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One Year of Preschool or Two – Is It Important for Adult Outcomes? Results from the Chicago Longitudinal Study of the Child-Parent Centers

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Abstract

Until the last year, public funding for preschool education had been growing rapidly over a decade with most state programs providing one year of preschool for four year olds. Fewer three year olds are enrolled in preschool. To investigate the importance of enrollment duration, this study is the first to estimate long-term dosage effects of years of preschool. We use data from a cohort of 1,500 students in the Chicago Longitudinal Study who enrolled in the Chicago Public Schools in the mid-1980s. Many of these students participated in a high-quality preschool program called Child-Parent Centers (CPC) for one or two years. To address selection with multiple treatments, we employ inverse propensity score weighting. Relative to children who attended one year of CPC preschool, the two-year group is significantly less likely to receive special education or be abused or neglected or to commit crimes. The findings provide support for the long-term benefits of greater exposure to preschool.

1. Introduction

Numerous studies suggest that investments in early childhood intervention offer high returns to society (Heckman et al., 2010; Reynolds et al., 2011a, 2002; Dynarski et al., 2011; Karoly et al., 2005; Ludwig and Phillips, 2007). Policy-makers allocating scarce resources may ask: Is one year enough to create long-term benefits? What is the impact of a second year of preschool? Several studies of outcomes observed in elementary school find that better outcomes are associated with two years of preschool compared to one at kindergarten entry (Barnett and Lamy, 2006; Loeb et al., 2007; Wen et al. 2012) and by sixth grade (Reynolds, 1995). This paper uses data from the Chicago Longitudinal Study, where many children participated in a high-quality preschool program called Child-Parent Centers (CPC) for one

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or two years. CPC is characterized by an emphasis on parental involvement, education supports such as small class sizes, an aligned curriculum and additional resources, such as provision of health and social services and free meals. We expand on previous findings by estimating the effects of zero, one, or two years of CPC preschool on outcomes from eighth grade into adulthood. Ours appears to be the first study to examine these long-term dosage effects. We address two questions:

1. What is the marginal effect of a second year of CPC preschool (in comparison to one year) on long-term outcomes such as the educational attainment or arrest? What is the marginal effect of a first year of CPC preschool (in comparison to zero years) on long-term outcomes?
2. Is the marginal effect for some subgroups such as boys or higher-risk children in our sample greater than others? If so, for what outcomes and what groups is the marginal effect most important?

The Chicago Longitudinal Study is an ongoing project investigating the effects of the federally-funded Child-Parent Center (CPC) preschool program on the educational and social development of 989 low-income minority children into adulthood and a control group of 550 that did not attend CPC preschools. While previous research has examined test score differences in elementary school resulting from one versus two years of preschool (Reynolds, 1995), in this study we compare a larger set of cognitive and social outcomes observed in eighth grade and twelfth grade, as well as educational, crime and economic outcomes into adulthood. We use propensity score weighting to address the nonrandom assignment of children to zero, one, or two years of preschool.

2. Related literature

Early childhood is recognized as an important period for human capital investments. Numerous studies have suggested that high-quality preschool programs can have strong short and long-term benefits for both preschool participants and society at large (e.g. Camilli et al., 2010; Barnett, Belfield, and Nores, 2005; Heckman, 2006). In the short term, preschool participation has been shown to improve children's cognitive skills as well as health outcomes (Gormley and Gayer, 2007; Magnuson et al., 2007; Currie and Thomas, 1995). The preschool enrollment of peers recently has been shown to positively affect test scores of other classmates, suggesting that some societal benefits of preschool can be observed early in elementary school (Neidell and Waldfogel, 2008). Additionally, preschool is promoted as a cost-effective way to reduce the achievement gap in elementary school and beyond (Karoly et al., 2005).

Notably, influential research on the Abecedarian and Perry Preschool projects has documented preschool's longer-term benefits for disadvantaged youth, including higher rates of high school completion, higher earnings and decreased crime (Campbell et al. 2012; Heckman et al. 2010; Schweinhart et al. 2005). Research on the long-term effects of the CPC program and Head Start have been consistent with these findings (Reynolds et al. 2011b; Deming, 2009; Ludwig and Phillips, 2007; Barnett et al., 2005; Temple and Reynolds, 2007). Recognizing the benefits of investments in early childhood education, the

Obama administration has proposed a federal-state partnership to increase access to preschool programs that meet established quality standards for lower-income families and incentives to states for expanding programs for higher-income families (Obama, 2013).

As public attention devoted to preschool grows, budget constraints generate a tradeoff between offering one year of government-funded preschool at age 4 to a larger number of children, offering two years of preschool or more to a smaller number of children, or dedicating the additional resources necessary to ensure that all children have access to two years of preschool. The recent *State of Preschool* yearbook published by the National Institute of Early Education Research highlights a recently- developing, serious resource problem causing U.S. children to experience the largest single-year reduction in state spending on pre-kindergarten (Barnett et al.; 2012). While in previous years access to preschool was expanding in many states and many states were expanding their programs to cover three as well as four year olds, now there is added urgency for policymakers to consider how to effectively use scarce preschool dollars.

Concern exists that one year may not be enough to achieve meaningful gains in school readiness. For example, Chase et al. (2008) recommended increasing access to two years of preschool in a report that estimated that poor school readiness among Minnesota children increases public K-12 education costs by \$100 million annually. Little research exists, however, on the marginal benefit of a second year of preschool to help guide these investment decisions.

A small but growing body of literature examines the short-term effects of different lengths of exposure to preschool. The small number of studies on preschool dosage are limited to evidence on the short-term effects of one or two years of preschool observed in preschool or kindergarten. Furthermore, many, though not all studies, are based on regression estimates that may be subject to selection bias. Nonetheless, these papers suggest that children with longer exposure to preschool demonstrate advantages over children with shorter exposure, at least in the short term (Loeb et al., 2007; Behrman, Cheng and Todd, 2004; Skibbe, 2011; Barnett and Lamy, 2006). These benefits include stronger cognitive skills during preschool or kindergarten (Perez-Escamilla and Pollitt, 1995; Loeb et al., 2007; Skibbe, 2011; and Barnett and Lamy, 2006), improved socioemotional outcomes (Skibbe, 2011; and Behrman, Cheng and Todd, 2004), and physical growth in a preschool program in Colombia that included a strong nutrition component (Perez-Escamilla and Pollitt, 1995). A recent meta-analysis by Nores and Barnett (2010) on the effects of preschool outside the United States found that programs lasting one to three years had average effect sizes of 0.312 standard deviations, as compared to 0.196 for programs lasting less than one year. Participating in preschool for more than three years does not, however, translate to greater gains; the average effect size for programs with this duration is 0.3 (Nores and Barnett, 2010). Similarly, according to recent estimates from the large and rich Early Childhood Longitudinal Survey – Kindergarten Cohort (ECLS-K), children who begin preschool at age two or three have cognitive advantages in kindergarten, but those who begin before age two show lower socioemotional functioning (Loeb et al. 2007).

Evidence on whether these early advantages persist into later elementary school and beyond is limited. In one study that does provide some evidence on outcomes into elementary school, Perez-Escamilla and Pollitt (1995) followed children who were randomly assigned to participate in one, two, three or four years of a preschool program with a nutrition supplement through third grade. They found effects on test scores in kindergarten and on child growth through second grade; however, effects faded by third grade.

Reynolds (1995) investigated preschool's dosage effects through grade 6 in the Chicago Longitudinal Study (CLS), analyzing the difference in outcomes through elementary school for participants with zero, one, or two years of CPC preschool participation. Participants were not randomly assigned to each treatment group; nevertheless, the treatment and comparison groups were similar on a variety of included socioeconomic and demographic characteristics. Controlling for observed student and family characteristics, participants with two years of preschool did significantly better than the one-year group on a variety of indicators in kindergarten and first grade. The advantage of a second year faded in the upper grades, but differences consistently favored the two-year group over the one-year group.

In the current study, we follow Reynolds (1995) in utilizing the Chicago Longitudinal Study to examine preschool dosage effects but we focus more closely on duration selection issues and we have data on longer-term outcomes. Unlike previous studies that focus solely on shorter-term outcomes, we estimate the dosage effects of participation in zero, one or two years of preschool on outcomes into adulthood. Following previous studies by Behrman et al. (2004) and Loeb et al. (2007), we employ propensity score methods that adjust for potential bias due to non-random selection into one or two years of CPC preschool. We utilize the rich array of covariates available on the CLS participants from birth records, school administrative records, surveys and other sources. We employ inverse propensity score weighting rather than propensity score matching to estimate the effects of differential duration of enrollment in preschool. We report results through age 26. While numerous influential papers have reported the long-term benefits of preschool, including work on the Abecedarian Project, the Perry Preschool project and the Chicago Child Parent Centers (see Campbell et al, 2012; Schweinhart et al., 2005; and Reynolds et al., 2011, among others), this is the first study to report the long-term effects of preschool dosage.

3. Methods

3.1. Sample and intervention

The Chicago Longitudinal Study (CLS) follows a single-aged cohort of 1,539 children. The majority of children (n=989) attended kindergarten in Chicago Child-Parent Centers (CPC) sites in 1985-86 and another group of children (n=550) attended kindergartens in similar comparison group schools located in low-income neighborhoods. As described in Temple and Reynolds (2007), since the 1960s the CPC program has been located in high-poverty neighborhoods in Chicago offering high-quality preschool with an emphasis on parental involvement. The educational intervention follows the preschool program with education supports such as small class sizes, an aligned curriculum and additional resources through grades two or three. All children in the study attended schools receiving federal Title 1 funds. The original intent of the Chicago Longitudinal Study was to examine schooling

outcomes for students in two different Title 1 programs serving kindergartners. A treatment group consisting of entire classrooms of students attending the Title 1-funded Child-Parent Centers were compared with a control group of entire classrooms of students in a matched sample of similar non-CPC schools that received Title 1-funded all-day kindergarten.

Schools were matched using school-level data on percent eligible for subsidized school lunch, neighborhood poverty, and race. A number of years later, birth certificate data were obtained on the students in the sample and the additional socio-demographic information verified the validity of the original match. However, because the children in the CPC neighborhoods resided in the highest poverty areas in Chicago, the comparison group members on average did not come from neighborhoods quite as disadvantaged as their peers in the treatment group.

The Chicago Child-Parent Centers (CPC) preschool program provides literacy and parent training for children at risk of school failure. Although a number of CPC programs are full-day now, in the 1980s, the centers offered a half-day preschool program for 3 and 4-year-old children during the 9-month academic year plus a 6-week summer program. Quality is enhanced through the following features: teachers have four-year college degrees, classes have teacher's aides, additional instructional materials and enrichment activities; class sizes are reduced (25 instead of the Chicago Public School average of 35), and parent involvement is a main component. Parents receive assistance to further their own education and participate in home visits and field trips. Additionally, the program provides health and social services and free meals. The CPC program began in 1965 and in 1978 state funding became available to expand the program through third grade.

Table 1 describes participant poverty, kindergarten and school-age CPC participation by preschool dosage groups. Some children that did not attend the CPC-preschool attended other area preschools although these were not prevalent in the mid-1980s. The majority of CPC participants attended sites that did not offer full-day kindergarten, while all children from the comparison group did. CPC preschool participants obviously were more likely to participate in the school-age CPC program than the comparison group.

Group Comparability—Table 2 reports differences in observed characteristics according to preschool duration. Overall, groups were similar on most background variables including home environment, family income as indexed by eligibility for subsidized lunches, single parent family status, and employment. Although mothers of two-year participants were slightly more likely to be high school graduates than the one-year or no-CPC group, college attendance rates were equivalent. In addition to the standard background information on students and families available from school administrative records, as previously mentioned the CLS also includes a rich set of socioeconomic information obtained from student birth records.

3.2. Selection bias in non-randomized experiments

For many questions of interest in child development, randomization is difficult or expensive and often not employed. For example, while the recent Head Start Impact Study includes children who were randomly assigned to Head Start preschool at ages 3 or 4, the formal

evaluation does not include a causal assessment of the effects of differential enrollment in preschool (Department of Health and Human Services, 2010).

Estimating the marginal effect of a second year of preschool was not part of the original evaluation plan because it was not feasible to mandate that the three-year-old participants continue or not for a second year of preschool at age four. A final report on the Head Start Impact Study at the third grade follow up describes the question of “Is there a benefit of having two years of Head Start rather than one year?” to be “one of many important unanswered questions” (Puma et al. 2012, p. xxxvii).

Even when program participation is randomly assigned, researchers often must rely on non-experimental methods to infer the impact of different treatment dosages. Simple comparisons of individuals receiving different treatments are potentially misleading or biased in that they may not reveal the effect of treatment per se. In many cases, these comparisons confound the effect of the treatment with that of the factors that lead individuals to select treatments of different durations.

In the Chicago Longitudinal Study, the students who had *any* participation in the CPC preschool program appear to be well matched with the comparison group individuals who attended kindergartens without a CPC preschool. This matching has been assessed in numerous studies (e.g., Reynolds et al. 2011b; Temple and Reynolds 2007, and Levin, Belfield, Muenning and Rouse, 2007) and it appears that the “any CPC preschool” vs. no CPC preschool groups were fairly well matched. However, the current study compares students who had zero, one, or two years of CPC preschool. There was no attempt at the beginning of the study (late-1980s) to ensure that these comparison groups were well matched. Overall, we find that the students with two years of CPC preschool are similar to students with one year on most observed characteristics, but there are several significant differences: participants with two years are more likely to be black or to have a mother that graduated from high school, and less likely to be born in the fourth quarter, to come from a family with a single parent or to have been in the child welfare system.

A logical concern is that individuals who experienced two years of preschool could be different from individuals who experienced one or zero years. The two-year group could have unobservable characteristics that are associated with better or worse subsequent academic and life outcomes. Due to limited space and budgets, project administrators may have given more disadvantaged children priority over other children for two years of preschool. Alternatively, participation in two years of preschool may be associated with parents who place greater importance on preschool or experience less geographic mobility. It might also be the case that variation in preschool duration arises from state-mandated cutoff dates that require children to have reached their third birthday before a specific day to be eligible to begin school each fall. If that is the case children who were born just after the cutoff date (usually on the fourth quarter) may be more likely participate in just one year of preschool and less likely to participate in two years of preschool.

3.3. Methodology

In the absence of suitable instruments for preschool duration that can predict zero, one and two years of enrollment, we use a non-instrumental variable estimation approach based on propensity scores to control for selection. We found that in some cases the set of quarter variables is jointly significant in the outcome equations. Concern about the appropriateness of using quarter of birth in instrumental variable analyses has been expressed by a number of researchers including Cascio and Lewis (2006). A benefit of propensity score methods is that they do not require exclusion restrictions.

Propensity score methods employ a predicted probability of group membership (e.g. CPC preschool or not) or group statuses (e.g. zero years, one year, two years of CPC preschool participation) based on observed predictors. By using the probability that an individual would have been treated or would have been assigned to a treatment category to adjust the estimate of the treatment effect, a researcher creates a “quasi-randomized” experiment (Rosenbaum & Rubin, 1983). This method relies on the basic assumption of unconfoundedness. This assumption states that the outcome and treatment are conditionally independent given a set of pre-treatment covariates. Satisfying this assumption generally requires a rich set of pre-treatment covariates to predict treatment statuses (e.g. years of CPC preschool participation). According to Hirano and Imbens (2001) it is reasonable to believe that the unconfoundedness assumption may be satisfied in some empirical studies where a rich set of pre-treatment covariates has been gathered. The Chicago Longitudinal Study has a rich set of covariates, drawn from birth records, education, crime and employment data from administrative sources, as well as self-reported data from parents, participants, teachers and principals over the years. These data include a large set of covariates that characterize the participant, his family and surrounding environment prior to, during the treatment and after the treatment.

Recent work in program or policy evaluation (e.g., Reynolds et al. 2011b; Peterson et al. 2006; Rotnitzk & Robins, 2005; Foster, 2003; Imbens & Ridder, 2003) uses propensity score weighting instead of propensity score matching. Propensity score matching is a popular method in part because results are very intuitive: one can match cases in the treatment group with similar cases in the comparison group based on the probability of receiving the treatment (e.g. participating in CPC preschool or not). This means that each case in the treatment group has a counterfactual in a comparison individual(s). This permits the unbiased estimation of treatment effects assuming that the unconfoundedness assumption is satisfied. Thus, a main advantage of using propensity score matching is that is very straightforward in interpretation.

However, propensity score matching has several disadvantages. First, it is not always possible to match all treatment cases as satisfying the common support condition becomes more difficult with larger numbers of covariates. This means that a loss of a large number of unmatchable cases could increase the variance of the estimates as the estimation is calculated with a smaller pool of matches. Second, and perhaps most important for the current study, propensity score matching is appropriate for matching a treatment group with a comparison group, but is not as useful for estimating the differential effects of multiple

treatments, as in the analysis of dosage effects (e.g. years of participation on an early childhood program).

Unlike propensity score matching, propensity score weighting is robust to the misspecification of the regression model. As explained by Stuart (2010), additional benefits of propensity score weighting include the absence of a problem with unmatchable cases as it does not rely on a matching scheme where one or more treatment cases are compared to non-treatment cases. Weighting also allows for interactions between treatment and covariates. While Imbens and Wooldridge (2009) and Li et al. (2008) discuss the efficiency properties of propensity score weighting, Stuart (2010) admits that there is little guidance in the literature to help applied researchers select among various propensity score methods.

Our main rationale for choosing propensity score weighting is the usefulness of this method for estimating dosage effects. As dosages in health and education studies may not be simply described as 0 or 1, propensity score methods have been extended to include studies with multiple treatment groups in which participants receive different treatment dosages (e.g. zero, one or two years of CPC preschool). Propensity score weighting is especially useful for studies with multiple treatment dosages where implementation of propensity score matching would be less appropriate. Under the assumption that the researcher can observe important variables that influence the probability of selection, this method estimates the impact of treatment by creating a reweighted data set that better resembles random assignment in which each individual has the same probability of participation.

Propensity score weighting can help correct for selection bias when evaluating a dose-response treatment. This dose-response methodology is explained by Imbens (2002) as one in which adjustments are made for preexisting observed differences among the groups using a propensity score weighting method. In propensity score weighting, individuals are assigned larger (smaller) weights if their observed intervention status is underrepresented (overrepresented) given the values of their covariates.

However, there are some caveats when using propensity score weighting. The main concern with these estimators arises when the covariates' distributions significantly differs by treatment group. In this case, the estimated propensity score takes on extreme values for some observations (e.g. close to zero or one). Extreme values of the propensity score raise two main issues. First, it might indicate that even when logistic or probabilistic models provide similar approximations of estimated probabilities for the middle ranges of their arguments, they won't do so when the probabilities are close to zero or one. In other words, estimates are sensitive to the choice of model and specification in the presence of extreme propensity score values. Second, for individuals with propensity score values close to zero, the weights can be extremely large, making those units influential in the estimates of the causal effects of a program or intervention. On the other hand, for individuals with propensity score values close to one, the weights can be extremely small, diminishing these observations' impact on the estimates of the causal effects. Thus, extreme values will make the estimator imprecise. In our case, we do not have this concern because our estimated propensity scores do not take on extreme values; all estimated propensity scores fall between 0.06 and 0.84.

Implementation of propensity score weighting begins with fitting a regression model that predicts the probability of receiving a treatment, given the covariates. This model is referred to as the treatment mechanism in the program evaluation literature. In this study, the probability of receiving zero, one or two years of CPC preschool is estimated using a multinomial probit regression, controlling for child characteristics, family risk factors and neighborhood characteristics.

The estimated probabilities generated by the multinomial probit regression are the propensity scores used to construct the weight. The estimation assigns each student a predicted probability of receiving each of the three treatment levels (and these predicted probabilities sum to one for each student.) Following Foster (2003), participants are assigned weights equal to the inverse of the predicted probability of receiving the dosage that they actually received. E.g., if a student who enrolled in two years of preschool has a predicted probability or propensity for the two-year treatment equal to 0.75, then his or her weight is $1/0.75$ or 1.33. The predicted probability for treatment levels not received is not used. The weighting results in students with overrepresented intervention status, given their covariates, to be assigned smaller weights. For example, if a student attended one year of preschool and is overrepresented among all of those who attended one year of preschool, given the covariates, this student is assigned a smaller weight. We estimate outcome equations by weighting by the inverse of the propensity score.

3.4. Measures

Dependent variables—We present dosage effects of CPC preschool on a variety of long-term outcomes. The first results to be discussed are for school-age and juvenile outcomes. The first measure is children's scores on reading and math tests at grade 8. Each subject area of the Iowa Test of Basic Skills (ITBS) was used as a measure of cognitive achievement. Both nationally standardized subtest scores have demonstrated high reliability ($KR-20s > .90$) and predictive validity (Hieronymus & Hoover, 1990). The second measure is special education placement. We measure effects on special education status in two ways with two dichotomous variables. The first is coded 1 if the child was assigned to a special education classroom (self-contained or otherwise) in at least one grade between 1st and 8th grades, and 0 otherwise. The second is coded 1 if the child was assigned to a special education classroom (self-contained or otherwise) in at least one grade between 1st and 12th grades, and 0 otherwise. The third measure, grade retention, was coded 1 if children are on record as repeating a grade at least once from 1st to 8th grade; all other children were coded 0 (promoted or not retained). Because very few children were retained more than once, grade retention was defined cumulatively (i.e., ever retained). Retained children were included in the analysis with their age cohorts. Data on school performance were obtained from a grade-by-grade analysis of the school system's computerized records.

The fourth measure is child abuse and neglect. This is a dichotomous variable coded 1 if there is any substantiated report of abuse or neglect from a court (Cook County Juvenile Court) or DCFS (Child Protection Division of Illinois Department of Child Services) for participants aged 4 through 17, and 0 otherwise. This indicates that the child was the victim of abuse or neglect between age 4 and 17, not that the child became abusive in adulthood.

Impacts on this outcome variable would reflect the effect of the parent involvement component of the CPC program. The fifth measure is juvenile criminal behavior. Juvenile delinquency was measured between 1990 and 1998 (ages 10-18) from administrative records including the Cook County juvenile court and two other mid-western locations. Juvenile arrest consisted of formal petitions of students who were arrested on criminal charges and went before a judge. Some petitions may cause a warning or referral to social service agencies. Individuals with any juvenile arrest records were coded 1 and 0 otherwise. Violent offenses refer to aggravated discharge of a firearm, assault, battery, criminal sexual abuse or assault, kidnapping or unlawful restraints, and murder or intent to kill, among others. Participants with any violent offense records were coded 1 and 0 otherwise.

Moving on to adult outcomes, the sixth outcome is adult criminal behavior. Arrest, conviction, and incarceration histories from ages 18 to 24 were obtained from administrative records from county, state, and federal agencies supplemented with the adult survey. Arrests were measured dichotomously both overall and by whether charges were felonies, or involved violent offenses (i.e. aggravated assault, armed robbery). The convictions variable indicates whether courts found individuals were found guilty of felonies or any violent offense. Incarceration measured whether individuals were sentenced to correctional institutions at the state or federal level or to county jails beyond 30 days. Most records were from Illinois and other Midwestern states through December 9, 2004.

Our analysis next proceeds to analyze additional outcomes in early adulthood. First, we measure educational attainment using multiple indicators. Assessed at age 25 (mean, 25.5 years as of August 31, 2005), attainment was derived from administrative records from colleges and universities in Illinois and other states, administrative records from K-12 schools, and brief surveys of participants or family members. High school graduation takes a value of one for participants who received an official high school diploma; for all others, including those who received a GED or equivalent credential, this variable is coded as zero. College attendance and 4-year college attendance measured whether participants earned course credit for enrollment in a 2 or 4 year college program or in a college awarding a bachelor's degree. Highest grade completed was an ordinal indicator ranging from 6 to 16 (bachelor's degree).

Next, we examine health insurance coverage, assessed at age 27. Public insurance coverage data came from state-level Medicaid records and the adult survey. Private insurance coverage (employer-based) data came from adult survey responses (i.e. "Do you get health benefits from your employer?") and were supplemented with records from the Illinois Department of Employment Security and administrative records from universities and colleges (individuals attended a 4-year institution were assumed to have had health insurance for the duration of attendance). Additionally, although individuals receive health care when incarcerated, individuals that received adult criminal sentences of 10 or more years by age 27 were coded as not having public or private insurance. We made this decision because of the evidence from national studies about the poor access to health care in prisons (e.g., Wilper et al. 2009) and because we viewed the health care acquired as part of a prison stay as not a positive outcome. Admittedly there could be some debate about how to view health insurance outcomes for the incarcerated portion of our sample."

Third, we measure socio-economic status. We used three dichotomous variables: occupational prestige by age 24; socio-economic status by age 24; a compound measure that includes education and earnings by age 27; and one scale variable: socio-economic status by age 27. The occupational prestige indicator, our first measure, is a 9 item scale based on the Barratt Simplified Measure of Social Status (BSMSS) and the Nakao Treas prestige scores. Our measure was coded 1 if greater than 3, adjusting for cases that completed a 4 year degree, and including cases with average annual income during 2002-04 less than or equal to \$9,000. In creating the composite measure of SES, occupational prestige and educational attainment scores were added. To receive an SES score, participants must have received a score for occupational prestige (1-5) and educational attainment (1-5). The resulting measure is a 2-10 scale of SES.

Our second SES measure is a dichotomous SES variable that was coded 1 if the index was greater than 6, and it includes cases with average annual income during 2002-04 less than or equal to \$9,000. Our third measure, the compound measure by age 27 was coded as one for cases that completed a degree or certificate by age 25, or had 8 or more quarters with earnings of \$4,000 or more from 2004 through 2007, or reported full-time employment on the adult survey. Otherwise, they were coded 0. Our last measure is a two factor indicator of SES by age 27. This measure was created by adding the variables attainment and average quarterly salaries. Cases must have a value for attainment and average quarterly salaries to be included in the sample, with the exception of cases that completed a 4-year degree by August 2005. All cases that completed a 4-year degree by August 2005 were coded 8 (the highest possible SES score). The lowest possible score is 0. A score of 0 is assigned to a high school drop out with average quarterly income of less than \$2,250.

Explanatory variables—The key explanatory variable in the analysis is CPC preschool participation. We consider three categories: zero, one, and two years. The children with zero years of CPC preschool either attended the comparison sites or entered the CPC program sites in their kindergarten year. One year CPC preschool refers to children who attended a CPC preschool for one year (typically, at age 4). Two-year CPC preschool refers to children who participated in the half-day CPC preschool program for two years at age 3 and 4.

A variety of child, family and school level variables are included as additional explanatory variables in the estimation. Child level variables include: race/ethnicity, gender, and low-birth weight. Following convention in the medical literature, we chose a birth weight of 2,500 grams (5 pounds, 8 ounces) as the threshold that defines low birth weight. Data from birth records were obtained from the Illinois Department of Public Health for students who could be matched on name and birth-date.

Family level variables include dichotomous family risk factors at child's age 0-3: (a) parent did not complete high school, (b) parent did not had any college education, (c) residence in a single-parent family, (d) parent not employed full or part time, (e) mother was less than 18 years old at child's birth (f) eligibility for a fully subsidized lunch defined as a family income at or below 130% of the federal poverty line, (g) temporary assistance for needy families (TANF) participation (h) residence in a school neighborhood in which 60% or more of children are low-income families; as well as a home environment index. This is a

continuous variable that was created using self-reported data from the adult survey and administrative records. Home environment problems considered were: any welfare case history, frequent family conflict, substance abuse of parent and family financial problems. Participants were asked retrospective questions about their home environment. They had to check a list of situations: “frequent family conflict,” “substance abuse of parent,” “family financial problems,” among others. School-level variable includes school age CPC intervention. Children who attended the CPC school-age program for one or more years from first to third grade were coded 1, regardless of preschool involvement status, and 0 otherwise. This was a program available to 1st through 3rd-graders at CPC schools. This is equal to 1 for children from the non-CPC preschool group only if they transferred into a CPC school after kindergarten.

4. Results

As explained above and noted in Table 2, individuals receiving different amounts of preschool may differ systematically. One strategy is to adjust for various individual or family risk factors and neighborhood characteristics that may potentially influence both dose and outcomes.

The dosage categories considered in this study are zero, one or two years of CPC preschool. Although these categories appear to lend themselves to an ordered estimation technique, some researchers have used multinomial regressions (Foster, 2003; Jepsen, 2008) and others have used ordered regressions to characterize similar dosage treatment (Glewwe and Jacoby, 1995; Bedard, 2001; Jimenez and Kuegler, 1987).

In this study, it is not clear that ordering assumptions are appropriate. An underlying assumption for ordered probit or logit models is that there is a monotonic relationship between the latent probability of choosing years of CPC preschool and the explanatory variables. Given the various reasons why children may have various exposures to preschool, children who received one year of preschool, for example, may be more or less disadvantaged than those who received two or zero. Students who participated for two years may be more or less disadvantaged than those who have zero or one if recruitment efforts targeted the most in need or if more stable or motivated parents enrolled their children for two years. Thus, an unordered multinomial regression seems to be more appropriate in our case.

The determinants of the preschool duration decision are estimated using a multinomial probit model (Hausman and Wise, 1978) assuming parents make choices about the years of preschool enrollment. The parents of each child are assumed to make a utility valuation for each choice according to the following formulas:

$$\begin{aligned}U_{i1} &= X_i B_1 + Z_{i1} \alpha + e_{i1}, \\U_{i2} &= X_i B_2 + Z_{i2} \alpha + e_{i2}, \\U_{i3} &= X_i B_3 + Z_{i3} \alpha + e_{i3},\end{aligned}$$

where U_{ij} is the utility valuation that the i th parents ($i = 1, \dots, n$) gives to choice j , where $j=1$ for zero years of CPC preschool, $j=2$ for one year and $j=3$ for two years of CPC preschool. The X s are characteristics of students or households, the B s represent weights given the student or household characteristics in making each choice, and the Z s represent characteristics of the three choices. Here, the only difference among choices is the duration of enrollment. While multinomial logit estimation restricts the correlation between each pairing of the three errors above to be zero, multinomial probit estimation allows for a nonzero correlation among errors.

We examine the appropriateness of using multinomial probit versus logit. The multinomial logit is frequently used because of its ease of computation. The logit, however, relies on the assumption of the independence of irrelevant alternatives. Statistically this is equivalent to assume independence of the error terms across pairs of alternatives. A simple way to test this assumption is to estimate the model taking off one modality (e.g. zero years of preschool), and to compare the parameters with those of the complete model. If the assumption holds, the parameters should not change significantly, if they do, this indicates that the assumption does not hold.

We used Hausman and McFadden's specification (1984) to test the validity of this assumption. Results are presented in Table 3. Under the null hypothesis of independence of irrelevant alternatives, Table 3 shows that if we omit alternative one (participation in one year of CPC preschool), the parameters change significantly when compared with the overall model's parameters. This indicates that the assumption does not hold, suggesting that the multinomial probit approach is more appropriate.

In the first stage of our analyses, we estimated a multinomial probit regression predicting dosage. Table 4 presents the results of these analyses where the reported estimated coefficients represent the effect of a one unit change in the covariate on the probability that one of the three levels of participation will be chosen. Note that the marginal effects associated with each covariate sum to zero across the three dosage levels.

The probability of attending zero years of CPC preschool is predicted by the child's birth year and birth quarter, being male, having a mother that did not complete high school at child's birth-age, having a mother that did not attend college at child's birth-age, having a mother that was employed at child's birth, not having a negative home environment, and the percentage of individuals over age 25 that have completed high school in the child's neighborhood in the year of the child's birth. The probability of attending one year of CPC preschool is negatively correlated with having a mother that did not attend college at child's birth and percentage of individuals over age 25 that have completed high school in the neighborhood at child's birth, and positively correlated with having a single mother at birth, having a negative home environment during the first five years of life, and having been born in the last quarter of the year. The probability of receiving two years of CPC preschool is negatively correlated with: being male, being born in the last quarter of the year, having a mother that did not complete high school at birth-age, having a mother that is employed at birth-age, having any substantiated report of abuse and neglect at birth age, and the rate of unemployment in the neighborhood at child's birth.

We observe that even while some of the factors that explain the probability of zero years of CPC preschool also explain the probability of two years of CPC preschool, there are some factors that only explain one or the other. Moreover, the factors that explain the probability of one year of CPC preschool are different than the ones that explain the probability of two years of CPC preschool.

4.1. Overall Effect

After exploring the observed factors that predict years of preschool enrollment, we examine the main equations of interest for outcomes observed in adolescence and adulthood. We are interested in knowing for what types of outcomes (crime, educational attainment, socioeconomic status, health and health insurance decisions) two years of preschool is more or less effective than one year of preschool, and the magnitude of the potential benefits of attending two years of CPC preschool instead of one.

The unadjusted differences in means between the two-year and one-year groups show an advantage for the two-year group in test scores in kindergarten and at age 14. Children in the two-year group are less likely to repeat a grade between first and eighth grades. The two-year group has a lower number of juvenile prosecutions and is less likely to be processed for juvenile crime. The two-year group is less likely to be reported to child protective services for a substantiated report of abuse or neglects. The two-year group shows a slight advantage in educational attainment and socioeconomic status, but these unadjusted differences are not statistically significant.

The coefficient estimates from the regression showed in Table 4 were used to calculate propensity scores or predicted probabilities for each dosage category (0, 1 and 2 years of CPC preschool). As mentioned in the methodological section, the inverse of that probability was used to create a weight. The propensity score-adjusted estimates presented in Table 5 incorporated these weights.

After controlling for family risk factors, socio-demographic characteristics, and school characteristics, our findings indicate that when compared to students with one year of preschool, two-year students obtained three more points on the kindergarten standardized readiness, were 6% less likely to ever be retained by eighth grade ($p\text{-value}=0.039$), 6% less likely to have a record of child abuse or neglect ($p\text{-value}=0.009$), and 4% less likely to have a felony arrest by age 24. Additionally, a second year of preschool reduces the total number of juvenile petitions by 12% ($p\text{-value}=0.024$). These results show that attending two years of preschool has a lasting effect on juvenile and behavioral outcomes (e.g. crime), but not on other adult outcomes measures (e.g. educational attainment, health, SES). See Table 5 for complete results.

On the other hand, one year of CPC preschool has a lasting effect on school performance, health, educational attainment and SES adult outcomes when compared with participants who did not attend CPC preschool. Participants who did not attend CPC preschool obtained 5.3 fewer points on the Standardized Reading Comprehension test taken at age 14 ($p\text{-value}=0.01$), 4.7 fewer points on the Standardized Math test taken at age 14 ($p\text{-value}=0.01$), were 5% ($p\text{-value}=0.06$) less likely to be in special education, and 6% ($p\text{-value}=0.04$) less

likely to be retained when compared with participants who attended one year of CPC preschool. Moreover, participants who did not attend a CPC preschool were 7% more likely to ever suffer a symptom of depression (p -value=0.04) and 12% less likely to have any health insurance (p -value=0.001) when compared to one year of CPC preschool participation. In addition, not participating in the CPC preschool program reduced the probability of obtaining a high school diploma by 7%, the probability of obtaining a high school diploma or GED by 8%, the likelihood of having higher occupational prestige by 7% and the probability of higher socioeconomic status by 8%. See Table 5 for further detail.

Appendix 2 presents results comparing outcomes for participants with any participation in CPC preschool (either one or two years) to individuals who did not participate in CPC preschool. The effect of attending any CPC preschool is generally closer to the effects observed for one versus zero years of CPC preschool than to the (generally smaller) effects observed for two versus one year of preschool.

4.2. Subgroup Effects

Analysis of long-term outcomes by important selected subgroups reveals that the CPC preschool program's impact varied by group. Analysis by subgroup confirms the hypothesis that an additional year of preschool has a stronger effect for more disadvantaged individuals. Examining effects on subgroups defined by the child's mother's education, gender, or school neighborhood poverty, we consider the more disadvantaged groups to be those whose mother had not graduated from high school by the participant's age three; those that attended kindergarten in higher poverty areas; and males, given the higher rates of crime and school failure among males in predominantly African-American neighborhoods like those in our sample. Results of subgroup analysis are presented in Table 6. The marginal effect is of greater magnitude and more significant for the more disadvantaged group when defined by mother's education and gender; results by school neighborhood poverty, however are mixed.

By mother's education—We applied a difference in difference technique to examine the differential marginal effect of a second year of preschool when compared the more disadvantaged group with the less disadvantaged group. In this case, the more disadvantaged group includes those participants whose mother had not completed high school at participant's age three, compared to those whose mother had. The differential marginal effect of the second year is significant in as many of the outcomes analyzed as of the first year for this subgroup, suggesting that the second year does not offer “diminishing returns”, broadly speaking, for this group. Results suggest that the second year is important in improving school-age academic outcomes, preventing juvenile and adult crime, and improving socioeconomic status. The outcomes that become significant for this subgroup include standardized reading test scores at age 14, probability of ever being in special education, juvenile crime, juvenile prosecutions, incarceration as an adult, felony arrest as an adult, and socioeconomic status. All significant results are in the expected direction.

By gender—We applied a difference in difference technique to examine the differential marginal effect of a second year of preschool when comparing outcomes for males and females. The differential marginal effect of the second year of CPC preschool for being

male, when compared with being female, is more significant in increasing reading and math scores, reducing adult crime, in improving educational attainment and in increasing socio-economic status.

By school neighborhood poverty—The subgroup effects by neighborhood poverty are less marked than those found in analysis by mother's education and gender. The differential marginal effect of the second year of CPC preschool for the higher poverty group, which attended kindergartens in neighborhoods with poverty rates over 60%, when compared with the lower poverty group, is more significant in increasing college attendance and in improving socio-economic status as a measure of occupational prestige. However, for criminal behavior and health outcomes, the differential marginal effect of the second year of CPC preschool is more significant for the lower poverty group, which attended kindergartens in neighborhoods with poverty rates below 60%, when compared with the higher poverty group. These mixed results that favor the more disadvantaged group for some outcomes and favor the less disadvantaged group for some other outcomes were only found when analyzing this subgroup.

4.3. Extensions and Robustness Checks

In this section we present the stability of our estimates to alternative samples. In Table 7, we first present results for a restricted sample in which we only use 95% of our original sample, leaving out 5% of individuals with the most extreme propensity scores values. Thus, for each CPC preschool category: zero, one and two, we eliminate from our sample the 2.5% highest propensity scores and the 2.5% lowest propensity scores. Although our main analysis did not show estimated propensity scores values that are very close to zero or one (the original propensity scores values were between .061 and .839), we still perform these estimations to test the robustness of our results (the new propensity scores values are between .114 and .782). We found virtually no change in the estimates. This is to be expected, because we did not have extreme propensity score values that could invalidate our estimates.

We next excluded from our sample children who did not attend CPC preschool at all, but attended other Title 1 preschools (e.g. Head Start). By doing this, we reduced the sample size for the zero category but warranted that our data only reflected years of preschool. Thus, in this scenario, zero years of CPC preschool means zero years of preschool. Because the zero category does not include any other type of preschool in this scenario, we expected that if any change would occur it would be with the marginal effects of participation in one year of preschool when compared to zero years of preschool. Under this scenario, we found that the marginal effect of the first year of preschool was almost identical than in the original scenario (with the overall sample), except for depression. We found that participating in one year of preschool decreased the likelihood of depression by 11% instead of 5%, as was found in the original analysis. All other results were very similar to the original analysis. We found virtually no changes for the marginal effect of attending two years of preschool.

5. Discussion

While most previous research on preschool dosage has assessed only short-term effects and has focused primarily on achievement, our study examined the effects of 2- versus 1-year of participation in the CPC program into adulthood using a relatively large urban cohort of minority children. The previous study of CPC preschool participation focused on outcomes as of age 12 or sixth grade for most participants. Long-term effects for a broader range of outcomes are provided. The use of inverse propensity score weighting in the current research strengthens confidence in the interpretation of estimated effects.

The literature on inverse propensity score weighting suggests some advantages of weighting vs. matching, but each is likely to have some advantages or drawbacks and some of these might be context specific. As shown here, propensity score weighting is especially useful with multiple treatment groups. Although weighting by the inverse propensity score does require a rich array of covariates with which to construct the propensity score, this methodology has the advantage of not requiring an instrumental variable and may be more flexible to implement than propensity score matching methods.

Effects of a Second Year of CPC Preschool on Long-term Outcomes—The data from the Chicago Longitudinal Study indicate that a second year of preschool has significant positive long-term impacts. Individuals that participated in two years of CPC preschool have significant advantages over those that participated in one year of CPC preschool in test scores, crime and reports of abuse or neglect of the participant. For this sample, our estimates with propensity score weighting indicate that the second year is associated with a 6.3% decrease in probability of ever being retained between grades one and eight, an 11.9% reduction in the number of juvenile petitions, and a 6.3% reduction in substantiated reports of child abuse or neglect. Considering findings that are significant at the 10% level, the second year is associated with a 4.2% decrease in arrests for felony charges. Generally, the marginal effect of attending one year of preschool is greater in magnitude than the marginal effect of a second year. The effect of attending any CPC preschool versus no CPC preschool is closer in magnitude to the effect of the first year of CPC preschool than to the second year. See Appendix 2 for full details.

However, the results suggest that for some outcomes, the effect of the second year is not smaller than that of the first. The second year's effect on retention is the same size as the first year's effect but is more highly significant. The second year's effects on juvenile petitions, adult felony arrest and juvenile petitions are larger and are accompanied by smaller standard errors. The second year's impact on educational attainment is not significant in the main analysis or in subgroup analysis. Nevertheless, the significant positive effects of preschool length on kindergarten school readiness (see Table 5 and Appendix 1) and early school performance (Reynolds, 1995) demonstrate that program duration provides an important foundation for promoting school success.

Many of the estimated effects are greater in magnitude and more significant for participants whose mother had not completed high school when the participant was three years old. For

these participants, the second year of preschool is important in improving academic achievement through high school, reducing crime and improving socio-economic status.

The size of the impact of two years of preschool over one year is likely to depend on various factors. Although not studied here, an important factor is likely to be program quality. Research indicates that programs with a well-developed instructional philosophy in which teachers are responsive to children's needs and effectively integrate whole-class, small-group, and child-initiated learning activities are most associated with learning gains (Bowman, Donovan, & Burns, 2001; Zigler, Gilliam, & Jones, 2006). As explained by Reynolds (1995; p, 23), "an additional year that simply repeats learning activities of the first year would not be expected to make much difference."

Other established ingredients of preschool effectiveness also would impact the magnitude of observed effects. These include teacher background and professional training (Bowman et al., 2001), small class sizes (Temple & Reynolds, 2007), intensity in the quantity and focus of instruction (Barnett & Youn, 2011; Bowman et al., 2001), and the provision of comprehensive family services (Zigler et al., 2006). These components of the CPC program likely contribute to the short-term and long-term effects of participation reported in the current study. They also are common elements of preschool programs that show high economic returns (Heckman et al. 2010; Temple & Reynolds, 2007; Reynolds & Temple, 2008). For example, the three early interventions demonstrating positive effects into adult and corresponding economic benefits—HighScope Perry Preschool, Carolina Abecedarian Project, and the CPCs—provided at least two years of services as well and executed the principles of effectiveness described above (Temple & Reynolds, 2007).

The provision of two or more years of program services also is an element of other human capital and preventive interventions for young people including social skills training (Nation et al., 2003; Reynolds & Temple, 2008), class size reductions (Finn & Achilles, 1999), home visitation (Reynolds, Mathieson, & Topitzes, 2009, and drug education programs (Nation et al., 2003).

Limitations—Despite the considerable strengths of using the Chicago Longitudinal Study (i.e., its sample size, longitudinal nature, and comprehensive set of covariates), this study has limitations that need to be acknowledged before discussing the policy implications. As mentioned above, CPCs represent a particular type of preschool, one that provided parental support in addition to child education at a time when this type of programs were rare, and one that targeted low-income minority children. Thus, our results only extend to disadvantaged populations.

In addition to that, it is important to note that propensity score weighting assumes that all characteristics that are driving selection into duration of preschool enrollment (zero, one or two years of CPC preschool) are observed by the researcher and have been included in the statistical models. Thus, our results could be biased if any additional variables that are confounded with years of CPC preschool participation or cognitive and socio-emotional long-term outcomes were excluded. We meticulously reviewed the literature when considering our covariates for this analysis, accounting for most if not all of what previous

studies have found to impact selection of duration of preschool participation. However, it is possible that unobserved factors such as supply-side factors including discretion of the project administrators to assign the available spots and demand factors such as parental beliefs and motivations may also explain selection into years of CPC preschool participation.

The ideal study would be a randomized experiment in which both observed and unobserved predictors are balanced. However, we conducted auxiliary analysis to check the robustness of our estimations. Our results were consistent when we dropped cases with extreme propensity scores and when we used an alternative comparison group for children who did not attend CPC preschool, restricting this group to those who did not attend any preschool. This provided more rigorous evidence to support our conclusions.

Policy Implications—The question of whether to invest public funds to expand access to a second year of high quality preschool has obvious policy implications. Providing access to a second year of preschool in a universal or targeted program has a significant opportunity cost. Policy-makers interested in improving early childhood education are likely to have to face the question of whether or not to prioritize expanding access to one year of preschool to a large number of children or to dedicate some funds toward a second year. The findings of this research indicate that a second year of preschool does have significant and lasting effects for the participants of the Child Parent Centers in this study. More specifically, policy-makers seeking a strategy to reduce crime or improve educational efficiency by reducing grade retention and demand for special education services may prioritize a second year of preschool. While the entire sample of students came from economically-disadvantaged backgrounds, the findings suggest that the most disadvantaged children within the sample benefited most from the CPC program. This suggests that targeting scarce resources for a second year of preschool to disadvantaged children may be an efficient decision for early childhood interventions.

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Highlights for review

This paper uses inverse propensity score weighting to estimate the differential effectiveness of zero, one or two years of preschool on adult outcomes. Public spending on preschool for four year olds is the fastest growing education expenditure category. Is there a benefit of offering preschool for two years instead of one?

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Table 1

CPC Program Characteristics

CPC-Preschool	N	Percent of total students	Percent in area with over 60% poverty	Percent in full-day kindergarten	Percent in school-age program	Average Age at Preschool Entrance
Any CPC	989	64.3%	77.7%	59.9%	69.2%	3.44
Two years	534	34.7%	78.7%	59.7%	70.2%	3.00
One year	455	29.6%	76.5%	60.0%	67.9%	3.95
Zero years	550	35.7%	72.9%	100.0%	30.2%	N/A
Total	1,539	100.0%	76.0%	74.2%	55.2%	N/A

Table 2

Group Differences

	2-Year Group	1-Year Group	0-Year Group	p-value: 2yr - 1yr	p-value: Any-CPC - No-CPC
Percentage in sample	0.320	0.260	0.312	0.020	0.067
Female	0.543	0.482	0.473	0.057	0.117
Black	0.948	0.903	0.935	0.008	0.589
School-age participation	0.702	0.679	0.302	0.433	0.000
Single parent	0.743	0.796	0.760	0.053	0.742
Mother under 18 at child-birth	0.139	0.176	0.173	0.108	0.385
Mother H.S. graduate at age 3	0.536	0.437	0.398	0.002	0.001
Mother has some college	0.135	0.134	0.100	0.972	0.048
TANF by age 3	0.625	0.637	0.622	0.700	0.723
Mother is employed	0.326	0.327	0.355	0.957	0.266
Child welfare history by age 3	0.019	0.046	0.049	0.014	0.080
School lunch eligibility	0.841	0.844	0.829	0.893	0.502
More than 4 children in the HH	0.170	0.147	0.178	0.322	0.353
Missing data for risk factors	0.139	0.158	0.189	0.385	0.035
School neighborhood poverty over 60%	0.787	0.765	0.729	0.415	0.037
Low birth weight	0.112	0.105	0.133	0.730	0.170
Child was born in first quarter	0.267	0.237	0.292	0.052	0.989
Child was born in second quarter	0.213	0.253	0.243	0.736	0.121
Child was born in third quarter	0.284	0.240	0.258	0.495	0.148
Child was born in fourth quarter	0.236	0.270	0.206	0.018	0.973
Negative home environment ^a	0.567	0.551	0.530	0.597	0.270
Extreme poverty, neighborhood	0.431	0.415	0.342	0.627	0.002
85% of minority in the neighborhood	0.708	0.677	0.735	0.293	0.091
HS completion, neighborhood (%)	0.413	0.390	0.466	0.001	0.000
Unemployment, neighborhood (%)	0.156	0.166	0.174	0.000	0.000

^a Home environment problems include: family conflict, family financial problem and substance abuse of parent.

Table 3
Hausman Test for Independence of Irrelevant Alternatives

Omitted	chi ²	D.F.	p-value	Evidence
0	4.91	24	1.0000	For H _o
1	50.63	24	0.0012	Against H _o
2	2.82	24	1.0000	For H _o

Table 4
Estimates of the Probability of Attending Zero, One or Two Years of Preschool - Multinomial Probit Regressions

Covariates	Zero years		One year		Two years	
	dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.
School neighborhood poverty	-0.017	0.1679	0.018	0.092	-0.001	0.114
Female	-0.038	0.0196 *	-0.013	0.022	0.051	0.022 *
Black	-0.238	0.2923	0.107	0.134	0.131	0.172
Low-birth weight	0.050	0.0355	-0.032	0.040	-0.018	0.038
Child was born in second quarter	-0.023	0.0469	0.053	0.047	-0.030	0.050
Child was born in third quarter	0.029	0.0359	0.013	0.038	-0.042	0.045
Child was born in fourth quarter	-0.065	0.0307 *	0.125	0.035 ***	-0.061	0.033 +
Year when child was born	-0.170	0.0745 *	0.087	0.056	0.083	0.063
Single mother	-0.031	0.0274	0.079	0.035 *	-0.048	0.036
Mother's age	0.022	0.0227	0.008	0.026	-0.030	0.031
Mother did not complete HS	0.080	0.0364 *	0.031	0.030	-0.112	0.028 ***
Mother did not attend college	0.084	0.0452 +	-0.110	0.057 +	0.026	0.047
Mother is employed	0.063	0.0382 +	-0.007	0.034	-0.056	0.033 +
Child abuse and neglect	0.111	0.0869	0.063	0.077	-0.174	0.064 **
TANF	-0.006	0.0361	0.021	0.034	-0.015	0.042
School lunch eligibility	-0.005	0.0346	-0.008	0.033	0.013	0.029
More than 4 children in the HH	0.040	0.0352	-0.027	0.031	-0.012	0.031
Missing risk factors	0.072	0.0278 **	-0.013	0.031	-0.059	0.024 *
Negative home environment	-0.046	0.0173 **	0.031	0.018 +	0.015	0.020
Extreme poverty, neighborhood	-0.055	0.0589	0.042	0.042	0.012	0.046
85% of minority in the neighborhood	-0.021	0.0726	-0.005	0.046	0.027	0.040
HS completion, neighborhood	0.016	0.0076 *	-0.010	0.004 **	-0.005	0.005
Unemployment, neighborhood	0.023	0.0214	0.005	0.011	-0.028	0.013 *

p<.001,

$p < .10$
+
 $p < .05$
*
 $p < .01$
**

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Table 5
Marginal Effects of One or Two Years of CPC Preschool

Outcome	N	2 vs. 1 year of CPC preschool						1 vs. 0 years of CPC preschool					
		Unadjusted		Adjusted without correction		With s election correction		Unadjusted		Adjusted without correction		With selection correction	
		dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.
<i>School performance</i>													
Year K standardized readiness test	1,531	3.314	0.532 ***	2.949	0.639 ***	2.960	0.541 ***	5.043	0.449 ***	4.620	1.169 ***	3.181	0.817 ***
SS reading scores	1,344	2.720	1.448 +	1.953	1.487	1.994	1.414	5.277	1.476 ***	3.620	1.685 *	5.273	1.483 **
SS math scores	1,343	1.714	1.223	1.206	1.076	0.893	1.066	4.638	1.226 ***	2.962	1.467 +	4.724	1.390 **
Ever in special ed, grades 1-8	1,377	-0.032	0.023	-0.030	0.022	-0.040	0.024	-0.060	0.026 *	-0.041	0.024 +	-0.045	0.028 +
Ever retained, grades 1-8	1,377	-0.077	0.028 **	-0.069	0.275 *	-0.068	0.030 *	-0.072	0.031 *	-0.012	0.035	-0.062	0.037 +
<i>Criminal behavior</i>													
Total juvenile prosecutions ^a	1,406	-0.217	0.110 *	-0.135	0.055 *	-0.115	0.054 *	-0.184	0.131	-0.082	0.079	-0.099	0.072
Any juvenile crime	1,406	-0.049	0.025 +	-0.037	0.026	-0.025	0.026	-0.063	0.028	-0.055	0.032 +	-0.048	0.031 +
Any felony arrest	1,418	-0.040	0.025	-0.023	0.022	-0.043	0.023 +	-0.052	0.027 +	-0.032	0.021	-0.020	0.021
Ever incarcerated	1,413	-0.029	0.023	-0.010	0.017	-0.027	0.018	-0.053	0.026 *	-0.032	0.019	-0.026	0.018
Total convictions	1,418	-0.105	0.075	-0.030	0.051	-0.030	0.057	-0.159	0.089 +	-0.053	0.050	-0.046	0.045
<i>Health outcomes</i>													
Any depression symptom	1,134	0.001	0.025	0.006	0.027	0.007	0.028	-0.048	0.028 +	-0.054	0.029 *	-0.072	0.034 *
Any substance misuse	1,137	-0.004	0.023	0.005	0.032	-0.004	0.031	-0.055	0.026 *	-0.038	0.023 +	-0.030	0.022
Any health insurance	1,304	0.022	0.030	0.021	0.034	0.021	0.038	0.087	0.032 **	0.094	0.036 **	0.124	0.035 ***
Any substantiated report of abuse/	1,411	-0.066	0.020 **	-0.057	0.020 **	-0.060	0.022 *	-0.029	0.024	-0.017	0.024	-0.034	0.033
<i>Educational attainment</i>													
High school diploma	1,373	0.009	0.033	-0.014	0.041	-0.010	0.041	0.065	0.034 +	0.054	0.033 +	0.069	0.032 *
High school diploma or GED	1,373	-0.003	0.027	-0.010	0.036	0.004	0.034	0.095	0.030 **	0.072	0.032 *	0.067	0.031 *

Outcome	N	2 vs. 1 year of CPC preschool				1 vs. 0 years of CPC preschool			
		Unadjusted		With selection correction		Unadjusted		Adjusted without correction	
		dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.	dY/dX	S.E.
Any college	1,373	0.012	0.030	-0.003	0.033	0.043	0.030	0.019	0.030
Highest grade completed	1,370	-0.009	0.112	-0.077	0.096	0.398	0.117 ***	0.316	0.117 *
<i>Socio Economic Status</i>									
Occupational prestige	1,280	0.020	0.032	-0.008	0.022	0.062	0.031 *	0.074	0.024 **
SES 4 ^c	1,257	0.005	0.029	-0.011	0.021	0.063	0.027 *	0.062	0.023 *

p<.001;

**
p <.01;

*
p<.05;

+
p<.10.

Notes: Coefficients are from OLS, probit, or negative binomial regression analysis transformed to marginal effects, and they are adjusted for earlier and later program participation (preschool or school-age), 8 indicators of pre-program risk status, sex of child, race/ethnicity, child welfare history by age 4, and a dummy-coded variable for missing data on risk status. The *p*-value is the probability level of the adjusted mean or percentage difference. Standard errors, and, thus, *p*-values, are adjusted for variation among program sites.

^aThis item refers to total juvenile petitions. All juvenile cases begin with the prosecuting attorney filing a juvenile petition with the court. The juvenile petition is similar to an adult criminal complaint. The juvenile petition alleges what the juvenile has done, or not done, which causes him to come under the purview of the juvenile justice system.

^bIndicates moderate to high occupational prestige or skill level. Occupational prestige ranges from 1 to 8.

^cThe socioeconomic status index accounts for occupational prestige and educational attainment by age 24. The index ranges from 0 to 8 with 4 indicating moderate SES.

Table 6
Differential marginal effect (Group A - Group B) of two years of CPC preschool

	Male vs. Female		Mom did not complete HS vs. Mom completed HS		High poverty vs. Low poverty	
	dy/dx	S.E.	dy/dx	S.E.	dy/dx	S.E.
School performance						
Year K standardized readiness test	2.740	1.901	0.069	1.036	-0.762	1.133
Standardized reading scores, age 14	4.323	2.481 +	4.636	2.096 *	1.177	2.557
Standardized math scores, age 14	3.607	2.045 +	3.013	1.985	1.627	2.465
Ever in special education, grades 1-8	-0.028	0.049	-0.140	0.045 **	-0.054	0.043
Ever retained, grades 1-8	-0.062	0.038	0.017	0.046	-0.016	0.063
Criminal behavior						
Any juvenile crime	0.021	0.049	-0.083	0.044 +	0.001	0.053
Total juvenile prosecutions ^a	0.297	0.198	-0.183	0.067 **	-0.057	0.105
Any adult felony arrest	-0.068	0.039 +	-0.053	0.031 +	0.076	0.040 +
Ever incarcerated	-0.068	0.043	-0.061	0.031 +	0.067	0.044
Total convictions	-0.162	0.104	0.003	0.103	0.094	0.134
Health outcomes						
Any depression symptom	-0.032	0.061	-0.021	0.035	0.016	0.074
Any substance misuse	-0.014	0.042	-0.021	0.046	0.061	0.061
Any health insurance	-0.009	0.050	0.069	0.045	-0.048	0.052
Any substantiated report of abuse/neglect	0.021	0.038	0.014	0.045	0.081	0.035 *
Educational attainment						
High school completion	0.112	0.052 *	-0.067	0.050	0.061	0.091
Any college	0.023	0.054	0.030	0.042	0.119	0.045 **
Highest grade completed	0.281	0.187	0.158	0.158	0.162	0.157
Socio Economic Status						
Occupational prestige ^{3 4 b}	0.067	0.048	0.070	0.043	0.11521	0.054 *
SES ^{4 c}	0.095	0.050 +	0.100	0.060 +	0.071	0.054

p<.001;

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p<.01;
p<.05;
p<.10.

Notes: Coefficients are from OLS, probit, or negative binomial regression analysis transformed to marginal effects, and they are adjusted for earlier and later program participation (preschool or school-age), 8 indicators of pre-program risk status, sex of child, race/ethnicity, child welfare history by age 4, and a dummy-coded variable for missing data on risk status. The *p*-value is the probability level of the adjusted mean or percentage difference. Standard errors, and, thus, *p*-values, are adjusted for variation among program sites.

^aThis item refers to total juvenile petitions. All juvenile cases begin with the prosecuting attorney filing a juvenile petition with the court. The juvenile petition is similar to an adult criminal complaint. The juvenile petition alleges what the juvenile has done, or not done, which causes him to come under the purview of the juvenile justice system.

^bIndicates moderate to high occupational prestige or skill level. Occupational prestige ranges from 1 to 8.

^cThe socioeconomic status index accounts for occupational prestige and educational attainment by age 24. The index ranges from 0 to 8 with 4 indicating moderate SES.

Table 7
Sensitivity Analysis to the Estimated Dosage Effect of CPC Preschool

Outcome	2 vs. 1 year of CPC preschool with selection correction						1 vs. 0 years of CPC preschool					
	IPW, overall sample			IPW, without extreme values			IPW, without other preschool			IPW, without extreme values		
	dY/dX	S.E		dY/dX	S.E		dY/dX	S.E		dY/dX	S.E	
School performance												
Year K standardized readiness test	2.960	0.541 ***		2.941	0.614 ***		2.861	0.562 ***		3.180	0.820 ***	
SS reading scores	1.994	1.414		1.220	1.485		2.001	1.440		3.620	1.685 *	
SS math scores	0.893	1.066		0.673	1.146		0.894	1.140		2.962	1.467 +	
Ever in special ed, grades 1-8	-0.040	0.024		-0.034	0.024		-0.038	0.023		-0.041	0.024 +	
Ever retained, grades 1-8	-0.068	0.030 *		-0.070	0.029 *		-0.067	0.030 *		-0.012	0.035	
Criminal behavior												
Total juvenile prosecutions ^a	-0.115	0.054 *		-0.108	0.058 +		-0.122	0.057 *		-0.082	0.079	
Any juvenile crime	-0.025	0.026		-0.017	0.029		-0.025	0.027		-0.055	0.032 +	
Any felony arrest	-0.043	0.023 +		-0.015	0.024		-0.041	0.023 +		-0.032	0.021	
Ever incarcerated	-0.027	0.018		-0.005	0.015		-0.025	0.019		-0.032	0.019 +	
Total convictions	-0.030	0.057		0.010	0.067		-0.037	0.055		-0.053	0.050	
Health outcomes												
Any depression symptom	0.007	0.028		0.019	0.029		0.002	0.029		-0.054	0.029 *	
Any substance misuse	-0.004	0.031		0.020	0.028		-0.005	0.031		-0.038	0.023 +	
Any health insurance	0.021	0.038		0.009	0.036		0.026	0.035		0.094	0.036 **	
Any substantiated report of abuse/neglect	-0.060	0.022 *		-0.061	0.022 *		-0.056	0.024 *		-0.017	0.024	
Educational attainment												
High school diploma	-0.010	0.041		-0.018	0.047		-0.013	0.041		0.054	0.033 +	
High school diploma or GED	0.004	0.034		-0.004	0.040		0.005	0.033		0.072	0.032 *	
Any college	0.015	0.033		-0.007	0.034		0.015	0.032		0.019	0.030	

Outcome	2 vs. 1 year of CPC preschool with selection correction						1 vs. 0 years of CPC preschool					
	IPW, overall sample			IPW, without extreme values			IPW, without other preschool			IPW, overall sample		
	dY/dX	S.E	dY/dX	S.E	dY/dX	S.E	dY/dX	S.E	dY/dX	dY/dX	S.E	dY/dX
Highest grade completed	-0.030	0.088	-0.076	0.102	-0.041	0.092	0.316	0.117 *	0.283	0.124 *	0.292	0.122 *
<i>Socio Economic Status</i>												
Occupational prestige ^{4 b}	-0.001	0.026	-0.020	0.026	-0.008	0.025	0.074	0.024 **	0.089	0.028 **	0.073	0.034 *
SES ^{4 c}	0.002	0.024	-0.005	0.023	-0.003	0.023	0.062	0.023 *	0.069	0.023 **	0.070	0.037 +

*** p<.001;

** p <.01;

* p<.05;

+ p<.10.

Notes: Coefficients are from OLS, probit, or negative binomial regression analysis transformed to marginal effects, and they are adjusted for earlier and later program participation (preschool or school-age), 8 indicators of pre-program risk status, sex of child, race/ethnicity, child welfare history by age 4, and a dummy-coded variable for missing data on risk status. The *p*-value is the probability level of the adjusted mean or percentage difference. Standard errors, and, thus, *p*-values, are adjusted for variation among program sites.

^aThis item refers to total juvenile petitions. All juvenile cases begin with the prosecuting attorney filing a juvenile petition with the court. The juvenile petition is similar to an adult criminal complaint. The juvenile petition alleges what the juvenile has done, or not done, which causes him to come under the purview of the juvenile justice system.

^bIndicates moderate to high occupational prestige or skill level. Occupational prestige ranges from 1 to 8.

^cThe socioeconomic status index accounts for occupational prestige and educational attainment by age 24. The index ranges from 0 to 8 with 4 indicating moderate SES.

Appendix 1

Unadjusted means by CPC preschool dosage

Compare means 1 vs. 2 years																														
Comparison Group																														
One year preschool																														
Two year preschool																														
Full sample																														
Sig.	N	t	p	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.
School performance																														
	989	-3.314	0.000	550	44.129	7.118	455	47.382	8.846	534	50.697	9.021	1,539	8.774																
+	880	-1.878	0.061	464	140.308	21.688	405	145.585	21.731	475	148.305	21.135	1,344	21.757																
-	880	-1.401	0.162	463	143.676	18.279	405	148.314	17.716	475	150.027	18.391	1,343	18.342																
-	895	1.394	0.164	482	0.207	0.406	413	0.148	0.355	482	0.116	0.321	1,377	0.364																
**	895	2.716	0.007	482	0.351	0.478	413	0.278	0.449	482	0.201	0.401	1,377	0.448																
Criminal behavior																														
+	911	1.93	0.054	495	0.830	2.08	413	0.65	1.82	498	0.43	1.49	1,406	1.817																
*	911	1.98	0.048	495	0.27	0.44	413	0.21	0.40	498	0.16	0.36	1,406	0.408																
-	918	1.62	0.105	500	0.24	0.43	413	0.19	0.39	505	0.15	0.35	1,418	0.393																
	911	1.29	0.197	500	0.20	0.40	411	0.15	0.36	502	0.12	0.33	1,413	0.366																
-	916	1.41	0.160	500	0.66	1.46	413	0.50	1.19	505	0.40	1.08	1,418	1.260																
Health outcomes																														
-	744	-0.023	0.982	390	0.185	0.388	329	0.137	0.019	415	0.137	0.017	1,134	0.361																
-	748	0.19	0.847	389	0.17	0.38	331	0.11	0.32	417	0.11	0.31	1,137	0.339																
-	850	-0.73	0.463	454	0.65	0.48	381	0.73	0.44	469	0.76	0.43	1,304	0.453																
**	914	3.27	0.001	497	0.17	0.38	414	0.14	0.35	500	0.07	0.26	1,411	0.333																
Adult educational attainment																														
-	893	-0.266	0.790	480	0.490	0.500	400	0.555	0.498	493	0.564	0.496	1,373	0.499																
-	893	0.126	0.900	480	0.698	0.460	400	0.793	0.406	493	0.789	0.408	1,373	0.428																
-	893	-0.40	0.692	480	0.24	0.43	400	0.28	0.45	493	0.29	0.46	1,373	0.444																
-	891	0.083	0.934	479	11.705	1.727	399	12.103	1.737	492	12.094	1.592	1,370	1.692																
Socio Economic Status																														
-	830	-0.615	0.539	450	0.240	0.428	377	0.302	0.024	453	0.322	0.022	1,280	0.453																
-	816	-0.180	0.858	441	0.15	0.35	366	0.21	0.41	450	0.22	0.41	1,257	0.393																

Notes:

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d<.001;

p<.01;
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p<.05;
+
p<.01.

^aThis item refers to total juvenile petitions. All juvenile cases begin with the prosecuting attorney filing a juvenile petition with the court. The juvenile petition is similar to an adult criminal complaint. The juvenile petition alleges what the juvenile has done, or not done, which causes him to come under the purview of the juvenile justice system.

^bIndicates moderate to high occupational prestige or skill level. Occupational prestige ranges from 1 to 8.

^cThe socio-economic status index accounts for occupational prestige and educational attainment by age 24. The index ranges from 0 to 8 with 4 indicating moderate SES.

Appendix 2

Marginal Effects of Any CPC Preschool participation

Outcome	N	Any CPC preschool participation								
		Unadjusted			Adjusted without correction			With selection correction		
		mg. effect	std. error	sig.	mg. effect	std. error	sig.	mg. effect	std. error	sig.
<i>Grade 8</i>										
Year K standardized readiness test	1,531	5.04	0.45	***	4.62	1.17	***	5.04	1.20	***
SS reading scores	1,344	6.75	1.24	***	4.65	1.45	**	4.73	1.46	**
SS math scores	1,343	5.56	1.04	***	3.60	1.39	*	3.30	1.41	*
Ever in special ed, grades 1-8	1,377	-0.08	0.02	***	-0.06	0.02	***	-0.06	0.02	*
Ever retained, grades 1-8	1,377	-0.11	0.03	***	-0.05	0.03		-0.05	0.03	
<i>Juvenile outcomes</i>										
Total juvenile prosecutions ^a	1,406	-0.30	0.10	**	-0.17	0.09	+	-0.17	0.07	*
Any juvenile crime	1,406	-0.09	0.02	***	-0.08	0.03	**	-0.08	0.04	*
Any felony arrest	1,418	-0.07	0.02	***	-0.05	0.02	*	-0.06	0.02	**
Ever incarcerated	1,413	-0.07	0.02	***	-0.04	0.02	*	-0.05	0.02	**
Total convictions	1,418	-0.22	0.07	**	-0.07	0.04	+	-0.06	0.06	
<i>Adult health outcomes</i>										
Any depression symptom	1,134	-0.05	0.02	*	-0.05	0.02	*	-0.05	0.02	*
Any substance misuse	1,137	-0.06	0.02	**	-0.04	0.02	+	-0.04	0.02	*
Any health insurance	1,304	0.10	0.03	***	0.11	0.03	***	0.11	0.03	***
Any substantiated report of abuse/neglect	1,411	-0.07	0.02	***	-0.05	0.02	*	-0.05	0.02	**
<i>Adult educational attainment</i>										
High school diploma	1,373	0.07	0.03	*	0.05	0.03	+	0.05	0.03	
High school diploma or GED	1,373	0.09	0.02	***	0.07	0.02	**	0.07	0.03	*
Any college	1,373	0.05	0.03	+	0.02	0.03		0.02	0.03	
Highest grade completed	1,370	0.39	0.10	***	0.27	0.09	**	0.30	0.11	**
<i>Socio Economic Status</i>										
Occupational prestige ^{a b}	1,280	0.07	0.03	**	0.07	0.02	**	0.08	0.03	**
SES ^{4 c}	1,257	0.07	0.02	**	0.06	0.02	*	0.06	0.03	*

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Notes:

- *** $p < .001$;
- ** $p < .01$;
- * $p < .05$;
- + $p < .10$.

Coefficients are from OLS, probit, negative binomial regressions, transformed to marginal effects, and they are adjusted using propensity score weighting to adjust for selection bias between the any CPC and non-CPC groups. Covariates are listed in table 2.

^a This item refers to total juvenile petitions. All juvenile cases begin with the prosecuting attorney filing a juvenile petition with the court. The juvenile petition is similar to an adult criminal complaint. The juvenile petition alleges what the juvenile has done, or not done, which causes him to come under the purview of the juvenile justice system.

^b Indicates moderate to high occupational prestige or skill level. Occupational prestige ranges from 1 to 8.

^c The socio-economic status index accounts for occupational prestige and educational attainment by age 24. The index ranges from 0 to 8 with 4 indicating moderate SES