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IZA DP No. 17430

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Arizona State University and IZA

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ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Effects of Center-Based Child Care on Disadvantaged Children: Evidence from a Randomized Research Design*

This paper uses the random assignment of poor families to treatment and control conditions in the Comprehensive Child Development Program (CCDP) to isolate the causal effect of center-based child care enrollment on child well-being. Operating throughout the early-1990's, the CCDP demonstration aimed to improve child development and family functioning by offering those in the treatment group five years of high-quality child care along with case management. As a result, treated children were substantially more likely to be enrolled in center-based programs throughout the preschool-age years, and I use this variation to estimate the impact of center care on children's language and social skills as well as health. I uncover mixed results: more time spent in center-based settings improves language skills but reduces social skills in the short-run, and both effects fade-out for most children within one to two years. I also find that early center care use is strongly predictive of later Head Start enrollment, indicating that a more deliberate "family retention strategy" may be effective at lengthening children's exposure to high-quality early education.

JEL Classification: I21, J13

Keywords: child care, child development, maternal employment, instrumental variables

Corresponding author:
Chris M. Herbst
School of Public Affairs
Arizona State University
411 N. Central Ave., Suite 420
Phoenix, AZ 85004-0687
USA

E-mail: chris.herbst@asu.edu

* I thank Jessica Brown, Lisa Gennetian, Spiro Maroulis, and Erdal Tekin for helpful comments and suggestions.

1 Introduction

Today, approximately 35% of U.S. children ages 0 to 5 are enrolled in a center-based child care program for an average of 25 hours per week, making it the predominant care mode among preschoolers (Herbst, 2023). Families' reliance on center-based services begins early in the child's life, with recent estimates suggesting that by age two child care centers surpass relatives as the most heavily used non-parental care type (Cui and Natzke, 2021). Altogether, 1.5 million children under the age of three—including 250,000 poor children and 370,000 children of single parents—are enrolled in such arrangements (Datta et al., 2023).[1] By comparison, 1 million children currently attend the federal Head Start program, and 1.6 million children are enrolled in state-administered pre-K programs.[2]

Despite the importance of center-based child care arrangements for young children, there is surprisingly little causal evidence on the developmental effects of enrolling in such programs (Bernal and Keane, 2011; Herbst, 2013). As with all studies of early education services, producing credible evidence on child care is difficult because of the endogeneity of families' care choices. However, whereas previous work on small-scale interventions (e.g., Abecedarian and Perry Preschool) as well as large publicly-provided programs (e.g., Head Start and pre-K) have produced credible evidence using randomized controlled trials, assignment lotteries, and policy-driven increases in supply, such opportunities are not readily available for studying privately-owned, community-accessible center-based programs.[3] The relative scarcity of evidence on center-based care is problematic in light of policymakers' recent interest in increasing families' access to these services.[4]

In this paper, I provide new evidence on the causal effect of center-based child care enrollment on the short- and medium-run development of young children. Specifically, I exploit the random assignment of families to treatment and control conditions in the Comprehensive Child Development Program (CCDP)—an early childhood

[1]The centers used by children ages 0 to 3 are generally referred to as community-accessible centers, so as to distinguish them from the publicly-provided, heavily (or fully) subsidized Head Start and pre-K programs that can operate out of center-based facilities. Indeed, the center services used by very young children are privately owned and operated—often by large national for-profit chains but also by non-profit entities—for which parents largely pay out-of-pocket. Throughout the paper, I refer to these providers as "center-based" services, and I sometimes precede this language with the "community-accessible" descriptor for emphasis or clarification.

[2]The Head Start figure sums over Early Head Start and Head Start enrollments. These figures are drawn from the Head Start Program Annual Fact Sheets (available at https://eclkc.ohs.acf.hhs.gov/browse/series/head-start-program-annual-fact-sheets) and the NIEER State of Preschool Yearbook (available at https://nieer.org/state-preschool-yearbooks).

[3]Evaluations of small-scale early education programs include Anderson (2008), Campbell et al. (2012), Duncan and Sojourner (2013), Heckman et al. (2013), and McCormick et al. (2006). Studies of Head Start and public preschool programs include Bailey et al. (2021), Durkin et al. (2022), Gray-Lobe et al. (2022), Kline and Walters (2016), Lipsey et al. (2018), Puma et al. (2010), Weiland and Yoshikawa (2013), and Weiland et al. (2020). While many children receiving child care subsidies (e.g., through the Child Care and Development Fund (CCDF)) attend community-accessible center-based arrangements, others attend a range of informal providers. Thus, studies of the CCDF are evaluating a mix of child care arrangements (Herbst and Tekin, 2016).

[4]Indeed, the child care entitlement program within the Build Back Better Act is predicted to increase enrollments in privately-owned, community-accessible center-based programs (Borowsky et al., 2022).

intervention that operated during the early-1990's—to examine whether the cumulative exposure to center-based settings during the first three years of life influenced children's cognitive and social skills as well as health. The CCDP randomized disadvantaged families with infants to receive case management and center-based child care for up to five years (Goodson et al., 2000; St.Pierre et al., 1997). Indeed, case managers were *mandated* to refer treated families to center-based providers in order to advance the CCDP's child development and school readiness goals. As a result, children in the treatment group were introduced to non-parental care early in life, with nearly 40% of such children enrolled in a center arrangement at 18-months. I also show that those in the treatment group used substantially more center-based care throughout the five-year demonstration than those in the control group. My methodology therefore uses the randomized offer to participate in CCDP as an instrument to estimate the local average treatment effect (LATE) of center-based enrollment on children's developmental outcomes.

Beyond its randomized design, the CCDP evaluation is well-suited for this analysis. First, parents responded to detailed surveys about their child care experiences at multiple points during the five-year demonstration. This paper takes advantage of four such interviews—conducted when the focal child was ages 18-, 24-, 30-, and 36-months—which included questions on both the participation and dosage margins for multiple care-types. Armed with this information, I define my primary variable of interest as the cumulative number of months each child spent in center-based care as of 36-months. Second, a variety of information was collected on child outcomes using a combination of direct assessments and detailed parent surveys. In addition, focal children were assessed at multiple ages until they reached five-years-old. This study examines the child's language and literacy skills, social skills, externalizing and internalizing behaviors, and health measured at 36-, 48-, and 60-months. Finally, the survey collected detailed information about the family, including parents' labor market outcomes, human capital and parenting skills investments, health, and drug and alcohol use, and means-tested program participation. Such information is crucial for understanding whether the randomized offer to receive CCDP services is correlated with other family outcomes beyond center care enrollment.

As with other early childhood interventions, treated families in the CCDP received access to center-based child care in addition to other supports like case management and parenting education. Indeed, those in the Infant Health and Development Program (IHDP)—previously used by researchers studying the impact of center-based child care—were offered a package of services ranging from mental health counseling and social service referrals to home visits and parenting education (McCormick et al., 2006). While the provision of these additional supports would seem to complicate efforts to isolate the effect of center-based enrollment, the IHDP resulted in only modest increases in service take-up, but had large effects on child care use (Brooks-Gunn, et al., 1994; Ramey et al., 1992). Thus, a number of studies have used this intervention to provide causal evidence on the developmental effects of center-based care (Chaparro et al., 2020; Duncan and Sojourner, 2013; McCormick et al., 2006).

The same dynamics were present in the CCDP demonstration: the treatment generated large increases in child care use without shifting other behaviors that might influence child outcomes. I provide several pieces of evidence in support of this argument. First, I summarize findings from the formal process and impact evaluations of the program (Goodson et al., 2000; St.Pierre et al., 1997). Results from the process study show that the CCDP was a low-dosage intervention: rates of service use within the treatment group were low and at comparable levels to the control group, participation in parenting education was well below its intended levels, and most parents reported no improvement in several domains of family functioning. Consistent with its low-dosage treatment, the impact evaluation found no effects of the program on child and parental well-being, leading the study's authors to conclude that "the combination of case management and parenting education, delivered through home visits, is not an effective means of improving developmental outcomes for low-income children" (Goodson et al., 2000).[5] Second, using the current study's analytic sample, I reanalyze the outcomes studied in the original evaluation. Altogether, I examine 60 outcomes measured when children were ages 36-, 48-, and 60-months, and my results provide confirmation that families in the treatment group were largely unaffected by the CCDP. Finally, I enlarge the set of instruments to allow for tests of the overidentifying restrictions as well as the simultaneous estimation of multiple components of the CCDP intervention. In one test, for example, I examine the effects of center care participation and parents' enrollment in parenting education classes. I find that while the impact of center care remains unchanged, in no case do the alternative services influence child development. Such evidence together suggests that my IV strategy is able to credibly identify the impact of center care enrollment.

Five key results emerge from this analysis. First, consistent with the CCDP's aim, children in the treatment group were substantially more likely to be enrolled in center-based child care than those in the control group. At the 18-month follow-up, they were 26 percentage points more likely to be attending center care; at the 36-month

[5] It was posited by the authors that CCDP's services were too diluted to be effective (St.Pierre et al., 1997).

follow-up, they remained about 18 percentage points more likely. Such differences are reflected in the results of the first-stage equation, which finds that treated children accrued nearly 4.5 more months of center care exposure as of 36-months, equivalent to a 74% increase in center care use from the mean.

Second, the ordinary least squares (OLS) estimates indicate that additional months spent in center-based child care is associated with improvements in language and social skills in the short-run (i.e., at 36-months) and medium-run (i.e., at 48- and 60-months). However, when I instrument for cumulative center enrollment using the randomized offer to receive CCDP services, I find mixed results, with positive short-run effects on language skills and negative effects on social skills. The estimates imply that a one-month increase in center care participation—equivalent to a 17% increase from the sample mean of 6.1 months—raises language and literacy test scores by 0.025 standard deviations (SD) while reducing scores on a measure of social skills by 0.021 SD. Second, the sub-group analyses reveal that the positive effects on language apply equally to boys and girls, but are larger for non-white children and those whose mothers have less education. The negative effects on social skills are driven by boys and non-whites.

Fourth, virtually all of the short-run effects of center-based care fade-out within one or two years, when focal children reach 48- and 60-months of age. One exception is girls, for whom more time spent in center care leads to a statistically significant reduction in language skills at 60-months. Specifically, a one-month increase in center-based enrollment at 36-months reduces 60-month test scores by 0.02 SD. Finally, I show that children who spend more time in center care as of 36-months are more likely to attend Head Start at 48- and 60-months. Such results imply that a type of "retention strategy" might be at work in which families actively seek out or are encouraged to enroll in similarly-organized education services throughout the preschool years. Such dynamics imply potentially large increases in the total amount of exposure to high-quality programs.

This paper contributes most directly to the small set of studies attempting to causally identify the impact of community-accessible child care, including center-based services, on child development. For example, Bernal and Keane (2011) instrument for total child care time during the preschool years using 78 social policy variables (e.g., welfare reform rules). Analyzing a sample of children of single mothers, the paper finds that each year of child care exposure reduces children's cognitive ability test scores by 2.1%. Another paper by Herbst (2013) instruments for child care time using seasonal variation in the demand for care services, finding that an additional month of child care exposure reduces test scores by 0.6%, with even larger reductions estimated for those in center-based care. There is also a related literature studying the impact of *publicly-provided* center-based programs set in an international context using randomized controlled trials (Bernal et al., 2019; Nores et al., 2019), lottery-based offers (Drange and Havnes, 2019), and regression discontinuity designs (Fort et al., 2020). This work uncovers mixed effects of center care exposure on academic skills.

By focusing on young children, this paper also contributes to a small literature estimating the developmental effects of infant and toddler child care attendance, with mixed results. Felfe and Lalive (2018) study the impact of county-level child care coverage rates for children ages 0 to 2 in West Germany, finding improved social-emotional skills among boys and disadvantaged families. Furthermore, Drange and Havnes (2019) use a lottery-based assignment mechanism to study the impact of being offered a child care slot in Oslo's public system at ages 1 and 2. The paper finds positive effects on tests of language and math skills at age 7. On the other hand, a recent paper by Fort et al. (2020) exploit the application and admission process rules within the Bologna Daycare System to study the impact of center-based enrollment for advantaged children ages 0 to 2 on cognitive skills at ages 8 to 14, finding that an additional month of child care exposure reduces ability test scores by 0.5%.

Finally, this paper contributes to a small literature that exploits the random assignment of some treatment for the purpose of generating instrumental variables (IV) estimates on a different variable of interest that is influenced by the treatment. For example, Crosby et al. (2010) exploit the random assignment of disadvantaged families to various welfare and employment programs to examine the impact of child care use on children's behavior problems. In addition, Auger et al. (2014) take advantage of the random assignment of different curricula within preschool classrooms to study the effect of classroom quality on children's cognitive skills. Similar methodologies have been used to estimate the effect of family income on child development (Morris and Gennetian, 2003), the impact of maternal education on child development (Gennetian et al., 2008), and the impact of maternal employment on domestic abuse (Gibson-Davis et al., 2005).

The remainder of the paper is organized as follows. Section 2 provides a detailed description of the CCDP demonstration as well as the analytic sample used in this paper. Section 3 introduces the IV strategy for recovering the LATE of center-based child care, while Section 4 presents the results. I provide a thorough discussion and interpretation of the results in Section 5, and Section 6 concludes the paper.

2 Comprehensive Child Development Program

2.1 Overview

Operating between 1990 and 1995, the CCDP demonstration delivered case management and early childhood education to economically disadvantaged families.[6] Each family was assigned a case manager who attempted periodic home visits beginning as soon as families were enrolled in the demonstration—usually in the focal child's first year of life—and continuing until the child entered school. Case managers worked with families to assess their goals and service needs, and make referrals to social services in the community. In addition, case managers were required to deliver early childhood education, which consisted of parenting education for the mother and arranging for the focal child to enroll in a child care program.[7] The goal was to link families to center-based services within the community, and as will be shown in Section 4, such programs were the predominant child care mode used by those in the treatment group.

As noted above, local CCDP administrators were *mandated* to make child care available to all treated families requesting it for the purpose of allowing parents to work or enroll in job training or education programs. Administrators took steps to ensure that children participated in high-quality center-based arrangements. Indeed, the programs were required to meet state licensing standards along with Head Start Performance Standards on child-teacher ratios and classroom group sizes, use of a developmentally appropriate curriculum and equipment, and teacher training. Program staff visited the facilities annually to monitor and assess quality by administering a modified version of the Early Childhood Environmental Rating Scale (ECERS). If a facility did not meet the established quality guidelines, it was deemed out of compliance, which prompted CCDP administrators to work with child care staff to improve service quality.[8]

Families received assistance from a variety of sources to defray the cost of these child care services (Smith and Lopez, 1994). Some families received a subsidy funded by one of the newly-created programs under the 1988 Family Support Act, while others received assistance from the Title XX program. Nevertheless, families generally did not have all of their costs covered, and were required to contribute a co-payment. At other sites, CCDP

[6] For detailed information on the CCDP and its evaluation results, see Goodson et al. (2000), St.Pierre et al. (1999), and St.Pierre et al. (1997)

[7] The parenting education component trained case managers to deliver a specific curriculum that focused on educating parents about infant and child development as well as parenting skills. Case managers did not work directly with children.

[8] Regrettably, the data on quality ratings were not made available to researchers conducting the evaluation of the CCDP.

administrators collaborated with local center-based providers to purchase a large number of slots at below-market prices, thereby allowing families to receive heavily subsidized care. Altogether, five of the project sites used CCDP funds to subsidize all or most of families child care expenses, while the remaining 16 projects used a mix of CCDP and other sources to subsidize costs.[9]

To be eligible for the CCDP, families were required to have an income below the federal poverty level, and mothers needed to be pregnant or have a child under age one.[10] Some families were recruited from hospitals, prenatal clinics, and other health programs, but most participants were on-boarded through door-to-door recruiting, in an attempt to reach communities' hardest-to-serve families. Grants were made available to a range of local entities (e.g., universities, public schools, and social service organizations) to administer the CCDP through a competitive process that identified highly-qualified bidders, defined as having a strong track record of providing comprehensive services to low-income families. Twenty-four sites in 22 states received grants in 1989 and 1990. The program sites were located in urban and rural areas: a total of 13 urban grantees were selected, while eight rural grantees participated in the CCDP.

The impact evaluation was conducted in 21 of the original 24 CCDP project sites. Families were recruited by grantees throughout 1990 and were randomly assigned to a program or control group. Grantees recruited more families than it could serve in order to facilitate the construction of a control group. At the time of recruitment, all families were administered a baseline survey, which collected information about the child's demographic characteristics; parent demographics, education, and employment status; and household income (St.Pierre et al., 1994).[11] Randomization was performed at the site-level under the supervision of research contractors. Those assigned to the program (or treatment) group were eligible to receive case management and child care for a maximum of five years, while those in the control group were not a "no services" group. They were free to use any educational, health, or child care services available in the community. The evaluation sample consisted of 4,410 families across 21 project sites, with 2,213 families assigned to the CCDP group and 2,197 families assigned to the control group. Projects

[9]Although the receipt of subsidized child care might imply that families' total income increased through a change in parental labor market earnings, the evidence presented in Appendix Table A1 shows that the CCDP did not influence parents' labor supply or earnings. Therefore, changes in employment and income are not likely to be mechanisms through which CCDP assignment affected child development, as will be discussed in more detail below.

[10]These children were designated as the "focus" children—with one such child allowed per family—who were given the battery of development tests.

[11]Note that there was a slight delay—spanning no more than a few months—between recruitment and enrollment in the CCDP. All but a small number of children were under age one at the time of recruitment, consistent with the program's goal. Information on child care use was not collected at baseline. The first major data collection on child care occurred during the 18-month follow-up.

began to deliver services in 1990, and evaluation data collection started in the fall of 1991. Detailed parent surveys and child development assessments were administered annually, beginning when children were 24-months old and continuing until 60-months. Smaller surveys were also administered when children were 18- and 30-months old.

2.2 Treatment Intensity

As noted above, the CCDP offered families case management and parenting education—in addition to center-based child care—in an attempt to improve child development. Like other evaluations of early childhood interventions with complex treatments, the credibility of this paper's research design requires that the randomized offer to receive CCDP services shifted child care enrollment without shifting other family behaviors that might influence child development (Chaparro et al., 2020; Duncan and Sojourner, 2013; McCormick et al., 2006). Below I begin to present evidence in support of this requirement. Additional evidence is discussed when presenting the IV model in Section 3.

Despite the CCDP's ambitious service delivery model, in practice it was a low-dosage intervention. For example, while parenting education was a key element of the demonstration, an independent process evaluation found that the amount of parenting education received by treated mothers was relatively weak, falling well below administrators' target level. In the first two years of the demonstration, parents received an average of 15 and 18 parenting classes, respectively, which amounts to a total of 16.5 hours of instruction over the two-year period (St.Pierre et al., 1997). While this alone might cast doubt on CCDP's parenting education component, the evidence suggests that parenting classes delivered in other contexts had little effect on child development (Barnett, 1995; Coy and Doyle, 2024; St.Pierre et al., 1995; Wasik et al., 1990).

Furthermore, with the exception of child care, rates of referred service use were low within the treatment group and at very similar levels to families in the control group. The receipt of mothers' dental care (treatment: 48%; control: 48%), mental health counseling (16%; 8%), preventive care (66%; 66%), acute care (40%; 40%), and well baby care (87%; 84%) were very similar across both groups (Goodson et al., 2000). Consistent with its low-dosage treatment, most parents reported that the CCDP did not help them achieve the goals they established with the case manager. Indeed, 11% reported an improvement in their parenting skills, 13% reported an improvement in their social supports, 17% experienced an increase in education, and 14% claimed that their child's intellectual and social development was enhanced (St.Pierre et al., 1999).

Also consistent with its low-dosage treatment, an independent evaluation of the CCDP—comparing a range of child and parent well-being outcomes across the treatment and control groups—was found to have no impact on family functioning. In particular, random assignment to the CCDP treatment had null effects on children's developmental outcomes, child health and health care access, parental health and well-being, parenting attitudes, parental employment and wages, household income, means-tested program participation, and the quality of the home environment (St.Pierre et al., 1999; Goodson et al., 2000).

To further probe the impact of CCDP within the current study's analytic sample, I reassess whether assignment to the CCDP treatment influenced the same set of outcomes. As shown in Appendix Table A1, the analysis focuses on health and education service receipt (Panels A and B), mother's health outcomes and health behaviors (Panel C), and family economic well-being (Panel D).[12] The results confirm that the CCDP had virtually no impact on a range of outcomes that might influence child development. Mothers and their children were no more likely to receive dental, medical, and occupational care services; mothers' health and risky behaviors like drug and alcohol use were unchanged; families' labor market outcomes did not improve; and families were no more likely to receive cash assistance. Importantly, there is only weak evidence that mothers assigned to the CCDP treatment were enrolled more intensively in school and parenting classes at some interview dates. Given that the response window for these survey items is the previous six months, the point estimates imply low-dosage participation in education services. For example, mothers at each interview date accrued about one additional hour of parenting education over the previous six months, and they attended school between three and 15 extra hours. Over a six-month period, the 15 hours of schooling (reported at 60-months) amounts to a little more than 30 minutes per week, on average. Together, such results are consistent with the argument that the CCDP was a fairly weak intervention.

2.3 Analytic Sample and Measures

This paper estimates the impact of the cumulative number of months children were enrolled in center-based care during the first 36 months of life. To construct the measure of child care participation, I rely on the 18-, 24-, 30-, and 36-month waves of data collection, when parents (in the treatment and control groups) were asked detailed questions about their children's care arrangements. In particular, parents were first asked about the

[12]For each model, the outcome is one of the listed variables measured at 36-, 48-, and 60-months. The independent variable is a binary indicator of an offer to participate in CCDP. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects.

type of care setting or caregiver regularly providing the most (i.e., the largest number of hours of) child care while they were working, looking for work, or at school/job training. Parents could select from a large number of options, ranging from parents/stepparents and other family members (e.g., siblings and grandparents) to non-family members (i.e., home-based services, friends, neighbors, and au pairs) and centers (i.e., group care facilities, nurseries and preschools, and Head Start programs).[13] Among those using any form of center-based care, non-relative care in the provider's home, or relative care in the provider's home, a question was then asked about the number of months the arrangement was used.[14]

At each interview, I calculate the number of months the focal child spent in center-based settings, defined as nursery schools, preschools, day care centers, and group care centers.[15] A value of zero is assigned to children using either parental care or a form of non-center child care. Limiting the sample to children with observed months-of-care at all interview dates, I create a variable that sums the total number of months children were reported to be enrolled in center-based care as of the 36-month survey. The full sample mean on this variable is 6.1 months; for the subset of children who attended any center-based care, the mean number of months of enrollment is 12. Given that the survey inquires about one arrangement for each child—the primary arrangement, or the one used most heavily—the months-of-care variable reflects the time spent in the primary arrangement, rather than the time spent in all arrangements.

This study examines three sets of child outcomes. First, children were administered the Peabody Picture Vocabulary Test (Revised) (PPVT), which is a frequently used measure of receptive vocabulary (Dunn and Dunn, 1981; Auger et al., 2014). This test was given to children at ages 36-, 48-, and 60-months, providing an opportunity to study the short- and medium-run implications of attending center-based care. Second, I examine measures of children's social and emotional development, first through the Adaptive Behavior Inventory (ABI), which is a 30-item test of social competence (Hogan et al., 1992). Administered by parents at the 36- and 48-month assessments, the ABI was designed to be sensitive to social behaviors that may be influenced by children's early child care

[13] Those using non-center arrangements were asked a follow-up question about whether that care occurred in the child's home or in the home of the provider.

[14] In other words, only those using non-parental, out-of-home care were asked about months-of-use. The recall period was 12 months during the initial child care interview; during each subsequent interview the recall period was shortened to 6 months. Given that the first comprehensive interview occurred at age 18-months, the initial 12-month recall means that for some children the measure of center care enrollment may not capture participation during the first 6 months of life.

[15] By measuring center-based child care enrollment up to age 36-months, I avoid capturing those in Head Start. Given that families in the CCDP evaluation are highly disadvantaged, it is not surprising that a sizable number of children are observed using Head Start at the 48- and 60-month surveys. Indeed, Table 8 shows that 16% of those in the control group were enrolled in Head Start at both interview dates, with even higher shares reported for those in the treatment group.

experiences, particularly among those from disadvantaged families (Goodson et al., 2000). The ABI produces a score measuring overall social skills as well as three domain-specific scores in Compliance, Expressiveness, and Disruptive Behavior.[16] I also explore scores on the Child Behavior Checklist (CBC), a parent-administered measure of behavior problems when children were 60-months old (Achenbach, 1991). The CBC produces a total score based on separate measures of externalizing and internalizing behaviors. The former scale inquires about the frequency of acting-out behaviors, including arguing, fighting, and anger, while the latter asks about the frequency with which children display anxiety, loneliness, low self-esteem, and sadness.[17]

The final outcome is a summary index of children's health outcomes and health behaviors. Specifically, the index combines a measure of children's overall health status (based on a four-point scale ranging from "poor" to "excellent"), the number of serious accidents or injuries (up to three) that occurred during the recall period (e.g., car accidents or poisonings resulting in broken bones, punctures, or illness), the number of health problems (up to three) identified during the recall period (e.g., lead poisoning, anemia, or dental issues), whether the child is up to date on her immunizations, and whether the child brushes her teeth regularly. Each variable is reported by the parent during the 36-, 48-, and 60-month interview. To construct the index, I first recode some of the variables so that higher values indicate more favorable health outcomes. I then standardize each variable to have a mean of zero and a standard deviation of one, and sum the values of the five standardized variables.[18]

The regression models control for a number of baseline child and family characteristics. The child-level controls include indicator variables for gender, firstborn, and non-white. The family-level controls include indicator variables for mother's marital status, mother's and father's employment status, residence in public housing, and English as the primary spoken language.[19] I also control for mother's age (in years) at the birth of the first child, mother's education (in years), and the log of per person household income. Finally, I include in the models a variable for the child's age (in months) at the time of each developmental assessment (i.e., 36-, 48-, or 60-months).

[16]The Compliance and Expressiveness domains together create another domain called Pro-Social Behavior. I investigate this combined domain as well. Higher scores on the total ABI and in the Compliance, Expressiveness, and Pro-Social domains indicate better social skills, while higher scores in the Disruptive domain indicate worse skills.

[17]Higher scores on the CBC indicate more behavior problems.

[18]Although the main analysis reports results on the full health index, I also examine each variable separately; in no case do the results on the individual elements differ from those on the health index.

[19]All survey questions about the mother's partner (e.g., baseline employment status) are answered by the mother, including cases where the partner does not reside in the household.

3 Empirical Methods

3.1 Model

To identify the impact of center-based enrollment on child development, I exploit the random assignment of families to either the CCDP or control group as a way of generating exogenous variation in child care participation. As described previously, those assigned to receive CCDP services were strongly encouraged by case managers to enroll their child in community-accessible center-based programs, thereby leading to large differences in center care use between those in the treatment and control groups, as will be shown below. This allows me to use the randomized offer to obtain CCDP services as an instrument for the amount of time spent in center-based care. In particular, my empirical strategy consists of the following equations:

$$Y_{ist} = \beta CC_{ist} + \alpha X^{'}_{ist} + \zeta_{st} + \varepsilon_{ist} \tag{1}$$

$$CC_{ist} = \gamma CCDP_{ist} + \alpha X^{'}_{ist} + \zeta_{st} + \nu_{ist}, \tag{2}$$

where Y_{ist} is a developmental outcome for child i located in study site s and born in year t, CC_{ist} is the cumulative number of months child i attended center-based child care in the first 36-months of life, and $CCDP_{ist}$ is a binary indicator equal to one for children randomly assigned to the treatment group and zero for children assigned to the control group. Recall that the child outcomes are measured at 36-, 48-, and 60-months. Both equations control for the baseline child and family characteristics, including children's age-at-assessment ($X^{'}_{ist}$), and for site-by-birth year fixed effects (ζ_{st}). The fixed effects account in a non-parametric manner for the fact that children in the CCDP demonstration were born over the course of four years, and thus were likely enrolled in the evaluation at different times across the sites.

The coefficient of interest is β, which represents the change in some child outcome Y_{ist} for a one month increase in center-based child care enrollment. The model is estimated by two-stage least squares (2SLS), such that the first-stage equation (Equation 2) produces predicted values of CC_{ist} using the variable $CCDP_{ist}$ as the instrument. The predicted values for months of center-based enrollment are then used to construct the variable of interest in the second-stage equation (Equation 1) estimating the impact of such enrollments on child development. The standard errors in all models are adjusted for clustering at the site-level.

The coefficient β represents the local average treatment effect (LATE) of center-based attendance on children who—because they were assigned to the CCDP group—used this particular type of service. Therefore, the LATE in this context captures the impact of *enrolling* in center-based care, which is a margin of key policy interest. Note also that the instrumental variables (IV) estimates apply to the sub-group of "compliers." A complier in this context is a child who was randomized to the CCDP group and enrolled in center-based care or who was randomized to the control group and did not enroll in center-based care. The IV estimates do not reflect impacts on "never takers" (i.e., those who would not enroll in center-based care even if they were assigned to the treatment group) or "always takers" (i.e., those who would use center-based care even if they were assigned to the control group).

3.2 Counterfactual Child Care Choices

Given that the 2SLS model described above produces a LATE estimate for compliers, it is important to understand the kinds of child care arrangements used by those in the control group. These counterfactual alternatives will provide insight into what center-based care is compared with in this context. Table 1 provides such an analysis, first by presenting the share of children in the control group who participated in various child care modes (i.e., parent, relative, non-relative, and centers) at each interview date. The table also estimates regressions of participation in each care arrangement on a binary indicator equal to one if a family was assigned to the CCDP group or zero if it was assigned to the control group.

As shown in column (1a), nearly 60% of children in the control group were cared for by a parent at each interview date, making it the predominant counterfactual care mode. The estimates in column (1b) reveal that those in the control group were between 14 and 19 percentage points more likely (than those in the treatment group) to be in parent care. In addition, between 14% and 22% of control group children were in relative care, depending on the interview date, and they were three to nine percentage points more likely to participate in such care [column (2b)]. Relatively few children were involved in non-relative arrangements [column (3a)], and the participation gap between treatment and control children is small and statistically insignificant [column (3b)]. Finally, column (4a) shows that while participation in center-based arrangements increased over time among those in the control group, from 12% to 21%, they were substantially less likely to use such care than those in the treatment group (i.e., between 17 and 26 percentage points less likely). These data imply that the offer to receive CCDP services encouraged families to shift primarily from parent care, but also from relative care, to center-based services. Therefore, this study compares children in center-based care with those who are either not participating in non-parental child care at all or who are involved in the most informal non-parental care arrangements, such as relative care.

3.3 Covariate Balance and Attrition

I now examine two issues that are central to the validity of the randomized research design. First, Table 2 checks whether the pre-treatment child and family characteristics are balanced across the treatment and control groups.[20] Column (1) shows that, as expected, the analytic sample is comprised of highly disadvantaged families. Approximately 71% of control group children are non-white, while mothers have less than a high school degree, on average, and only 13% are employed at baseline. A somewhat larger share of fathers (or partners) were working at baseline (17%), and over one-third (35%) of families were living in public housing.

Columns (2) and (3) conduct the balance tests by estimating regressions of each characteristic on the binary indicator for CCDP assignment. Column (2) estimates the model without controls, while column (3) adds a set of site fixed effects. The estimates imply that the baseline characteristics are well-balanced across the treatment and control groups, and adding site fixed effects does little to change the estimates, as one would expect under random assignment. The only imbalances that emerge are for parent's employment status, showing that those in the treatment group are more likely to be working at baseline. I handle these imbalances by controlling for mother's and father's employment status (and all other characteristics in Table 2) in the reduced form and IV analyses.

Second, I investigate the degree of outcome missingness and whether such missingness differs across the treatment and comparison groups. This analysis is particularly important for the current study, which relies on multiple child assessments spanning a long time period (five years) for a highly disadvantaged sample of families. Differential attrition may impart a form of selection bias onto the estimates if changes in the composition of the sample become confounded with treatment assignment. Column (1) of Table 3 displays the share of children in the analytic sample with missing information on each outcome. Column (2) estimates regressions in which the outcome is a binary indicator equal to one if a given child is missing information on each listed outcome, and the key independent variable is the binary indicator for CCDP assignment.[21] All models also control for site fixed effects. I find that the

[20] All regressions are restricted to families with non-missing data on each baseline characteristic, and to families with child care information available at 18-, 24-, 30-, and 36-months. The number of observations ranges from 1,530 to 1,702.

[21] The sample is restricted to families with child care information available at 18-, 24-, 30-, and 36-months. The number of observations for all analyses is 1,702.

outcome data are missing at fairly low rates (2% to 15%), and that treatment-control differences in missingness are small (one to four percentage points).[22] As a further check on differential attrition, Appendix Table A2 assesses whether the availability of outcome data is correlated with the baseline child and family characteristics. Here, I estimate separate regressions in which the outcome is a binary indicator equal to one if a given child is missing information on each listed outcome, and the independent variables are the listed baseline characteristics. The results confirm that outcome missingness is largely independent of the child and family characteristics.

Given that the analytic sample is restricted to families with complete child care information available—defined as having participated in the child care interview at 18-, 24-, 30-, and 36-months—it is also important to examine whether there are imbalances in the availability of child care information across the baseline characteristics. Appendix Table A3 examines this issue by estimating regressions of a binary indicator equal to one if a family participated in all of the child care interviews on the set of listed baseline characteristics, controlling for site fixed effects. The results reveal few differences in the composition of the full CCDP sample across those with and without child care information available at every interview date. Key exceptions are families whose focal child is the firstborn (less likely to provide full information), those whose father is employed (more likely to do so), and those whose primary language is English (more likely to do so).

3.4 Exclusion Restriction

A key requirement for all IV analyses is that the instrument must meet the exclusion restriction. The exclusion restriction stipulates that the instrument influences the outcome(s) only through its effect on the independent variable of interest. In the context of this study, it assumes that random assignment to the treatment group influences children's developmental outcomes only through changes in center-based child care enrollment. In other words, the exclusion restriction defines a single, indirect pathway through which the instrument—the randomized offer to receive CCDP services—affects child development, and that is via changes in child care participation.

As described in Section 2, the CCDP aimed to improve child outcomes by offering families case management and parenting education in addition to center-based child care programs. It is therefore possible that one or more

[22] These missingness figures are comparable to several recent studies of early childhood education interventions using similar methodologies. For example, Gray-Lobe et al. (2022) report outcome missingness on up to 15% of the sample, with treatment-control differentials up to four percentage points. Weiland et al. (2020) reports outcome missingness on up to 16% of the analytic sample, with treatment-control differentials up to five percentage points. Finally, Durkin et al. (2022) report rates of outcome missingness between 10% and 13%.

of these additional supports had an independent effect on child development, which would violate the exclusion restriction. However, recall that a comparison of a wide range of outcomes across the treatment and comparison groups (shown in Appendix Table A1) found virtually no impact of the CCDP intervention on family well-being or parents' schooling and labor market outcomes. Such results are consistent with an independent evaluation of the program, and they accord with the low take-up of program services by treated families. Such results indicate that the CCDP had little influence on the outcomes that would confound the impact of center care enrollment, thereby providing support for the exclusion restriction.

Nevertheless, I undertake two additional tests to probe the validity of the IV. To do so, I expand the instrument set to include treatment status-by-site interactions, for a total of 20 identifying instruments. As discussed in the next section, these variables are extremely strong determinants of center care enrollment in the first-stage equation. Given that random assignment to the CCDP was performed at the site-level, these interactions should be orthogonal to the determinants of child development. Increasing the number of instruments is advantageous in this context, first because it allows for a test of the exogeneity of the overidentifying instruments. Specifically, I implement the Anderson-Rubin test, which examines the null hypothesis that the instruments are validly excluded from the second-stage equation (Kiviet and Kripfganz, 2021).

Furthermore, the expanded instrument set can be used to identify the impact of multiple endogenous variables. In robustness checks, I examine various combinations of center care participation as well as the receipt of other services offered to treated families, including enrollment in school or parenting classes. Because 2SLS estimates are shown to be biased toward ordinary least squares (OLS) estimates as the number of instruments increases, I estimate the overidentified models using a limited information maximum likelihood (LIML) estimator, which adjusts for this many-instruments bias (Bound et al., 1995; Hansen et al., 2008). Results from these robustness exercises, which will be discussed below, indicate that the exclusion restriction is satisfied.

4 Results

4.1 Main Results

Before summarizing the results from the IV models, it is useful for comparison purposes to briefly discuss estimates from ordinary least squares (OLS) regressions of child outcomes. Results from these models, which are presented in Appendix Table A4, are based on the same analytic sample and include the same control variables as the IV models. Generally speaking, the results indicate that more time enrolled in center-based child care in the first three years of life (hereafter referred to as *CC*) is associated with higher PPVT scores and better social skills, as measured by the ABI. The coefficients imply that a one-month increase in *CC* participation is associated with an improvement in PPVT scores of 0.23 points at 36-months and 0.22 points at 48-months and 0.18 point at 60-months. These correspond to effect sizes of 0.016, 0.013, and 0.011 standard deviations (SD), respectively. Estimates from the model of total ABI indicate that a one-month increase in *CC* enrollment is associated with a 0.12% percent (0.006 SD) improvement in overall social skills at 36-months, an estimate that drops to a statistically insignificant 0.06% (0.003 SD) by 48-months. Finally, the OLS estimates are not indicative of a change in child health, given that the coefficients are small in magnitude and never statistically significant.

Table 4 provides the baseline IV estimates for the impact of *CC* enrollment on child development at 36-months. These results should be interpreted as the short-run effect of child care participation, since the 36-month outcomes are the most proximate to the timing of *CC* measurement. As shown in column (3), estimates from the first-stage equation are highly statistically significant, indicating that children in families randomly assigned to receive CCDP services accrued an additional 4.4 months of *CC* use during the first 36 months of life. Given that the sample mean number of months spent in *CC* is 6.1, the estimates imply that assignment to CCDP increased the time spent in center care to 10.5 months, on average. The F-statistic on the instrument, *CCDP*, is consistently at or above 35, indicating that it safely exceeds the critical value in a weak instruments test (Stock and Yogo, 2005). The reduced form estimates, presented in column (4), indicate that assignment to the CCDP treatment increased children's PPVT scores by 1.6 points but reduced their (total) ABI social skills scores by 1.8%. Although most of the domain-specific scores are indicative of negative effects, the largest reductions are estimated in the Compliance domain, whose scores declined 2.3%. While negatively signed, the coefficient on *CCDP* for the health index suggests that children assigned to receive CCDP services did not experience any changes in health outcomes.

Column (5) presents results from the IV model estimated by 2SLS. The findings continue to show that more time spent in *CC* increases children's 36-month PPVT scores. Moreover, the 2SLS point estimate is larger than its companion OLS estimate, implying that a one-month increase in *CC* enrollment raises PPVT scores by 0.37 points. This corresponds to an effect size of 0.025 SD. Furthermore, estimates on the ABI scores become *negative*, implying reductions of 0.41% in total social skills, 0.51% in Compliant behavior, 0.36% in Expressive behavior, and 0.40% in Pro-Social behavior for a one-month increase in *CC* participation. These impacts are equivalent to effect sizes of -0.021 SD, -0.017 SD, -0.018 SD, and -0.019 SD, respectively. The coefficient on *CC* enrollment in the model for Disruptive behavior remains positively signed, comparatively small in magnitude, and statistically insignificant. Finally, there is a null effect of *CC* enrollment on the health index.[23]

It is useful to compare these effect sizes with other determinants of child outcomes within the CCDP analytic sample. In particular, I focus on contrasts using mothers' education and children's race/ethnicity. Children whose mothers have at least some college education (i.e., at least 14 years of education) score 0.34 SD higher on the PPVT, while the white/non-white test score gap is estimated to be 0.50 SD. The estimates imply that the impact of an additional six months of *CC* enrollment (i.e., the sample mean) is equivalent to 45% of the college education gap in PPVT scores and 30% of the white/non-white test score gap. Looking at the total ABI, the results indicate that children of college educated mothers score 0.40 SD higher, and white children score 0.37 SD higher. The estimated effect of an additional six months of *CC* enrollment is equivalent to 31% of the college education gap and 34% of the white/non-white social skills gap. These benchmarks imply that center-based participation has economically meaningful short-run effects on disadvantaged children's developmental outcomes.

The short-run effects of *CC* enrollment fade-out entirely by the 48-month assessment, as shown in Panel A of Table 5. The reduced form effect on PPVT scores decreases by over 55% relative to the 36-month assessment, while the corresponding IV estimate falls by 59%, and neither estimate is statistically significant. The negative effects on social skills also fade, such that none of the coefficients on *CC* participation remain statistically significant for the ABI outcomes. For example, the IV estimate for total ABI decreases approximately 63% relative to the 36-month assessment, while the domain-specific score for Pro-Social behavior is reduced by 53%. Panel B of Table 5 provides the analogous results for the 60-month assessment. Interestingly, the IV estimate on *CC* enrollment flips signs in the model for PPVT scores, implying that center care use has a negative effect on test scores, although the coefficient is comparatively small in magnitude and not statistically significant. In addition, the results continue to show no discernible effect on the measure of behavior problems or the health index.

Before turning to the remaining results, it is important to assess whether the exclusion restriction in the 2SLS

[23] To ensure that the results discussed here are not sensitive to differences in the analytic sample, as shown in column (2), I estimate the 2SLS models on a common set of observations (N=1,505). Results are very similar to those presented in the table.

model is likely to hold in this context. As previously discussed, the results presented in Appendix Table A1 reveal that assignment to the CCDP group had little influence on a range of outcomes that might have implications for child development. While these results are promising, I undertake two additional tests that shed light on whether the exclusion restriction is met. To set the stage for these diagnostic tests, I overidentify the IV model so that the instrument set includes 20 CCDP-by-site interactions, and then estimate it using LIML. Column (6) of Tables 4 and 5 present these alternative IV estimates. It is noteworthy that increasing the number of instruments (and utilizing a different estimator) does not significantly change the point estimates, although it does generate smaller standard errors. This is not surprising given that the enlarged instrument set is highly statistically significant in the first-stage equation.

In the first diagnostic, I implement in column (7) of Tables 4 and 5 the Anderson-Rubin test of the overidentifying restrictions on the LIML models. A separate test is performed for each outcome measured at 36-, 48-, and 60-months. The results provide evidence in support of the exogeneity of the instruments. Indeed, the p-values indicate that the null hypothesis of valid overidentifying instruments cannot be rejected for any of the 36-month outcomes at the 95% level, and it can be rejected only twice across the 48- and 60-month outcomes. Similar results are obtained when the test of overidentifying restrictions is performed on the 2SLS estimates using the Sargan test.

In the second diagnostic, I use the expanded instrument set along with the LIML estimator to generate IV estimates on two variables of interest: *CC* enrollment *and* a binary indicator for whether the mother participated in formal schooling, vocational classes, or parenting classes.[24] I focus on these three variables because they were shown in Appendix Table A1 to be moderately correlated with assignment to the CCDP treatment. Given the evidence that parental employment and income may have implications for child development, it might seem reasonable to include such characteristics as additional endogenous variables in this analysis. However, as shown in Appendix Table A1, CCDP assignment is uncorrelated with parental employment and mothers' earnings, thereby invalidating the CCDP assignment indicator as an instrument for these variables. Since weak instruments can impart substantial bias on the variables of interest, the IV analysis described here is not undertaken on these characteristics (Bound et al., 1995).

Two noteworthy findings emerge from these analyses. First, the alternative variables of interest are rarely

[24]In other words, I estimate three models: one that includes *CC* enrollment and participation in formal schooling as the endogenous variables, another that includes *CC* enrollment and participation in vocational classes, and a final model that includes *CC* enrollment and participation in parenting classes. Each model is estimated on the full set of outcomes measured at 36-, 48-, and 60-months.

statistically significant. Of the 51 models estimated, the coefficient on these variables is significant in only two cases: formal schooling in the model for 48-month PPVT scores and parenting classes in the model for 48-month PPVT scores. In both cases, however, the coefficient on these variables is implausibly large, most likely because they have weak (first-stage) associations with CCDP assignment. Second, instrumenting for these additional variables does not appreciably change the estimated effect of *CC* enrollment. For example, in the model for 36-month PPVT scores, a one-month increase in center care enrollment increases test scores by 0.25 points, 0.42 points, and 0.19 points when the secondary variables are formal schooling, vocational classes, and parenting classes, respectively. In the model for 36-month overall ABI scores, the coefficients imply reductions of 0.38%, 0.44%, and 0.40%, respectively. Together, this evidence indicates that the CCDP-induced increase in *CC* enrollment is driving any change in child outcomes, thus providing additional support for the exclusion restriction.

4.2 Robustness Checks

Table 6 conducts some specification checks on the baseline IV model. I present estimates from the just-identified 2SLS model, where the outcomes are PPVT scores, total ABI and CBC scores, and the health index measured at 36-, 48-, and 60-months. The models in the first row omit the controls for mother's and father's baseline employment status, while those in the second row omit all of the baseline child and parent characteristics. These models are important because, as shown in Table 2, those in the treatment group are more likely to be working at baseline. The models in the third row express *CC* as a binary indicator equal to one if a given child ever enrolled in center-based care by 36-months.

Generally speaking, the results are robust to these specification changes. Looking at the first two rows in Table 6, I find that the estimates are not sensitive to the removal of any baseline characteristics. Indeed, I continue to find that children's 36-month PPVT scores are increasing in *CC*, while their overall social skills are decreasing. As with the baseline results, both effects fade-out and become statistically insignificant by the 48-month follow-up. Also consistent with the baseline results is that center care attendance does not influence children's health outcomes. Recall that the main IV specification found that a one-month increase in *CC* enrollment raises PPVT scores at 36-months by 0.025 SD. The equivalent effect sizes in the robustness checks presented in rows one and two (of column (1)) are 0.028 SD and 0.032 SD, respectively. In addition, the estimated reduction in total social skills is -0.021 SD in the baseline IV model; the corresponding effect size in rows one and two (of column (4)) is -0.017 SD.

Such similarities are reassuring, implying that any differences in the baseline characteristics are not driving the results.

When measured as a binary indicator of any center-based enrollment, as shown in the third row, the coefficients imply a nearly eight-point increase in PPVT scores and an 8.6% decline in social skills at 36-month months, corresponding to effect sizes of 0.53 SD and -0.43 SD, respectively. Both effects fade-out by the time children reach 48-months, and none of the coefficients have statistically significant impacts on the health index. Although many studies in the early education literature estimate effect sizes at the participation margin, some caution is warranted in comparing such effects across studies, given their reliance on tests that lack a relevant cardinal scale (Drange and Havnes, 2019).

4.3 Heterogeneous Effects

Having established the robustness of the baseline IV results, I now turn to an exploration of the heterogeneous effects of *CC* enrollment. Table 7 displays the IV estimates from the just-identified 2SLS model, where the outcomes are the PPVT scores, total ABI and CBC scores, and the health index measured at 36-, 48-, and 60-months. I present separate estimates for boys and girls, non-white and white children, and low- and high-education mothers.[25]

The results suggest that the positive short-run effect of *CC* enrollment (on 36-month PPVT scores) applies about equally to boys and girls but is concentrated on non-white children and those with low-education mothers [column (1)]. However, these positive test score effects fade-out for all children, and for girls the impact becomes negative and statistically significant at 60-months [columns (2) and (3)]. In addition, recall that *CC* participation had negative short-run effects on children's social skills. The sub-group analyses indicate that these negative effects are driven by boys and non-white children [column (4)]. However, while the social skills deficit persists among boys to the 48-month follow-up, there is no evidence of behavior problems for any sub-group at the 60-month follow-up [columns (5) and (6)]. Finally, white children experience a short-run improvement in health [column (7)], although this effect fades entirely as of the longer-run follow-ups [columns (8) and (9)], and no other demographic group is found to have experienced a health improvement.

I examine one other source of heterogeneity: the impact of *CC* enrollment over the distribution of PPVT scores measured at 36-, 48-, and 60-months. To do so, I create separate binary indicators equal to one if a given child's

[25] The category "low-education" includes mothers with no more than 11 years of completed schooling, while "high-education" includes mothers with more than 11 years of schooling.

PPVT score exceeds the 10th, 25th, 50th, 75th, and 90th percentiles of the distribution. The cut-offs are based on the distribution of test scores within the control group. I then estimate the just-identified 2SLS model, where the outcomes are the binary indicators for exceeding a certain percentile cut-off. Results for the 36-month PPVT reveal positive effects of CC enrollment throughout the test score distribution, with comparatively large and statistically significant effects at the top of the distribution: a one-month increase in CC exposure increases the likelihood of scoring above the 10th, 25th, 50th, 75th, and 90th percentiles by 0.62, 0.50, 0.79, 0.97, and 0.97 percentage points, respectively, with the latter three estimates being statistically significant. The corresponding estimates for the 48- and 60-month PPVT scores are mostly small in magnitude, and none are statistically significant. Of note is that the estimates imply negative effects of CC enrollment throughout most of the 60-month test score distribution, with the estimates at the 10th and 25th percentiles showing the largest reductions.

4.4 Subsequent Use of Early Childhood Education Services

While the discussion above focuses on a variety of maternal- and family-level outcomes that might influence child development, a related issue is whether early center-based child care participation—measured, in this case, up to 36-months of age—affects the use preschool education services at older ages. I investigate this question by estimating the IV model described in Equations 1 and 2 on three outcomes, all of which are measured as binary indicators. Specifically, I study whether a given child was cared for exclusively by the parents (i.e., no non-parental care), whether the child participated in center-based care (i.e., nursery school, preschool, or center care facility), and whether the child participated in Head Start. All three outcomes are measured at the 48- and 60-month interviews.

Two key considerations motivate this analysis. First, the transition from formal modes of child care use (e.g., center-based services) very early in life to other forms of preschool or Head Start at older ages could be viewed loosely as a "family retention strategy" in which families actively seek out or are encouraged to enroll in similarly-organized education services throughout the preschool years and even into kindergarten and elementary school (Weiland et al., 2020). Indeed, Johnson et al. (2014) find that toddlers using publicly-subsidized center-based care are more likely to enroll subsequently in public pre-K or Head Start, while Gormley et al. (2018) show that children participating in public pre-K are more likely to enroll in the public school system. The current paper tests a related proposition, asking whether exposure to formal, center-based services early in life is correlated with the later use of the same service-types. A second consideration is the possibility that the impact of educational experiences very early in life could be sustained or even enlarged if those experiences are paired with complementary inputs at older ages.[26] Indeed, Bailey et al. (2017) argue the importance of "sustaining environments" in early childhood, in which the quality of post-intervention environments are critical for sustaining the impact of the intervention. If sustaining environments are in fact important ingredients to a successful early childhood program, then understanding whether and how families' child care preferences are sustained is an important issue for researchers and policymakers.

Table 8 reports 2SLS and LIML estimates on *CC* enrollment for the subsequent use of child care and early education services. As shown in column (1), around half of control group children rely exclusively on parent care at ages 48- and 60-months, between 11% and 14% use center-based providers, and 16% are enrolled in Head Start. Generally speaking, the IV estimates reveal that families' early and later child care choices are correlated: the use of formal child care early in life increases participation in similar care services at older ages. The 2SLS estimates in column (3) imply that a one-month increase in *CC* exposure by 36-months leads to a 1.4 percentage point increase in the probability of using center care and a 1.5 percentage point increase in the probability of using Head Start at 48-months. These increases are offset by a reduction in parent care of 1.8 percentage points for a one-month increase in *CC* enrollment. At 60-months, the linkages are more tenuous. While there is no discernible effect on parent care, I find that the increase in Head Start participation persists to the 60-month interview, while the persistence of center-based enrollment is weaker.[27]

5 Discussion

The current paper has uncovered four key findings. First, I find that additional months spent in center-based child care (as of 36-months) has positive short-run effects on language and literacy skills but negative effects on social skills. Second, the positive effects on language and literacy are comparatively strong among non-white children and those whose mothers are low-education, while the negative effects on social skills are driven by non-whites and boys. Third, nearly all of the short-run effects of center care fade-out by the time children reach 48- and 60-months of age. One exception is girls, for whom more time spent in child care leads to a reduction in 60-month PPVT scores. Finally, I find that center care exposure early in life is predictive of later center-based and Head

[26] Conversely, it seems equally reasonable that any skill development resulting from early intervention would fade if those skills are not subsequently cultivated.

[27] At 60-months, *CC* enrollment primarily reduces the likelihood of using relative care.

Start program participation. In this section, I discuss these findings in the context of previous work and attempt to address any implications for policy.

5.1 Who Benefits from Center-Based Child Care?

Central to the discussion of early childhood education is whether new programs or expansions to existing ones should target children in economically disadvantaged families or whether such programs should be available to children regardless of family income level. An important consideration in this debate is who benefits from such programs and by how much. The CCDP provides an opportunity to examine heterogeneity across sub-sets of poor children, making comparisons to the Perry, Abecedarian, and Head Start programs particularly relevant.

The results indicate, first, that more time spent in center-based care generates about equal-sized increases in test scores for boys and girls in the short-run. This result is consistent with a meta-analysis showing that boys and girls benefit about equally on cognitive and school readiness measures after participating in early education programs (Magnuson et al., 2016). It is also consistent with some individual studies of long-term outcomes measuring schooling attainment and labor market success among those who attended the Head Start (Bailey et al., 2021) and Perry Preschool (Heckman et al., 2010) programs. In addition, I find that boys' social skills were adversely affected by center-based enrollment, while such programs had no impact on girls. These results appear to be inconsistent with the meta-analysis by Magnuson et al. (2016), which finds no effect on the behavioral outcomes of boys and girls of attending early education programs. However, a recent study of demand-side child care subsidies—which encourages low-income families to use community-accessible center care—increases externalizing and internalizing behavior problems among boys but not girls (Herbst and Tekin, 2016).

Second, the heterogeneity analysis reveals a complicated set of effects of center-based care across non-white and white children. For non-white children, there is an immediate positive effect on test scores (i.e., at 36-months), while white children do not show any short-run gains. By 48-months, however, the test score gains for non-white children fade-out entirely, but large positive effects emerge for white children. The 60-month results continue to show test score gains among whites and sizable reductions among non-whites, although neither estimate is statistically significant. Furthermore, the reduction in social skills appears to be driven exclusively by non-white children. Together, these results imply that white children might benefit more from enrolling in center-based child care, a pattern that is largely consistent with Currie and Thomas (1995)'s analysis of Head Start, which finds that white children experience substantially larger test score gains as well as reductions in the likelihood of repeating a grade. At least one study of Head Start's longer-run effects on educational attainment and earnings similarly shows larger positive effects for white children (Garces et al., 2002), but more recent work finds about equal-sized effects on a summary index of human capital characteristics (Bailey et al., 2021).

Finally, the results indicate that children with mothers without a high school diploma benefit substantially in the short-run from attending center care, while such services have null effects on those whose mothers have at least a high school diploma. This finding is consistent with a number of studies showing that early education programs have larger effects on children from more disadvantaged families. Indeed, Drange and Havnes (2019) show that lottery-based offers to receive center care in Oslo have larger impacts on children from low-income and -education families, while Weiland and Yoshikawa (2013) report larger short-run effects of Boston's pre-K program on low-income children. It is also interesting to note that early education policies and programs having negative consequences for child development often show concentrated effects on economically advantaged children, with null effects on their disadvantaged counterparts (Bernal and Keane, 2011; Herbst, 2013; Herbst and Tekin, 2016).

5.2 Fade-Out

One of the key results in this paper is that the positive (test score) *and* negative (social skills) results largely fade-out one year after the end of the child care measurement period. The lack of persistence in program impacts in the medium-run is the subject of considerable discussion because of the pervasive nature of fade-out documented throughout the literature (e.g., Bailey et al., 2017). For example, randomized controlled trial evaluations of the Head Start program and Tennessee's Voluntary Pre-Kindergarten program uncovered positive end-of-program effects on cognitive skills that faded by the end of kindergarten (Lipsey et al., 2018; Pion and Lipsey, 2021; Puma et al., 2010).[28] Furthermore, an evaluation of the Infant Health and Development Program (IHDP) found sizable positive effects on cognitive ability at ages two and three, followed by null effects starting at age five (Duncan and Sojourner, 2013). Finally, Boston's pre-K program had null effects on third grade outcomes (Weiland et al., 2020), despite having positive end-of-program effects on children's academic skills (Weiland and Yoshikawa, 2013).[29]

Despite the evidence discussed above, a lack of medium- to longer-run positive effects of early childhood programs

[28] A recent follow-up study of the Tennessee pre-K program uncovers negative longer-term effects, with children randomized to attend pre-K scoring lower on tests of academic skills and exhibiting more behavior problems in the sixth grade (Durkin et al., 2022).

[29] Some caution is warranted in interpreting the third grade effects of Boston's pre-K program as evidence of fade-out. The end-of-program estimates were generated on a different sample and using different empirical methods relative to the third grade study.

is not an immutable fact. Indeed, recent work by Ladd et al. (2014) find positive effects on third-graders of the roll-out of financing for pre-K slots in North Carolina, while Gormley et al. (2018) uncover persistence in Tulsa's school-based pre-K program on middle-school children. Furthermore, Drange and Havnes (2019) show that lottery-based offers to attend Oslo's child care institutions at ages one and two have positive effects on test scores for children at ages six and seven. More to the point, there is a growing body of evidence that early childhood education programs can have a range of positive effects that reach into adulthood. This evidence comes from evaluations of small, boutique programs like the Perry and Abecedarian preschool interventions (Heckman et al., 2013; Campbell et al., 2012) as well as large, at-scale programs like Head Start (Bailey et al., 2021), pre-K and kindergarten (Chetty et al., 2011; Gray-Lobe et al., 2022), and heavily-subsidized universal child care (Havnes and Mogstad, 2011; Herbst, 2017). Findings such as these have sparked recent discussions about the conditions under which early childhood program effects persist or reemerge, thereby allowing for the possibility that the center-based impacts estimated here could reemerge if such conditions are present (e.g., Bailey et al., 2017).

Nevertheless, a defining feature of the fade-out phenomenon is that initially positive effects become null (e.g., Puma et al., 2010). A noteworthy feature of the results in this paper, however, is that the short-run *negative* effects of center-based care on social skills also fade. That the negative effects of early childhood programs can fade as well is not without precedent in the literature. Indeed, Herbst and Tekin (2016) find that the receipt of child care subsidies in the year before kindergarten has short-run negative effects on children's test scores and behavior at kindergarten entry, with most of these effects fading by the end of first grade. Another noteworthy feature of the results in this paper is a type of "fade-in"—that is, initially positive effects that become negative in the medium-run. Recall that girls at 36-months experience test score gains that turn into about equal-sized reductions in test scores at 60-months. Again, the fading-in of negative effects is not entirely without precedent, with a recent study of Tennessee's pre-K program showing short-run positive effects on academic skills that turned into negative effects several years later. Together, this discussion suggests that it is equally important to understand the conditions under which negative program impacts fade-out or positive impacts fade into negative impacts.

5.3 Generalizability

Recall that most of the data collected for the CCDP evaluation—including information on children's child care arrangements—occurred throughout the early-1990's. Therefore, an important question is whether the effects of center-based enrollment estimated here would apply to contemporary cohorts of young children. To address this question, two considerations seem particularly relevant. First, it is crucial to understand whether, and how, the treatment group's counterfactual child care arrangements have evolved over time. Indeed, as others note, these counterfactuals determine to a great extent the impact of a given early childhood program (Kline and Walters, 2016). The second consideration is whether the center-based sector has evolved in ways—through changes in quality, for example—that might influence child development differently today than it did in the early-1990's. Although it is beyond the scope of this paper to provide a detailed analysis of changes in the child care market, below I summarize the most salient information related to both considerations.

The primary counterfactual care arrangement in my analysis of the CCDP is parent care (with participation rates between 57% and 62% within the control group), followed by relative care (between 14% and 22%). While the share of children in parent care remains steady during the first three years of life, participation in relative care declines, offset by an increase in center-based enrollment. Meanwhile, participation in non-relative arrangements is comparatively low and steady, at 7% to 9%. Although it is not possible to make precise comparisons with the current context, I pool data from the 2016 and 2019 waves of the National Household Education Survey (NHES), which permits the calculation of age-specific participation rates for children in single-parent households.[30] For children ages zero to three, parent care today remains the predominant care mode, followed by relative care. However, the data show some important differences. First, the share of children in parent care is lower than in the CCDP, and participation rates decline with age, from 55% at age one to 38% at age three. Second, substantially more children are in relative care—between 26% and 32%—and participation is steady during the first three years. Finally, the share of children in non-relative arrangements—between 12% and 14%—is nearly double that in the CCDP. Thus, it seems reasonable to conclude that while the mix of counterfactual care modes might still be dominated by parents, relative and non-relative caregivers are more important today than in the CCDP evaluation.

Regarding the second consideration—on the nature and magnitude of changes within the center-based sector—it is clear that the supply of centers increased dramatically over the last few decades. Herbst (2023) finds that the number of private-sector establishments increased from 35,000 in 1990 to 73,000 in 2019. This is consistent with the increase in the share of young children enrolled in center-based arrangements. Fully 21% of three-year-olds in

[30] I examine single parents in the NHES in order to align its sample characteristics as closely as possible with the CCDP evaluation. Indeed, two-thirds of the CCDP sample contains children of single parents.

CCDP's control group were in center care, compared to 27% in the pooled NHES samples.[31] While the supply of centers has grown, the available evidence suggests that some quality-related characteristics have not changed. For example, the share of center-based workers with a bachelor's degree has remained steady since early-1990's, and hourly wages have been largely flat (Herbst, 2018b, 2023). Furthermore, although states' Quality Rating and Improvement Systems (QRIS) are widespread today because of mounting pressure to improve program quality, at least one study finds that minimum quality regulations (e.g., staff education requirements, child-to-ratios, and classroom group sizes) are not substantially different today (Herbst, 2018b). Together, this discussion implies that changes in the center-based sector have not been so severe as to preclude the estimates in this paper from being relevant to the contemporary child care market or reform proposals.

5.4 Implications for Policy

As noted earlier, community-accessible center-based child care is a critical care setting for preschool-age children. According to recent estimates, 1.5 million U.S. children under the age of three regularly attend a center-based provider, and by age two such services become children's predominant care mode (Cui and Natzke, 2021; Datta et al., 2023). Results from this study therefore have important implications for policy reforms that alter families' access to the center-based sector.

Currently, there are several early care and education policies that intersect with center-based programs. First, the Child Care and Development Fund (CCDF), which is a means-tested child care subsidy program, was created in the mid-1990s to move single mothers with young children from welfare to work. Today, an average of 1.4 million children receive CCDF-funded subsidies every month, and most of these children (76%) use the subsidy to pay for center-based care.[32] In fact, the econometric evidence suggests that the CCDF encourages the use of center-based care, and that these services are of higher-quality, on average, than other forms of subsidized care (Herbst, 2023). Conversely, states' QRIS, which are initiatives that evaluate, rate, and improve market-wide child care quality, are shown to discourage the use of center-based care among disadvantaged families, perhaps because QRIS participation enables (or forces) providers to increase prices (Herbst, 2018a).[33] A final, non-child care policy that

[31]Evidence exists elsewhere of rising center care participation. For example, Cascio (2021) shows that preschool and nursery school participation among three-year-olds rose from 9% in 1968 to 39% in 2018. In their study of Boston's pre-K program, Weiland and Yoshikawa (2013) estimate that 57% of three-year-olds in the control group were in some type of center-based setting.

[32]The CCDF has evolved over time to serve a larger share of children in centers. In 2000, for example, 58% of subsidized children used center-based services. These figures are drawn from the Office Child Care's CCDF statistical fact-sheets: https://www.acf.hhs.gov/occ/data/child-care-and-development-fund-statistics.

[33]There is a related line of work on child care regulations, which shows modest effects on the supply of and demand for center-based

may nevertheless have implications for families' use of center-based services is local minimum wage reforms. These reforms are found in a recent paper to increase earnings and prices in the center-based sector, thereby reducing parents' satisfaction with their care provider (Brown and Herbst, 2023).

While existing policies appear to have conflicting effects on families' ability to access center-based services, some recently proposed policy reforms would substantially improve access. For example, the House-passed Build Back Better Act (BBBA) created a child care entitlement program that would have made subsidies available to all children in families below 250% of state median income (SMI). The subsidies would have fully offset child care costs for low-income families while capping expenses at 7% of family income for all remaining eligible families. Importantly, the legislation restricted the set of providers eligible to service subsidized children to center- and home-based caregivers, which would have significantly increased the demand for these services. Indeed, a recent equilibrium analysis of the BBBA's child care reforms shows that such a policy—by substantially reducing family costs—is predicted to increase the use high-cost and presumably higher-quality center care, particularly among the most disadvantaged families (Borowsky et al., 2022).

It is clear from the discussion above that some policies increase access to center-based providers, while others reduce access. It also seems clear that prices are an important driver of access. Given that center-based providers are rated to be of higher-quality, on average, than those in other sectors, and that quality is predictive of children's school readiness, these policies are likely to (indirectly) influence child development by altering parents' preferences for child care (Auger et al., 2014; Bassok et al., 2016). Despite this line of reasoning, results in this study imply that policies inducing a shift into the center-based sector might have conflicting short-run effects on child development and little effect on development at the time children enter school.

6 Conclusion

This paper uses the random assignment of poor families to treatment and control conditions in the CCDP to isolate the causal effect of center-based child care enrollment during the first three years of life. Those in the treatment group were significantly more likely to enroll their child in center-based services, and I use this variation to estimate the impact of center-based enrollment on children's test scores, social skills, and health throughout the preschool-age years. The paper uncovers mixed results: while more time spent in center care improves test scores

establishments (Herbst, 2023).

in the short-run, it reduces social skills, and both effects fade-out entirely for most children within one or two years.

While studies of small-scale interventions (Campbell et al., 2012; Heckman et al., 2013), Head Start (Puma et al., 2010), pre-K (Lipsey et al., 2018), and kindergarten (Chetty et al., 2011) have used random assignment or as-good-as random techniques to study children's short- and long-run outcomes, the literature on community-accessible center-based child care has not been able to exploit such sources of variation. Indeed, among the most relevant work to date are papers by Bernal and Keane (2011) and Herbst (2013), which instrument for center-based participation using variation in the eligibility rules for means-tested programs and seasonal changes in the demand for child care, respectively. Therefore, the current paper contributes new evidence to this literature through the use of a randomized research design.

A number of existing policies change the relative price of purchasing center care, thereby altering the attractiveness of these services to families. In addition, several recently proposed and enacted policies at the federal-level likely have implications for families' ability to access center-based providers. Individual states and local jurisdictions are also experimenting with a range of child care reforms, from enacting system-wide changes (e.g., New Mexico) and increasing teacher education requirements (e.g., District of Columbia) to expanding eligibility and benefits for subsidies (e.g., Minnesota and North Dakota) and subsidizing teachers' child care expenses (e.g., Kentucky). Given the intense and persistent interest in early childhood education, it is crucial that researchers continue to provide high-quality evidence on the full spectrum of programs and services, including those in the center-based sector.

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Table 1: Counterfactual Child Care Choices

	Parent		**Relative**		**Non-Relative**		**Center**	
	Control Mean	Diff.	Control Mean	Diff.	Control Mean	Diff.	Control Mean	Diff.
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
18-months	0.571	-0.190***	0.224	-0.093**	0.087	0.020	0.118	0.263***
		(0.050)		(0.037)		(0.030)		(0.044)
24-months	0.594	-0.157***	0.190	-0.062***	0.076	0.015	0.139	0.205***
		(0.025)		(0.018)		(0.014)		(0.021)
30-months	0.617	-0.146***	0.144	-0.041**	0.075	0.020	0.163	0.167***
		(0.024)		(0.016)		(0.014)		(0.021)
36-months	0.578	-0.144***	0.140	-0.026	0.071	-0.004	0.210	0.175***
		(0.024)		(0.016)		(0.013)		(0.022)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Columns (1a), (2a), (3a), and (4a) report the mean participation rate in each listed child care arrangement for those not receiving an offer to participate in CCDP. Columns (1b), (2b), (3b), and (4b) report coefficients and standard errors (in parentheses) from separate regressions in which the outcome is a binary indicator for participation in each listed child care arrangement. The independent variable is a binary indicator for an offer to participate in CCDP. Each analysis is conducted separately at 18-, 24-, 30-, and 36-month wave of data collection. Robust standard errors are reported. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. The number of observations ranges from 414 to 1,692. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Covariate Balance

	(1) Control Mean	(2) No Controls	(3) Site FE
Child is male	0.493	0.014	0.012
		(0.028)	(0.027)
Child is firstborn	0.329	-0.000	-0.008
		(0.019)	(0.021)
Child is non-white	0.707	-0.016	-0.015
		(0.019)	(0.010)
Mother's age (years)	20.0	-0.162	-0.185
		(0.168)	(0.153)
Mother is married	0.320	-0.008	0.002
		(0.023)	(0.024)
Mother's education (years)	10.97	0.008	-0.086
		(0.129)	(0.117)
Mother is employed	0.127	0.055**	0.044
		(0.024)	(0.025)
Father is employed	0.170	0.053**	0.065**
		(0.022)	(0.023)
Household size	3.99	-0.039	-0.036
		(0.083)	(0.083)
Lives in public housing	0.352	-0.013	-0.027
		(0.026)	(0.019)
Primary language is English	0.866	0.009	-0.003
		(0.018)	(0.015)
Log of household income	6.65	0.179	0.174
		(0.251)	(0.221)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Column (1) reports the means of the listed baseline characteristics for those not receiving an offer to participate in CCDP. Columns (2) and (3) report coefficients and standard errors (in parentheses) from separate regressions in which the outcome is the listed baseline characteristic. The independent variable in each model is a binary indicator for an offer to participate in CCDP. Column (2) includes no controls, and column (3) controls for site fixed effects. The standard errors are adjusted for clustering at the site-level. Mother's age is the age (in years) at the birth of the first child. Household income is expressed as income per person in the household. The regressions are restricted to families with non-missing data on each baseline characteristic, and to families with child care information available at 18-, 24-, 30-, and 36-months. The number of observations ranges from 1,530 to 1,702. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Attrition Analysis

	(1) Percent Missing	(2) Difference
PPVT 36-months	0.059	-0.026
		(0.016)
PPVT 48-months	0.135	0.039*
		(0.020)
PPVT 60-months	0.148	0.032
		(0.019)
ABI 36-months	0.019	0.007
		(0.006)
ABI 48-months	0.094	0.034***
		(0.012)
CBC 60-months	0.082	0.015
		(0.018)
Health Index 36-months	0.017	0.006
		(0.006)
Health Index 48-months	0.089	0.036**
		(0.013)
Health Index 60-months	0.076	0.019
		(0.017)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Column (1) reports the share of observations with missing data on each listed child outcome. Column (2) reports coefficients and standard errors (in parentheses) from separate regressions in which the outcome is a binary indicator for whether the child is missing information on each listed developmental outcome (i.e., PPVT or Peabody Picture Vocabulary Test, ABI or Adaptive Behavior Inventory, CBC or Child Behavior Checklist, and the health index). The independent variable in each model is a binary indicator of an offer to participate in CCDP. All regressions control for site fixed effects, and the standard errors are adjusted for clustering at the site-level. The sample is restricted to families with child care information available at 18-, 24-, 30-, and 36-months. The number of observations for all analyses is 1,702. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Instrumental Variables Estimates for the Effects of Center-Based Child Care Attendance on Child Development at 36-Months

	(1) Control Mean	(2) Obs.	(3) First Stage	(4) RF	(5) 2SLS	(6) LIML	(7) A-R Test
PPVT	83.08	1,602	4.35***	1.6159*	0.3714**	0.2567**	p=0.093
			(0.76)	(0.8599)	(0.1861)	(0.1293)	
ABI Total	42.22	1,669	4.41***	-0.0183**	-0.0041**	-0.0041***	p=0.922
			(0.75)	(0.0085)	(0.0019)	(0.0015)	
ABI Compliant Behavior	14.00	1,670	4.42***	-0.0225*	-0.0051*	-0.0053***	p=0.887
			(0.75)	(0.0129)	(0.0028)	(0.0020)	
ABI Expressive Behavior	19.47	1,669	4.41***	-0.0158*	-0.0036*	-0.0038**	p=0.563
			(0.75)	(0.0087)	(0.0021)	(0.0017)	
ABI Pro-Social Behavior	33.47	1,669	4.41***	-0.0178**	-0.0040**	-0.0041***	p=0.926
			(0.75)	(0.0083)	(0.0019)	(0.0015)	
ABI Disruptive Behavior	5.17	1,669	4.41***	0.0150	0.0034	0.0051	p=0.505
			(0.75)	(0.0255)	(0.0054)	(0.0037)	
Health Index	0.028	1,590	4.37***	-0.0387	-0.0089	0.0093	p=0.160
			(0.75)	(0.1650)	(0.0369)	(0.0324)	

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—The outcome variable in column (3) is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. The outcome variable in columns (4) through (6) is one of the listed developmental measures taken at 36-months. The ABI (Adaptive Behavior Inventory) outcomes are expressed in log form. The independent variable in columns (3) and (4) is a binary indicator of an offer to participate in CCDP. The independent variable in columns (5) and (6) is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Instrumental Variables Estimates for the Effects of Center-Based Child Care Attendance on Child Development at 48- and 60-Months

	(1) Control Mean	(2) Obs.	(3) First Stage	(4) RF	(5) 2SLS	(6) LIML	(7) A-R Test
Panel A: 48-Month Assessment							
PPVT	78.20	1,473	4.76***	0.7200	0.1511	-0.0469	p=0.065
			(0.79)	(0.9059)	(0.1896)	(0.1978)	
ABI Total	42.95	1,542	4.73***	-0.0072	-0.0015	-0.0003	p=0.517
			(0.78)	(0.0101)	(0.0021)	(0.0025)	
ABI Compliant Behavior	14.76	1,542	4.73***	-0.0181	-0.0038	-0.0026	p=0.361
			(0.78)	(0.0166)	(0.0034)	(0.0036)	
ABI Expressive Behavior	19.45	1,542	4.73***	-0.0040	-0.0008	0.0012	p=0.804
			(0.78)	(0.0086)	(0.0018)	(0.0017)	
ABI Pro-Social Behavior	34.21	1,542	4.73***	-0.0088	-0.0019	-0.0002	p=0.609
			(0.78)	(0.0102)	(0.0021)	(0.0023)	
ABI Disruptive Behavior	5.18	1,542	4.73***	-0.0117	-0.0025	-0.0032	p= 0.313
			(0.78)	(0.0282)	(0.0058)	(0.0072)	
Health Index	-0.065	1,413	4.85***	-0.1318	-0.0272	0.0124	p=0.049
			(0.80)	(0.1819)	(0.0376)	(0.0384)	
Panel B: 60-Month Assessment							
PPVT	81.60	1,450	4.84***	-0.4305	-0.0890	-0.0896	p=0.036
			(0.77)	(1.0369)	(0.2058)	(0.1727)	
CBC Total	51.37	1,562	4.84***	-0.0061	-0.0013	-0.0015	p=0.160
			(0.76)	(0.0122)	(0.0024)	(0.0016)	
Health Index	-0.123	1,471	4.82***	0.0487	0.0101	0.0029	p=0.937
			(0.80)	(0.0980)	(0.0194)	(0.0144)	

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996

Notes.—The outcome variable in column (3) is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. The outcome variable in columns (4) through (6) is one of the listed developmental measures taken at 36-months. The ABI (Adaptive Behavior Inventory) and CBC (Child Behavior Checklist) outcomes are expressed in log form. The independent variable in columns (3) and (4) is a binary indicator of an offer to participate in CCDP. The independent variable in columns (5) and (6) is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Robustness Checks on the Instrumental Variables Estimates for the Effects of Center-Based Child Care Attendance on Child Development at 36-, 48-, and 60-Months

	(1) PPVT 36	(2) PPVT 48	(3) PPVT 60	(4) ABI 36	(5) ABI 48	(6) CBC 60	(7) Health 36	(8) Health 48	(9) Health 60
Omit Controls for Parents' Employment	0.4175**	0.1714	-0.0643	-0.0034*	-0.0009	-0.0016	-0.0086	-0.0157	0.0164
	(0.1863)	(0.1930)	(0.2006)	(0.0019)	(0.0021)	(0.0025)	(0.0349)	(0.0373)	(0.0207)
Omit All Baseline Controls	0.4652**	0.2085	-0.0334	-0.0034*	-0.0015	-0.0016	-0.0142	-0.0241	0.0134
	(0.2034)	(0.2073)	(0.2042)	(0.0019)	(0.0022)	(0.0025)	(0.0360)	(0.0372)	(0.0219)
Express *CC* as a Binary Indicator	7.7747**	2.9912	-1.8301	-0.0855**	-0.0303	-0.0256	-0.1895	-0.5529	0.2081
	(3.6573)	(3.7216)	(4.1570)	(0.0392)	(0.0418)	(0.0495)	(0.7887)	(0.7487)	(0.4011)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996

Notes.—The outcome variable in columns (1) through (9) is one of the listed developmental measures taken at 36-, 48-, and 60-months. The ABI (Adaptive Behavior Inventory) and CBC (Child Behavior Checklist) outcomes are expressed in log form. The independent variable for the models in rows one and two is the log (approximate) cumulative number of months the child attended center-based child care as of 36-months. The independent variable for the models in row three is the cumulative number of months (in levels) the child attended center-based child care as of 36-months. The independent variable for the models in row four is a binary indicator equal to one if a given child attended any center-based care as of 36-months. All regressions control for the child's age (in months) at assessment and site-by-birth year fixed effects. The models in row one omit the baseline controls for mother's and father's employment status, while those in row two omit all of the baseline child and parent characteristics. The models in rows three and four include all of the baseline child and parent characteristics. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Instrumental Variables Estimates for the Effects of Center-Based Child Care Attendance on Sub-Groups of Children at 36-, 48-, and 60-Months

	(1) PPVT 36	(2) PPVT 48	(3) PPVT 60	(4) ABI 36	(5) ABI 48	(6) CBC 60	(7) Health 36	(8) Health 48	(9) Health 60
Boys	0.3268	0.4836	0.1280	-0.0076**	-0.0054*	0.0001	-0.0132	-0.0372	0.0181
	(0.2718)	(0.3251)	(0.3063)	(0.0033)	(0.0029)	(0.0027)	(0.0519)	(0.0506)	(0.0324)
Girls	0.3495**	-0.1444	-0.3302*	-0.0019	0.0015	-0.0001	-0.0070	-0.0328	-0.0144
	(0.1766)	(0.1949)	(0.1870)	(0.0033)	(0.0030)	(0.0040)	(0.0394)	(0.0422)	(0.0304)
Non-White	0.4444**	-0.0510	-0.1700	-0.0062***	-0.0020	0.0011	-0.0676	-0.0517	-0.0054
	(0.2151)	(0.1504)	(0.2118)	(0.0019)	(0.0024)	(0.0024)	(0.0475)	(0.0446)	(0.0246)
White	0.1608	0.8653***	0.2667	0.0015	0.0027	-0.0084	0.1974***	0.0581	0.0542
	(0.3506)	(0.3336)	(0.4792)	(0.0073)	(0.0063)	(0.0052)	(0.0572)	(0.0730)	(0.0602)
Low-Education	0.7126**	0.1608	-0.0132	0.0006	0.0011	-0.0012	-0.0050	-0.0169	-0.0041
	(0.2938)	(0.2417)	(0.2976)	(0.0020)	(0.0026)	(0.0031)	(0.0434)	(0.0520)	(0.0343)
High-Education	0.1698	0.2260	-0.0618	-0.0030	-0.0031	-0.0028	-0.0218	-0.0325	0.0096
	(0.1493)	(0.2046)	(0.2064)	(0.0028)	(0.0027)	(0.0034)	(0.0391)	(0.0344)	(0.0387)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—The outcome variable in columns (1) through (9) is one of the listed developmental measures taken at 36-, 48-, and 60-months. The ABI (Adaptive Behavior Inventory) and CBC (Child Behavior Checklist) outcomes are expressed in log form. The independent variable in all models is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. "Low Education" is defined as mothers with no more than 11 years of completed schooling, while "High Education" is defined as mothers with more than 11 years of schooling. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Instrumental Variables Estimates for the Effects of Center-Based Child Care Attendance on Subsequent Early Childhood Education Service Use

	(1) Control Mean	(2) Obs.	(3) 2SLS	(4) LIML
Parental Care at 48-Months	0.484	1,545	-0.0184***	-0.0151***
			(0.0059)	(0.0050)
Parental Care at 60-Months	0.513	1,392	-0.0051	-0.0037
			(0.0057)	(0.0050)
Center-Based Participation at 48-Months	0.137	1,545	0.0144***	0.0077
			(0.0048)	(0.0050)
Center-Based Participation at 60-Months	0.106	1,392	0.0078	0.0038
			(0.0048)	(0.0037)
Head Start at Participation 48-Months	0.161	1,545	0.0149***	0.0153***
			(0.0039)	(0.0037)
Head Start Participation at 60-Months	0.158	1,392	0.0105**	0.0118***
			(0.0045)	(0.0034)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Each cell in this table reports coefficients and standard errors (in parentheses) from separate regressions in which the outcome is one of the listed survey items measured at 48- and 60-months. All outcomes are measured as binary indicators. The independent variable in all models is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix Figures and Tables

Table A1: Estimates for the Effects of CCDP Assignment on Service Receipt and Well-Being

	(1) 36-Months	(2) 48-Months	(3) 60-Months
Panel A: Health Services Receipt			
Mother Visited the Dentist	0.020	0.015	0.003
	(0.023)	(0.023)	(0.027)
Child Visited the Dentist	-0.019	0.015	-0.009
	(0.018)	(0.027)	(0.023)
Mother Had Wellness Check-Up	-0.004	0.066*	0.015
	(0.026)	(0.036)	(0.026)
Child Had Wellness Check-Up	0.006	0.052	0.016
	(0.029)	(0.035)	(0.020)
Mother Received Acute Care	0.023	-0.001	0.013
	(0.022)	(0.019)	(0.023)
Child Received Acute Care	0.030	-0.009	0.033
	(0.022)	(0.035)	(0.042)
Mother Received Physical/Occupational Therapy	-0.005	-0.009	0.007
	(0.012)	(0.012)	(0.009)
Child Received Physical/Occupational Therapy	-0.000	-0.011	0.002
	(0.008)	(0.008)	(0.009)
Panel B: Education Services Receipt			
Mother's Hours of Schooling	2.481	9.807	14.573**
	(8.046)	(7.130)	(6.500)
Mother's Hours in Vocational Classes	1.929	2.992	8.948
	(3.930)	(4.641)	(5.189)
Mother's Hours in Parenting Classes	1.130**	1.303	1.199***
	(0.453)	(1.069)	(0.415)
Panel C: Maternal Health and Health Behaviors			
Mother is in Excellent/Very Good Health	0.008	-0.015	-0.034
	(0.025)	(0.025)	(0.023)
Mother Used Alcohol	0.026	-0.000	0.011
	(0.023)	(0.020)	(0.028)
Mother Used Drugs	0.012	0.009	0.001
	(0.013)	(0.014)	(0.013)
Panel D: Program Participation, Employment, and Earnings			
Family Receives Food Stamps	0.024	0.044**	-0.000
	(0.023)	(0.017)	(0.020)
Family Receives AFDC	0.013	-0.010	0.041
	(0.025)	(0.027)	(0.024)
Mother is Employed	0.030	0.014	-0.015
	(0.023)	(0.031)	(0.024)
Mother's Weekly Hours of Work	0.879	-0.499	-1.151
	(0.929)	(1.177)	(1.076)
Mother's Weekly Earnings	0.151	0.022	-0.115
	(0.119)	(0.170)	(0.134)
Husband/Partner is Employed	0.024	0.017	-0.005
	(0.024)	(0.020)	(0.027)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Each cell in this table reports coefficients and standard errors (in parentheses) from separate regressions in which the outcome is one of the listed survey items measured at 36-, 48-, and 60-months. Mother's weekly earnings is measured in log form. The independent variable in each model is a binary indicator of an offer to participate in CCDP. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Relationship Between Baseline Characteristics and Child Outcome Missingness

	(1) PPVT 36	(2) PPVT 48	(3) PPVT 60	(4) ABI 36	(5) ABI 48	(6) CBC 60	(7) Health 36	(8) Health 48	(9) Health 60
Child is male	0.001	-0.011	-0.022	0.010*	-0.012	-0.022*	0.008	-0.013	-0.019
	(0.013)	(0.020)	(0.016)	(0.005)	(0.013)	(0.012)	(0.006)	(0.014)	(0.013)
Child is firstborn	-0.004	0.003	-0.004	-0.002	0.006	-0.008	-0.005	0.007	-0.004
	(0.012)	(0.011)	(0.013)	(0.004)	(0.010)	(0.010)	(0.005)	(0.010)	(0.009)
Child is non-white	0.007	-0.027	-0.013	0.013**	-0.002	0.019	0.017**	0.003	0.010
	(0.023)	(0.036)	(0.029)	(0.006)	(0.029)	(0.029)	(0.006)	(0.028)	(0.028)
Mother's age (years)	0.001	-0.002	-0.002	-0.002	-0.003*	-0.003*	-0.002*	-0.003*	-0.003
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Mother is married	0.019	0.032	0.003	0.010	0.013	-0.022	0.011	0.009	-0.013
	(0.020)	(0.030)	(0.027)	(0.006)	(0.021)	(0.021)	(0.008)	(0.020)	(0.017)
Mother's education (years)	-0.004	-0.008	-0.010**	-0.002	-0.008*	-0.007**	-0.003	-0.007	-0.006**
	(0.003)	(0.005)	(0.004)	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)
Mother is employed	-0.005	0.016	-0.023	-0.002	0.016	-0.021	-0.000	0.020	-0.023
	(0.018)	(0.028)	(0.026)	(0.007)	(0.028)	(0.013)	(0.008)	(0.029)	(0.014)
Father is employed	0.000	-0.007	-0.027	-0.008	-0.008	-0.046**	-0.005	-0.017	-0.038**
	(0.013)	(0.016)	(0.027)	(0.006)	(0.022)	(0.017)	(0.006)	(0.022)	(0.015)
Household size	-0.001	-0.014**	-0.004	-0.003	-0.012**	-0.001	-0.004	-0.011**	-0.001
	(0.003)	(0.006)	(0.005)	(0.003)	(0.005)	(0.003)	(0.003)	(0.005)	(0.003)
Lives in public housing	-0.007	-0.039*	-0.020	-0.010	-0.020	-0.011	-0.009	-0.017	0.000
	(0.012)	(0.022)	(0.022)	(0.010)	(0.017)	(0.021)	(0.008)	(0.016)	(0.023)
Primary language is English	-0.029	-0.017	-0.107**	-0.003	-0.020	-0.054	-0.006	-0.015	-0.052
	(0.021)	(0.037)	(0.051)	(0.009)	(0.027)	(0.035)	(0.011)	(0.025)	(0.037)
Log of household income	0.000	-0.005	-0.007	-0.003*	-0.006	-0.002	-0.001	-0.004	-0.001
	(0.003)	(0.004)	(0.005)	(0.002)	(0.004)	(0.004)	(0.001)	(0.003)	(0.004)

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Each cell in this table reports coefficients and standard errors (in parentheses) from a separate regression in which the outcome is a binary indicator for whether the child is missing information on each listed developmental outcome. The independent variable in each model is the listed baseline characteristic. Mother's age is the age (in years) at the birth of the first child. Household income is expressed as income per person in the household. All regressions control for site fixed effects, and the standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with non-missing data on each baseline characteristic, and to families with child care information available at 18-, 24-, 30-, and 36-months. The number of observations ranges from 1,530 to 1,702. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Relationship Between Baseline Characteristics and the Availability of Child Care Information

	Coeff. on Baseline Characteristic (1)	Observations (2)
Child is male	-0.008	4,033
	(0.014)	
Child is firstborn	-0.042**	3,750
	(0.018)	
Child is non-white	-0.019	4,117
	(0.032)	
Mother's age (years)	-0.001	3,724
	(0.002)	
Mother is married	0.020	3,784
	(0.023)	
Mother's education (years)	0.006*	3,549
	(0.004)	
Mother is employed	0.019	4,173
	(0.020)	
Father is employed	0.054**	4,173
	(0.020)	
Household size	0.004	4,064
	(0.005)	
Lives in public housing	-0.017	3,847
	(0.022)	
Primary language is English	0.057**	3,847
	(0.019)	
Log of household income	0.003	4,173
	(0.005)	

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—This table reports coefficients and standard errors (in parentheses) from separate regressions in which the outcome is a binary indicator for whether a family has full child care information available. The independent variable in each model is the listed baseline characteristic. A family is coded as having full child care information if they responded to the CCDP's child care survey at the 18-, 24-, 30-, and 36-month waves of data collection. Mother's age is the age (in years) at the birth of the first child. Household income is expressed as income per person in the household. All regressions control for site fixed effects, and the standard errors are adjusted for clustering at the site-level. Each analysis is restricted to families with non-missing data on each baseline characteristic. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: OLS Estimates for the Effects of Center-Based Child Care Attendance on Child Development

	(1) 36-Months	(2) 48-Months	(3) 60-Months
PPVT	0.2282***	0.2147***	0.1799***
	(0.0367)	(0.0447)	(0.0456)
	1,602	1,473	1,450
ABI Total	0.0012**	0.0006	–
	(0.0005)	(0.0008)	
	1,669	1,542	
ABI Compliant Behavior	0.0009	0.0006	–
	(0.0009)	(0.0010)	
	1,670	1,542	
ABI Expressive Behavior	0.0014**	0.0015*	–
	(0.0005)	(0.0007)	
	1,669	1,542	
ABI Pro-Social Behavior	0.0013**	0.0011	–
	(0.0006)	(0.0008)	
	1,669	1,542	
ABI Disruptive Behavior	-0.0007	0.0023	–
	(0.0012)	(0.0019)	
	1,669	1,542	
CBC Total	–	–	0.0014**
			(0.0006)
			1,562
Health Index	-0.0027	0.0103	0.0010
	(0.0087)	(0.0060)	(0.0072)
	1,590	1,413	1,471

Source.—Comprehensive Child Development Program (CCDP), Evaluation Data, 1990-1996
Notes.—Each cell in this table reports coefficients, standard errors (in parentheses), and the number of observations from separate regressions in which the outcome is one of the listed developmental measures taken at 36-, 48-, and 60-months. The ABI (Adaptive Behavior Inventory) and CBC (Child Behavior Checklist) outcomes are expressed in log form. ABS Total is measured at 36- and 48-months, while CBC Total is measured at 60-months. The independent variable in each model is the (approximate) cumulative number of months the child attended center-based child care as of 36-months. All regressions control for the child's age (in months) at assessment, baseline child and parent characteristics, and site-by-birth year fixed effects. The standard errors are adjusted for clustering at the site-level. The regressions are restricted to families with child care information available at 18-, 24-, 30-, and 36-months.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.