(0.484)	(0.00635)
Missing Parent Survey	0.112	0.185	0.686	0.498	-0.0305**
	(1.782)	(0.824)	(1.380)	(0.651)	(0.0108)
Single Parent Status	1.393	0.658	1.947	0.382	0.0123
	(1.456)	(0.629)	(1.384)	(0.486)	(0.0114)
Constant	-44.82*	-6.631	-43.52**	-11.33*	0.830***
	(24.49)	(10.72)	(15.33)	(5.922)	(0.132)
Observations	780	792	829	941	943
R-squared	0.650	0.643	0.619	0.600	0.203

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.4: Weighted, Difference in Difference impacts of crime on student achievement, Control Students only

VARIABLES	(1) Literature	(2) Math	(3) Cognitive	(4) Physical	(5) Attendance
Crime Dummy	-28.16*	-3.013	-0.458	-1.605	0.0852***
	(12.03)	(5.941)	(1.872)	(2.326)	(0.0240)
Spring Time Dummy	21.50***	11.69***	16.23***	8.539***	-0.0143
	(0.702)	(0.744)	(2.357)	(0.981)	(0.0237)
DD Estimate	-13.08***	-5.787***	-14.52***	-1.682	-0.0608
	(0.702)	(0.744)	(2.357)	(4.031)	(0.0527)
Neighborhood Household Median Income	-0.00119**	-0.000242	-6.37e-05**	-3.83e-05	-4.19e-07
	(0.000354)	(0.000168)	(2.21e-05)	(3.26e-05)	(1.03e-06)
Months Present	7.659***	3.216***	-0.443	-0.276	0.0416
	(1.857)	(0.377)	(1.315)	(0.264)	(0.0326)
School level achievement	-1.168**	-0.210	-0.296***	-0.0920***	0.00156
	(0.367)	(0.195)	(0.0263)	(0.0166)	(0.000902)
Fall Assessment Date	-4.222**	-2.049*	-2.154***	-1.962**	0.0763**
	(1.059)	(0.811)	(0.420)	(0.597)	(0.0288)
Black	16.67**	7.392	8.044	3.067**	-0.0148
	(4.220)	(4.428)	(5.472)	(0.888)	(0.0135)
Hispanic	13.10***	6.802*	6.934	3.175***	0.0353*
	(3.197)	(3.245)	(4.235)	(0.649)	(0.0162)
Female	3.644***	2.112***	1.350	0.186	-0.00483
	(0.592)	(0.487)	(1.410)	(0.445)	(0.0102)
Special Education Status	-17.76***	-7.479***	-10.87***	-4.715***	-0.00246
	(2.460)	(1.145)	(1.666)	(0.759)	(0.00790)
Age in Months	1.622***	0.886***	1.050***	0.452***	-0.00262*
	(0.0399)	(0.0899)	(0.126)	(0.0449)	(0.00123)
Free lunch eligible	0.609	-1.967***	0.958	-2.085**	-0.0235
	(1.651)	(0.469)	(1.713)	(0.803)	(0.0276)
Mother HS Grad	4.142***	1.457	1.509**	0.302	0.0305
	(0.854)	(0.842)	(0.473)	(0.714)	(0.0176)
Mother Employed	2.241	-0.0587	-1.034	0.0381	-0.0207*
	(3.547)	(1.633)	(2.009)	(0.392)	(0.00904)
Missing Parent Survey	-1.357	-0.693**	0.0660	-0.101	-0.0643
	(1.501)	(0.227)	(0.350)	(0.709)	(0.0383)
Single Parent Status	-0.0259	0.0318	-1.013	0.0250	-0.000399
	(1.249)	(1.221)	(1.582)	(0.227)	(0.00502)
Constant	-17.49	-34.98	10.12	14.21***	0.515
	(30.35)	(26.01)	(15.54)	(2.103)	(0.308)
Observations	349	372	381	426	422
R-squared	0.708	0.709	0.749	0.703	0.274

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.5: Weighted, Difference in Difference impacts of crime on student achievement, CPC Students only

VARIABLES	(1) Literature	(2) Math	(3) Cognitive	(4) Physical	(5) Attendance
Crime Dummy	-0.929	-0.380	-1.548	-0.346	-0.0114
	(2.395)	(1.123)	(1.941)	(0.947)	(0.0105)
Spring Dummy	26.24***	15.83***	17.36***	9.377***	-0.109***
	(3.235)	(1.026)	(1.353)	(0.603)	(0.00969)
DD estimate	2.693	-0.491	1.402	-1.044	8.03e-05
	(4.217)	(1.576)	(1.818)	(0.747)	(0.0192)
Median Household Income	2.61e-07	-1.78e-05	-5.94e-06	4.98e-05	1.99e-07
	(0.000176)	(8.19e-05)	(6.76e-05)	(0.000112)	(4.90e-07)
Full-Day	-2.710	1.147	0.840	0.426	0.0593***
	(5.078)	(2.379)	(2.161)	(1.965)	(0.0112)
Months Present	0.154	-0.767	3.066**	0.600*	-0.00155
	(1.876)	(0.921)	(1.206)	(0.330)	(0.00745)
3rd Grade reading achievement	-0.213***	-0.0655**	-0.0584	-0.00565	0.00117***
	(0.0642)	(0.0297)	(0.0516)	(0.0325)	(0.000253)
Fall assessment	7.825**	3.654**	7.639***	3.005***	0.00779
	(3.076)	(1.243)	(1.083)	(0.875)	(0.0102)
Black	6.670*	1.722	2.999	-0.194	-0.0676**
	(3.639)	(1.442)	(3.405)	(2.417)	(0.0253)
Hispanic	8.695**	5.990***	6.334**	1.796	-0.0467
	(3.607)	(1.368)	(2.853)	(1.920)	(0.0391)
Female	2.573*	1.382***	1.040**	0.392**	0.00292
	(1.319)	(0.455)	(0.413)	(0.169)	(0.00626)
Special Education Status	-3.419	-1.806	-6.652*	-2.970**	-0.0108
	(3.882)	(1.965)	(3.159)	(1.343)	(0.0373)
Age in Months	1.409***	0.694***	0.967***	0.435***	0.000131
	(0.146)	(0.109)	(0.102)	(0.0595)	(0.000706)
Free lunch status	-9.057***	-4.090***	0.673	1.353	0.00776
	(2.067)	(0.695)	(1.858)	(0.990)	(0.0294)
Mother HS grad	3.508	2.065	1.101	0.673	0.0280*
	(2.033)	(1.465)	(1.437)	(0.541)	(0.0156)
Mother employed	-2.587	-1.578*	-2.846*	-1.018*	-0.0149***
	(1.576)	(0.860)	(1.505)	(0.515)	(0.00480)
Missing Parent Survey	2.325	0.820	1.345	0.617	-0.0288**
	(2.211)	(1.103)	(1.498)	(0.673)	(0.0102)
Single Parent Status	1.257	0.547	2.146	0.274	0.0137
	(1.604)	(0.633)	(1.473)	(0.470)	(0.0106)
Constant	-32.03	-0.601	-44.52***	-6.382	0.894***
	(32.18)	(12.88)	(12.69)	(6.575)	(0.129)
Observations	780	792	829	941	943
R-squared	0.627	0.626	0.618	0.601	0.204

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1