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Chicago Child-Parent Center (CPC)

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by Douglas J. Besharov, Peter Germans, Caeli A. Higney, and Douglas M. Call

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Chicago Child-Parent Center (CPC)

The Chicago Child-Parent Center (CPC) program, started in 1967 and currently operating, provides a school-based preschool and early school-age intervention for low-income children ages three to nine in selected Chicago public schools. The main objectives of the program are to promote academic success by providing a “school-stable” learning environment during the preschool and primary-grade years and to actively involve parents in their children’s education.¹ At the time of the evaluation, it offered comprehensive education and family support services.

Arthur Reynolds, currently a professor at the University of Illinois at Chicago, and his colleagues (the “Chicago team”) conducted the major evaluation of the Chicago Child-Parent Center program, using a nonexperimental design to compare children in 1986 who had graduated from a CPC kindergarten to children who graduated from a non-CPC kindergarten. They report that the CPC program improved a wide range of school performance outcomes and reduced criminal activity. According to the Chicago team, the CPC program resulted in improvements in reading and math achievement test scores during adolescence, fewer special education placements, less grade retention, increased high school completion, and fewer juvenile arrests.² These are among the largest and broadest positive impacts of all early childhood intervention programs. But a careful review of the study suggests that a more cautious view is warranted. Thomas Cook and Vivian Wong, professors at Northwestern University, write that the CPC evaluation “depends on an opaque matching procedure and on data analyses (Heckman-type selection models and propensity scores) that have routinely failed to recreate similar effect sizes to an experiment on the same topic. This implies the possibility of a selection confound not fully

¹Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000).

²Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 2001): 2339–46.

controlled.”³ In addition, high levels of missing data, the absence of a true randomized experiment, and various self-selection biases raise considerable uncertainty about the findings.

Program Design

Program group. At the time of the evaluation, the CPC program targeted children in economically disadvantaged families living in high-poverty neighborhoods in which schools received Title I funds.⁴ To participate, children had to have demonstrable educational needs and their parents had to agree to participate for at least one-half day per week. In addition, they could not be enrolled in other preschool programs, such as Head Start. Children could enroll when they were three years old and continue through the second or third grade, depending on the CPC.

Participating children were from low-income black (93 percent) and Hispanic (7 percent) families.

Services. At the time of the evaluation, the CPC Program was a center-based early education program with comprehensive family support services.⁵ It was integrated with the school system, and thus classrooms were typically located at or near the children’s primary elementary school. It was first implemented with four goals in mind: (1) parent involvement; (2) academic achievement, particularly geared towards development of speaking and listening skills; (3) individual attention; and (4) health and nutritional services. To facilitate academic achievement, children ages three and four were provided with a structured half-day preschool program during the nine-month school year, followed by a full-day kindergarten program at age five. During this time, the curriculum followed the Chicago Early Assessment and Remediation Laboratory (EARLY) model. Children’s individual strengths and weaknesses were assessed, and then their needs were met through a flexible activity-based approach. This approach includes instructional guidance within several major developmental areas. To give each child individual attention, reduced class sizes, additional teacher aides, and instructional supplies and activities were provided. These services were provided until the children reached the second or third grade, thus easing their transition to elementary school. In addition, parents were required to participate at least one-half day per week in activities such as reading to small groups, going on field trips, and supervising play activities. Each center had a Parent-Resource Teacher who was responsible

³Thomas D. Cook and Vivian C. Wong, “The Warrant for Universal Pre-K: Can Several Thin Reeds make a Strong Policy Boat?” *Social Policy Report* 21, no. 3 (2007): 14, http://www.srcd.org/documents/publications/spr/21-3_early_childhood_education.pdf (accessed April 8, 2009).

⁴Title I of the federal Elementary and Secondary Education Act which provides federal funding to K-12 schools with a high percentage of low-income children.

⁵The information for this section was obtained from “Program Overview and History,” *The Chicago Longitudinal Study: Understanding the Impact of Early Intervention* (Madison, WI: University of Wisconsin-Madison, 1998), <http://www.waisman.wisc.edu/cls/History.htm> (accessed June 18, 2001).

for implementing the parent portion of the program. Adult-to-child ratios averaged 1:8 for the preschool and 1:12 for the kindergarten/school-age components. Finally, the CPC program provided a comprehensive array of health and social services, including health screening, speech therapy, nursing services, and free breakfasts and lunches.

The Evaluation. The evaluation used a nonexperimental design based on a 1986 cohort of CPC children and a comparison group of children who graduated from one of Chicago's all-day kindergarten programs. The CPC children were selected from among those who began participating in the fall of 1983 and who graduated kindergarten in 1986. The original comparison group included children who graduated from a public, all-day kindergarten program in non-CPC schools in 1986. (The construction of other comparison groups is described later.) The principal investigator was Reynolds.

The data for the study came from the Chicago Longitudinal Study (CLS), which contains information on a cohort of 1,539 low-income, minority children born in 1980 who attended early childhood programs in 1985–1986. Twenty sites included CPC programs and five did not. Data on children's educational and family experiences were collected annually from school records and participant surveys. The data for many of the background characteristics were not obtained at the time the children entered preschool or were preschool age, but rather were collected from parent and child surveys in later years.⁶

The evaluation sought to estimate the impact of: (1) any CPC participation, (2) preschool CPC participation, (3) school-age CPC participation, and (4) extended CPC participation. The following paragraphs describe the four main interventions and the comparison groups involved in the analysis. The number of children in a CPC group fluctuated because different comparisons were based on varying participation patterns.

Any CPC vs. no CPC. The CPC group included 1,150 children who enrolled in a CPC anytime during their preschool or early school-age years, beginning in the fall of 1983. The original comparison group consisted of 389 children who graduated in 1986 from all-day kindergarten programs in seven schools participating in the Chicago Effective Schools Project (CESP). In two of the seven CESP schools, the CPC program was available, but the children in the comparison group chose not to participate. For the other five CESP schools, the CPC program was not available. If it had been, the children would have been eligible to participate.

⁶Data on child abuse, race, gender, and school poverty were collected at the time of program entry. Family background information (at the time of program entry)—including years of parent education, SES, single parent status, and employment—were collected beginning when the child turned eight years old, at age ten, on average. Reynolds indicated, "One would expect stability in parent education/SES between age 4 and 10, especially given this relatively homogeneous disadvantaged sample." Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, November 30, 2001.

CPC preschool vs. no CPC preschool. To measure the impact of CPC preschool, the Chicago team compared 989 children who participated in a CPC during preschool and kindergarten with 550 children who participated in a different full-day kindergarten program. This latter group included 374 children who attended full-day kindergarten in non-CPC schools, as well as 176 children who attended full-day kindergarten in six CPC schools, but who were not enrolled in preschool there. (In addition, this comparison group included 161 children who participated in the CPC program after preschool.)

CPC school-age vs. no CPC school-age services. The impact of CPC school-age services was based on a comparison of 850 children who attended a CPC at least one year between first and third grade with 689 children with no school-age participation. (This comparison included 290 children who participated in the CPC program before entering the first grade, but not afterwards.)

Extended vs. less extended CPC. To measure the impact of extended CPC participation (four to six years), the Chicago team compared 553 children with extended participation to 602 children with “less” extended participation (one to three years). (This comparison essentially involved dividing the original CPC intervention group based on length of participation.)

Major Findings

The CPC program has been widely cited as having improved a wide range of school performance outcomes and reduced criminal activity.⁷ According to the Chicago team, the CPC program resulted in improvements in reading and math achievement test scores during adolescence, fewer special education placements, less grade retention, increased high school

⁷The findings reported here are from multiple sources: Arthur J. Reynolds, “Effects of a Preschool Plus Follow-On Intervention for Children at Risk,” *Developmental Psychology* 30, no. 6 (November 1994):787–804; Arthur J. Reynolds, “One Year of Preschool Intervention or Two: Does It Matter?” *Early Childhood Research Quarterly* 10 (1995): 1–31; Arthur J. Reynolds, Majida Mehana, and Judy A. Temple, “Does Preschool Intervention Affect Children’s Perceived Competence?” *Journal of Applied Developmental Psychology* 16, no. 2 (April-June 1995): 211–230; Arthur J. Reynolds, Nancy A. Mavrogenes, Nikolaus Bezruczko, and Mavis Hagemann, “Cognitive and Family-Support Mediators of Preschool Effectiveness: A Confirmatory Analysis,” *Child Development* 67, no. 3 (June 1996): 1119–1140; Arthur J. Reynolds, *The Chicago Child-Parent Centers: A Longitudinal Study of Extended Early Childhood Intervention*, Discussion Paper no. 1126-97 (Madison, WI: Institute for Research on Poverty, March 1997); Arthur J. Reynolds and Judy A. Temple, “Extended Early Childhood Intervention and School Achievement: Age Thirteen Findings From the Chicago Longitudinal Study,” *Child Development* 69, no. 1 (February 1998): 231–246; Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln: University of Nebraska Press, 2000), 93; Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2339–2446; and Suh-Ruu Ou and Arthur J. Reynolds, “Early Childhood Intervention and Educational Attainment: Age 22 Findings from the Chicago Longitudinal Study,” *Journal of Education for Students Placed at Risk* 11, no. 2 (April 2006):175–198.

completion, and fewer juvenile arrests.

Cognitive. *Any CPC vs. no CPC comparison.* The Chicago team found persistent statistically significant differences in reading and math scores (using the Iowa Tests of Basic Skills) between the program and comparison group from third grade to ninth grade. In a 1994 study, the Chicago team found that, in third grade, children who had been enrolled in a CPC program had small, statistically significantly higher reading scores (94.6 vs. 90.6) and math scores (99.9 vs. 96.5). In a 1997 study, the Chicago team included controls for missing data and a larger sample size found larger statistically significant differences in reading (98.6 vs. 92.9, an effect size of 0.34 SD) and math (101.8 vs. 97.6, an effect size of 0.32 SD) for children in third grade. These findings persisted in fifth grade (reading: 112.8 vs. 109.8, an effect size of 0.17 SD; math: 118.5 vs. 114.7, an effect size of 0.24 SD), and also in eighth grade (reading: 146.1 vs. 142.3, an effect size of 0.17 SD; math: 148.4 vs. 144.9, an effect size of 0.19 SD). In a 2000 study, the Chicago team only reported the differences in the test scores between the program and comparison groups in ninth grade. The program group continued to have small, statistically significantly higher scores in reading (a difference of 3.5 points, an effect size of 0.33 SD) and math (a difference of 3.3 points, an effect size of 0.31 SD). (See Tables 1 and 2)

CPC preschool group vs. no CPC preschool comparison. The Chicago team, in four separate studies and using varying statistical models, also found that the program group had persistently higher reading and math scores between Kindergarten and ninth grade. Over this nine year period, statistically significant differences were found in all years except for grade three and grade seven (the latter because there was no data reported). Differences in the adjusted means of the test scores ranged within a narrow band from 3.7 to 6.3 (with effect sizes ranging from 0.24 SD to 0.51 SD) for reading scores and from 3.2 to 6.7 (with effect sizes ranging from 0.25 SD to 0.35 SD) for math scores. There was no discernable pattern in the changing test scores to indicate fade-out. (See Tables 1 and 2)

Extended vs. less extended comparison. The Chicago team found statistically significant differences that narrowed over time. In third grade, children who had been enrolled in the extended CPC through third grade had statistically significantly higher reading (102.2 vs. 91.4, a difference of 10.8 points) and math scores (106.1 vs. 97.8, a difference of 8.3 points). (Effect sizes were not reported.) The differences were smaller in fourth grade (reading: 107.7 vs. 101.1, a difference of 6.6 points; math: 111.5 vs. 107.2, a difference of 4.3 points) and fifth grade (reading: 116.8 vs. 109.7, a difference of 7.1 points; math: 122.4 vs. 117.2, a difference of 5.2 points), and this smaller difference persisted in eighth grade (reading: a difference of 6.5 points; math: a difference of 6.2 points) and ninth grade (reading: a difference of 5.9 points; math: a difference of 4.4 points). (See Tables 1 and 2).

Table 1. CPC: Effects on Reading Achievement

Grade and study	Adjusted Means and Differences in Reading Scores								
	Any CPC vs. no CPS			CPC preschool vs. no CPC preschool			Extended vs. Less extended		
	Program	Comp.	Diff.	Program	Comp.	Diff.	Program	Comp.	Diff.
K						4.5			
R95									
Grade 1						5.3			
R95									
Grade 2						5.6			
R95									
Grade 3									
R94	94.6	90.6	4.0				102.2 ^a	91.4 ^b	10.8
R95						—			
R97	98.6	92.9	5.7						
RT									7.8
Grade 4									
R94							107.7 ^a	101.1 ^b	6.6
R95						3.7			
Grade 5									
R94							116.8 ^a	109.7 ^b	7.1
R95						5.1			
R97	112.8	109.8	3.0						
Grade 6									
RMT						6.1			
RMBH						6.3			
R97						6.1			
Grade 7									
RT									8.6
Grade 8									
R97	146.1	142.3	—	147.4	142.0	5.4	154.9	147.7	7.2
R00			4.1			5.5			6.5

Grade 9			
R00	3.5	4.2	5.9

Source:

R94: Arthur J. Reynolds, "Effects of a Preschool Plus Follow-On Intervention for Children at Risk," *Developmental Psychology* 30, no. 6 (November 1994): 787–804.

R95: Arthur J. Reynolds, "One Year of Preschool Intervention or Two: Does It Matter?" *Early Childhood Research Quarterly* 10, no. 1 (March 1995): 1–31.

RMT: Arthur J. Reynolds, Majida Mehana, and Judy A. Temple, "Does Preschool Intervention Affect Children's Perceived Competence?" *Journal of Applied Developmental Psychology* 16, no. 2 (April-June 1995): 211–230.

RMBH: Arthur J. Reynolds, Nancy A. Mavrogenes, Nikolaus Bezruczko, and Mavis Hagemann, "Cognitive and Family-Support Mediators of Preschool Effectiveness: A Confirmatory Analysis," *Child Development* 67, no. 3 (June 1996): 1119-1140.

R97: Arthur J. Reynolds, *The Chicago Child-Parent Centers: A Longitudinal Study of Extended Early Childhood Intervention*, Discussion Paper no. 1126–97 (Madison, WI: Institute for Research on Poverty, March 1997).

RT: Arthur J. Reynolds and Judy A. Temple, "Extended Early Childhood Intervention and School Achievement: Age Thirteen Findings From the Chicago Longitudinal Study," *Child Development* 69, no. 1 (February 1998): 231–246.

R00: Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln: University of Nebraska Press, 2000), 93.

Notes: Only significant differences are reported. "—" indicates that the difference is not statistically significant at the 5 percent level. Scores from Iowa Test of Basic Skills or Tests of Achievement and Proficiency.

^aThe program group consists of children who were enrolled in CPCs through grade three

^bThe comparison group consists of children who were enrolled in CPCs in preschool and Kindergarten

Table 2. CPC: Effects on Mathematics Achievement

Grade and study	Adjusted Means and Difference in Mathematic Scores								
	Any CPC vs. no CPS			CPC preschool vs. no CPC preschool			Extended vs. Less extended		
	Program	Comp.	Diff.	Program	Comp.	Diff.	Program	Comp.	Diff.
K									
R95						6.7			
Grade 1									
R95						3.2			
Grade 2									
R95						4.7			
Grade 3									
R94	99.9	96.5	3.4				106.1 ^a	97.8 ^b	8.3
R95						—			
R97	101.8	97.6	4.2						
RT									4.5
Grade 4									
R94							111.5 ^a	107.2 ^b	4.3
R95						3.3			
Grade 5									
R94							122.4 ^a	117.2 ^b	5.2
R95						4.5			
R97	118.5	114.7	3.8						
Grade 6									
RMT						5.1			
RMBH						5.7			
R97						4.6			
Grade 7									—
RT									
Grade 8									
R97	148.4	144.9	3.5	149.4	145.1	4.3	154.3	142.7??	5.2
R00			3.6			4.4			6.2

Grade 9 R00	3.3	3.3	4.4
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Source:

R94: Arthur J. Reynolds, "Effects of a Preschool Plus Follow-On Intervention for Children at Risk," *Developmental Psychology* 30, no. 6 (November 1994): 787–804.

R95: Arthur J. Reynolds, "One Year of Preschool Intervention or Two: Does It Matter?" *Early Childhood Research Quarterly* 10, no. 1 (March 1995): 1–31.

RMT: Arthur J. Reynolds, Majida Mehana, and Judy A. Temple, "Does Preschool Intervention Affect Children's Perceived Competence?" *Journal of Applied Developmental Psychology* 16, no. 2 (April-June 1995): 211–230.

RMBH: Arthur J. Reynolds, Nancy A. Mavrogenes, Nikolaus Bezruczko, and Mavis Hagemann, "Cognitive and Family-Support Mediators of Preschool Effectiveness: A Confirmatory Analysis," *Child Development* 67, no. 3 (June 1996): 1119–1140.

R97: Arthur J. Reynolds, *The Chicago Child-Parent Centers: A Longitudinal Study of Extended Early Childhood Intervention*, Discussion Paper no. 1126-97 (Madison, WI: Institute for Research on Poverty, March 1997).

RT: Arthur J. Reynolds and Judy A. Temple, "Extended Early Childhood Intervention and School Achievement: Age Thirteen Findings From the Chicago Longitudinal Study," *Child Development* 69, no. 1 (February 1998): 231–246.

R00: Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln: University of Nebraska Press, 2000), 93.

Notes: Only significant differences are reported. "—" indicates that the difference is not statistically significant at the 5 percent level. Scores from Iowa Test of Basic Skills or Tests of Achievement and Proficiency.

^aThe program group consists of children who were enrolled in CPCs through grade three

^bThe comparison group consists of children who were enrolled in CPCs in preschool and Kindergarten

School readiness/performance.

Any CPC vs. no CPC comparison. The Chicago team found persistent effects on grade retention for this group. In grade three, children in the program group were significantly less likely to have been retained a grade (19.2 percent vs 26.2 percent). By grade eight, the difference had grown larger (25.3 percent vs. 36.5 percent) and this difference persisted in grade nine (24.0 percent vs. 35.0 percent). There were, however, no statistically significant differences in the percent of children who were assigned to special education classes and services.

CPC preschool group vs. no CPC preschool comparison. The Chicago team found persistent differences in the percent of the program group that had been retained a grade compared to the comparison group. In grade 1, the program group was 6.6 percentage points less likely to have been retained a grade. (Only percentage point differences were reported.) In grades four, five, and six, the program group was 10.7, 10.6, and 9.8 percentage points less likely,

respectively. In their 2000 study, the Chicago team reported that the difference had narrowed, with the program group being only 7.9 percentage points less likely to have been retained a grade.

In a 2001 *Journal of the American Medical Association (JAMA)* paper, the Chicago team updated these findings and arrived at somewhat different results (see table 3). Despite the fact that the age of follow-up was the same, the estimated impact of CPC preschool participation was twice as large in the more recent study, showing a 15.4 percentage point reduction in grade retention (compared to just an 8 percentage point reduction in the earlier study). This difference may stem from the fact that there was less attrition in the more recent study or from the use of a slightly different statistical model, but it also shows how sensitive the findings are to the available data and modeling choices. (Reynolds notes that the patterns were consistent and that differences were “mostly within the standard errors of the estimates.”)⁸

The findings for special education placements reveal a similar pattern. There were no statistically significant differences between the program and control groups in grades one and two. In grade three, the program group was 4.5 percentage points less likely to have received special education services. By grade six, the difference had grown to 10 percentage points. As with the grade retention findings, the 2000 study findings showed a narrowing in the difference (5.3 percentage points) but the 2001 *JAMA* study showed a persistent effect (10.2 percentage points).

Preschool participation in CPC was found to increase high school completion by age twenty and twenty-two (49.7 percent vs. 38.5 percent, and 66.9 percent vs. 55.3 percent, respectively) with differences greater for boys (at twenty, 42.6 percent vs. 29.0 percent; at twenty-two, 61.1 percent vs. 41.5 percent) than girls (at twenty, 56.5 percent vs. 48.0 percent; at age twenty-two, the difference was not statistically significant.)⁹ (See table 5).

Extended vs. less extended comparison. The program group was also less likely to have been retained a grade over the nine year period, although the differences narrowed somewhat over time. In grade three, the program group was 19.1 percentage points less likely to have been retained a grade (6.6 percent vs. 25.7 percent) compared to the control group. Similar findings persisted through grade eight. For grade nine, the Chicago team found that the program group

⁸Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, November 30, 2001.

⁹Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2443; Suh-Ruu Ou and Arthur J. Reynolds, “Early Childhood Intervention and Educational Attainment: Age 22 Findings from the Chicago Longitudinal Study,” *Journal of Education for Students Placed at Risk* 11, no. 2 (April 2006): 175–198.

was 14.8 percentage points less likely to have been retained a grade (20.0 percent vs. 34.8 percent), but the 2001 *JAMA* study found that the difference was only 10.4 percentage points (21.9 percent vs. 32.3 percent).

Differences in special education placement were more varied over time. In grade three, the Chicago team found that the program group was 6.7 percentage points more likely to have received special education services (10.9 percent vs. 4.2 percent) compared to the control group. There were no statistically significant differences in special education placement in grades four, five, and eight. This finding reversed in grade seven (10.0 percent vs. 15.7 percent) and persisted through age eighteen (13.5 percent vs. 20.7 percent).

At age fifteen, children in the program group were 31 percent less likely to have been retained a grade (24 percent vs. 35 percent), and about 41 percent less likely to have been assigned to a special education classroom (23 percent vs. 38 percent). At age eighteen, members the program group were 32 percent less likely to have been arrested two or more times (17 percent vs. 25 percent). At age twenty-two, members of the program group were 20 percent more likely to have graduated from high school (66 percent vs 55 percent). These are among the largest and broadest impacts of all early childhood intervention programs, with effect sizes ranging from 0.22 SD to 0.59 SD.¹⁰

Although extended participation was associated with higher achievement scores, less grade retention, and less placement in special education, there was no statistically significant impact on high school completion. (Similarly, participation in the school-age component had no significant effect on high school completion.)

¹⁰Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, November 30, 2001.

Table 3. CPC: Effects on Grade Retention

Grade and study	Adjusted Means and Difference in Grade Retention Rate								
	Any CPC vs. no CPS			CPC preschool vs. no CPC preschool			Extended vs. Less extended		
	Program	Comp.	Diff. (% points)	Program	Comp.	Diff. (% points)	Program	Comp.	Diff. (% points)
Grade 1 R95						-6.6			
Grade 2 R95						-8.5			
Grade 3 R94	19.2%	26.2%	-7.0				6.6% ^a	25.7% ^b	-19.1
R95						-6.6			
Grade 4 R94							11.8% ^a	25.0% ^b	-13.2
R95						-10.7			
Grade 5 R94							15.3% ^a	31.8% ^b	-16.5
R95						-10.6			
Grade 6 R95						-9.8			
RMBH						-10.6			
R97						4.6			
Grade 7 RT							15.3%	30.1%	-14.8
Grade 8 R97	25.3%	36.5%	-11.2	24.0%	31.8%	-7.8	6.9%	32.1%	-18.6
R00			-10.3			-6.9			-16.2
Grade 9 R00	24%	35%	-12.2	27.0%	34.9%	-7.9	20.0%	34.8%	-14.8
RTRM				23.0%	38.4%	-15.4	21.9%	32.3%	-10.4

Sources:

R94: Arthur J. Reynolds, "Effects of a Preschool Plus Follow-On Intervention for Children at Risk," *Developmental Psychology* 30, no. 6 (November 1994): 787–804.

R95: Arthur J. Reynolds, "One Year of Preschool Intervention or Two: Does It Matter?" *Early Childhood Research Quarterly* 10, no. 1 (March 1995): 1–31.

RMT: Arthur J. Reynolds, Majida Mehana, and Judy A. Temple, "Does Preschool Intervention Affect Children's Perceived Competence?" *Journal of Applied Developmental Psychology* 16, no. 2 (April-June 1995): 211–230.

RMBH: Arthur J. Reynolds, Nancy A. Mavrogenes, Nikolaus Bezruczko, and Mavis Hagemann, "Cognitive and Family-Support Mediators of Preschool Effectiveness: A Confirmatory Analysis," *Child Development* 67, no. 3 (June 1996): 1119-1140.

R97: Arthur J. Reynolds, *The Chicago Child-Parent Centers: A Longitudinal Study of Extended Early Childhood Intervention*, Discussion Paper no. 1126-97 (Madison, WI: Institute for Research on Poverty, March 1997).

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R00: Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln: University of Nebraska Press, 2000), 93.

RTRM: Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Long-term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-year Follow-up of Low-Income Children," *Journal of the American Medical Association* 285, no. 18 (May 2001): 2339–2346.

Notes: Only significant differences are reported. "—" indicates that the difference is not statistically significant at the 5 percent level. Scores from Iowa Test of Basic Skills or Tests of Achievement and Proficiency.

^aThe program group consists of children who were enrolled in CPCs through grade three

^bThe comparison group consists of children who were enrolled in CPCs in preschool and Kindergarten

Table 4. CPC: Effects on Special Education

Grade and study	Adjusted Means and Differences in Special Education Rates								
	Any CPC vs. no CPS			CPC preschool vs. no CPC preschool			Extended vs. Less extended		
	Program	Comp.	Diff. (% points)	Program	Comp.	Diff. (% points)	Program	Comp.	Diff. (% points)
Grade 1 R95						—			
Grade 2 R95						—			
Grade 3 R94	7.8%	8.9%	—				10.9% ^a	4.2% ^b	6.7
R95						-4.5			
Grade 4 R94							10.5% ^a	8.4% ^b	—
R95						-9.2			
Grade 5 R94							12.5% ^a	10.6% ^b	—
R95						-8.8			
Grade 6 RMT						-10.0			
Grade 7 RT							10.0%	15.7%	-5.7
Grade 8 R97	0.6 yrs	0.9 yrs	—	0.5 yrs	0.9 yrs	-0.4 yrs	0.4 yrs	0.6 yrs	—
R00			-0.3 yrs			-0.4 yrs			-0.5 yrs
Grade 9 R00			—	16%	21.3%	-5.3	12.0%	21.3%	9.3
Age 18 RTRM				14.4%	24.6%	-10.2	13.5%	20.7%	-7.2

Sources:

R94: Arthur J. Reynolds, "Effects of a Preschool Plus Follow-On Intervention for Children at Risk," *Developmental Psychology* 30, no. 6 (November 1994): 787–804.

R95: Arthur J. Reynolds, "One Year of Preschool Intervention or Two: Does It Matter?" *Early Childhood Research Quarterly* 10, no. 1 (March 1995): 1–31.

RMT: Arthur J. Reynolds, Majida Mehana, and Judy A. Temple, "Does Preschool Intervention Affect Children's Perceived Competence?" *Journal of Applied Developmental Psychology* 16 no. 2 (April-June 1995): 211–230.

RMBH: Arthur J. Reynolds, Nancy A. Mavrogenes, Nikolaus Bezruczko, and Mavis Hagemann, "Cognitive and Family-Support Mediators of Preschool Effectiveness: A Confirmatory Analysis," *Child Development* 67, no. 3 (June 1996): 1119-1140.

R97: Arthur J. Reynolds, *The Chicago Child-Parent Centers: A Longitudinal Study of Extended Early Childhood Intervention*, Discussion Paper no. 1126-97 (Madison, WI: Institute for Research on Poverty, March 1997).

RT: Arthur J. Reynolds and Judy A. Temple, "Extended Early Childhood Intervention and School Achievement: Age Thirteen Findings From the Chicago Longitudinal Study," *Child Development* 69, no. 1 (February 1998): 231–246.

R00: Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln: University of Nebraska Press, 2000), 93.

RTRM: Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Long-term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-year Follow-up of Low-Income Children," *Journal of the American Medical Association* 285, no. 18 (May 2001): 2339–2346.

Table 5. CPC: Effects on High School Completion

Type of comparison	Percent completing		Difference (percentage points)
	CPC group	Comparison group	
Any CPC vs. no CPC (age 20)	NA	NA	NA
CPC preschool vs. no CPC preschool (age 20)	49.7%	38.5%	11.2
CPC preschool vs. no CPC preschool (age 22)	66.9%	55.3%	11.6
Extended vs. less extended (age 20)	48.7%	44.0%	—

Source: Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2344; and Suh-Ruu Ou and Arthur J. Reynolds, “Early Childhood Intervention and Educational Attainment: Age 22 Findings from the Chicago Longitudinal Study,” *Journal of Education for Students Placed at Risk* 11, 2:175–198.

Notes: Only significant differences are reported. “—” indicates that the difference is not statistically significant at the 5 percent level. “NA” indicates that the data were not available. Estimates are adjusted for earlier/later (preschool or school-age) program participation, sex of child, risk index, program sites, and race/ethnicity.

Socioemotional development. Relevant tests apparently not administered or results not reported.

Health. Apparently data either not collected or reported.

Behavior. Apparently data either not collected or reported.

Crime/delinquency. A report from Fight Crime: Invest in Kids—an anti-crime organization led by police chiefs and other law enforcement officers—concluded that the CPC program, which has enrolled about 100,000 children since its inception, will result in a reduction of 13,000 violent crimes by the time the children turn eighteen.¹¹ The basis of this claim is the the Chicago Team’s 2001 study.

The Chicago team examined delinquency infractions as reported by school personnel for children age thirteen to fifteen (see table 6). They found a statistically significant reduction in school-reported delinquency infractions only for those who had been in the school-age program. But he found that having participated in the preschool component reduced the proportion of those ever arrested by age eighteen by 8.2 percentage points (16.9 percent vs. 25.1 percent). (See table

¹¹David S. Broder, “Dividends of Early Learning,” *The Washington Post*, July 25, 2001, A21. See also Sanford Newman, T. Berry Brazelton, Edward Zigler, Lawrence W. Sherman, William Bratton, Jerry Sanders, and William Christeson, *America’s Child Care Crisis: A Crime Prevention Tragedy* (Washington, DC: Fight Crime: Invest in Kids, 2000).

6.) The difference in the proportion with two or more arrests was a bit smaller (9.5 percent vs. 12.8 percent). There was, however, no difference associated with school-age CPC participation, even though this was the only group to show a reduction in school-related infractions. Similarly, there was no impact from extended participation.

These impacts do not seem consistent, because the latter finds an impact for “CPC preschool” and the former for “CPC school-age.” This suggests happenstance or a data problem more than a real impact.

Table 6. CPC: Effects on Delinquency Infractions (Ages 13–15) and Arrests (Age 18)

Type of Comparison	Percent with delinquency infractions		Difference (percentage points)	Percent with two or more arrests		Difference (percentage points)
	CPC group	Comparison group		CPC group	Comparison group	
Any CPC vs. no CPC	12.4%	14.2%	—	NA	NA	NA
CPC preschool vs. no CPC preschool	23.0%	23.0%	—	16.9%	25.1%	-8.2
CPC school-age vs. no CPC school-age	20.4%	26.3%	-5.9	19.8%	19.8%	—
Extended vs. less extended	21.0%	24.0%	—	19.2%	20.1%	—

Source: For delinquency infractions: Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE: University of Nebraska Press, 2000), 103–105; and for arrests: Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2344.

Notes: Only significant differences are reported. “—” indicates that the difference is not statistically significant at the 5 percent level. “NA” indicates that the data were not available. For delinquency infractions, estimates are adjusted for sex and risk status. For arrests, estimates are adjusted for earlier/later (preschool or school-age) program participation, sex of child, risk index, program sites, and race/ethnicity. Data were from official juvenile arrest records in Cook County for children from age ten to seventeen.

Early/nonmarital births. Data apparently either not collected or not reported.

Economic outcomes. Data apparently either not collected or not reported.

Effects on parents. Data apparently either not collected or not reported.

Benefit-cost findings. The Chicago team conducted a benefit-cost analysis of the

Chicago CPC program using the age twenty data reported in their 2001 *JAMA* article.¹²

The Chicago team assessed the benefits and costs of CPC from three perspectives: (1) “participant,” (2) “taxpayer/crime victims,” and (3) “societal.” The “participant” perspective compares program benefits to participants relative to program costs. The “taxpayer/crime victims” perspective compares program benefits (including estimated reductions in government expenditures on remedial education and the criminal justice system, as well as reduced expenditures of crime victims and higher tax revenues for the government) to program costs. The “societal” perspective attempts to combine the participant and taxpayer/victim perspectives. Increased participant earnings are counted as social benefits. In some cases, the benefits and costs of the “participant” and “taxpayer/victim” perspectives simply offset each other. For example, if a program increased tax payments from former participants (because their earnings rose), this would be regarded as a cost to participants, but an offsetting gain to taxpayers.

In addition to the perspectives presented by the Chicago team, this assessment includes the “taxpayer” perspective, adjusting the reported data to deduct savings to crime victims. Our analysis then becomes comparable to similar calculations presented for the Abecedarian Project (see chapter 1), the High/Scope Perry Preschool Project (see chapter 14), and the Elmira Nurse Family Partnership Project (see chapter 18).

For taxpayers/crime victims, the Chicago team conducted benefit-cost analyses separately for the preschool component, the school-age component, and extended vs. limited participation. This summary, however, examines only the impact of the preschool component (because the estimated benefit-cost ratios for the other comparisons were smaller).

Costs. Program costs for the CPC preschool component included expenditures associated with “staff, family and community support, operations and maintenance, instructional materials, transportation and community services, school-wide services, school district support, and capital depreciation and interest.”¹³ The present value of the average cost of the CPC preschool program, based on 1.5 years of participation, was \$8,113 per child, or about \$5,400 per year.¹⁴ (The average cost of school-age participation was about \$1,940 per child per year.)

¹²Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers,” *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 267–303.

¹³Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers,” *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 274–275.

¹⁴This calculation of costs differs from the calculation of costs for the Abecedarian program (see chapter 1). For Abecedarian, the program costs are calculated as net costs or the additional costs beyond the cost of child care for children in the control group. For CPC, the programs costs are average gross costs.

Benefits. Benefits were calculated in several categories: schooling, adult earnings, criminal justice, and welfare expenditures.

The benefits associated with schooling, including reduced spending on special education, were based on data from the school system and the estimated impacts of the intervention. (The added cost of special education was based on the weighted annual cost per pupil of various categories of such costs in the Chicago public school system.) The savings were estimated to be \$839 per child from reduced grade retention and \$5,067 per child from reduced special education placement. There are several issues related to these estimates. First, the estimated impacts were based on the follow-up at age twenty. As noted, there were differences in impacts between the various studies. Had the grade retention (at age fifteen) findings been based on the 2000 book, rather than the 2001 article, they would have been about 50 percent lower. The fact that the findings are so sensitive to the data source suggests caution. Second, the researchers assumed that grade retention would result in an additional year of school, costing \$5,448 per child per year. This may be the case for some students, but those who are retained are also likely to be poorer students and may drop out prior to high school completion, without necessarily spending more time in school. Indeed, this is suggested by the lower reported rates of high school completion. Actual years of schooling would therefore have been a better measure.

The Chicago team estimated a lifetime increase in earnings (and compensation) effect for ages eighteen to sixty-five based on differences in high school completion rates between the program and comparison groups. (These estimates included a 2 percent real income growth rate and a 20 percent fringe benefit rate.) They then estimated the impact on tax revenues, assuming tax revenues equaled 33.3 percent of earnings, based on a 15 percent federal tax rate, a 3 percent state income tax rate, and a 15.3 percent Social Security tax rate (including the employer's share of the tax). The CPC program was, thus, estimated to increase tax payments by \$8,781 for each participant.

The Chicago team derived estimates of savings from reductions in criminal justice system expenditures from reductions in juvenile arrests and from projections of reduced adult criminal activity, based on juvenile arrests. The estimated savings in government expenditures were \$8,644 per participant. They also included reductions in expenditures of crime victims, as savings, basing estimates on "national estimates of the amount and proportion of tangible losses to crime victims for violent and property offenses."¹⁵ This amounted to an additional \$7,428 per participant. It is unclear why the authors used national estimates, however, since most of the crime reduction would have occurred in low-income areas, where property values should be considerably lower. We excluded these savings from the assessment of impacts on taxpayers.

¹⁵Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers," *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 277.

Finally, the Chicago team based the estimates of reductions in child welfare system expenditures on savings associated with reductions in substantiated reports of child maltreatment and the costs associated with handling them.

Benefit-cost ratio. Table 8 summarizes the taxpayer's benefits and costs for the CPC preschool component from four perspectives: participants; taxpayers/crime victims; society; and taxpayers (the latter being our calculations).

Table 7. CPC: Estimated Benefits and Costs per Participant (Preschool Program)

	Participant (Reynolds et al.)	Taxpayers/ Victims (Reynolds et al.)	Society (Reynolds et al.)	Taxpayers (Authors' calculations)
Benefits				
Education				
Grade retention	\$0	\$839	\$839	\$839
Special education	\$0	\$5,067	\$5,067	\$5,067
College tuition	-\$226	-\$450	-\$675	-\$450
Child Care	\$2,009	\$0	\$2,009	\$0
Child welfare				
Government	\$0	\$572	\$572	\$572
Abuse/neglect	\$0	\$361	\$361	\$0
Crime				
Measured (juvenile)				
Government	\$0	\$5,477	\$5,477	\$5,477
Victim	\$0	\$4,107	\$4,107	\$0
Projected (adult)				
Government	\$0	\$3,167	\$3,167	\$3,167
Victim	\$0	\$3,320	\$3,320	\$0
Projected taxes	\$0	-\$8,781	\$0	-\$8,781
Projected post-tax compensation	\$24,872	\$0	\$24,872	\$0
Total benefits	\$26,655	\$31,241	\$57,897	\$23,453
Program cost	\$0	\$8,113	\$8,113	\$8,113
Net present value	\$26,655	\$23,129	\$49,784	\$15,340
Benefit-cost ratio	NA	\$3.85/1	\$7.14/1	\$2.89/1

Source: Adapted from Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers," *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 267–303.

Note: Estimated in 2005 dollars discounted at 3 percent. "NA" indicates that the calculation was not applicable.

The Chicago team estimated that the CPC preschool component had a total benefit to society of \$57,897 per participant, saving \$7.14 for every \$1.00 spent. Most of the estimated benefits accrue to participants and their families. For taxpayers and victims, the net present value is \$23,129, returning \$3.85 for each \$1.00 spent. For taxpayers alone, our adjusted data suggest a net present value of \$15,340, saving \$2.89 for every \$1.00 invested. Each of these estimates are subject to considerable uncertainty due to various data limitations and potential selection bias, described below.

Overall Assessment

The CPC evaluation has major weaknesses that undermine its findings, including high levels of missing data, the absence of a true randomized experiment, and various self-selection biases. These problems are reflected in fairly large differences in estimated effects depending on the completeness of data and statistical methods used.

Program theory. The program sought to promote academic success by providing a “school-stable” learning environment during the preschool and primary-grade years and to involve parents actively in their children’s education.¹⁶ According to the Chicago team, “The theory is that children’s readiness for school entry and beyond can be enriched with systematic language learning activities and opportunities for family support experiences through direct parent involvement in the centers.”¹⁷

Program implementation. The Chicago team reports that evaluations have “confirmed that the program has consistently served the intended target population and successfully provided the expected educational and family support services.”¹⁸ These evaluations indicated that child attendance rates typically exceeded 92 percent and that over 80 percent of CPC parents reported that “they visited the center or had been involved in the instructional program.”¹⁹ Administrative records indicated that over half the parents were actively involved at least two days per month.

Although CPCs provide educational services through second or third grade, some children left before the end of the program, for example, because the parents preferred that their children attend regular public school. So, even though the program is available for up to six years, children were enrolled an average of 3.7 years.

The large estimated effects of some early intervention programs may be due the fact that they were implemented as “model” programs that had enhanced funding for special services or that received extra attention that might not be possible if the programs operated on a larger scale. As noted below, one of the strengths of the CPC evaluation is that it measures the effects of an ongoing program, so these concerns do not apply.

¹⁶Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000).

¹⁷Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 24.

¹⁸Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 48.

¹⁹Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 48.

Assessing the randomization. CPC was not evaluated with a random assignment design.

Assessing statistical controls in experimental nonexperimental evaluations. Because assignment to CPCs was not based on a randomized design, selection bias is a potentially serious problem. The CPC is a voluntary program, raising the possibility that parents who enrolled their children in the program had a greater-than-average interest in their children's development. To the extent that this interest may have been associated with positive child outcomes, as discussed below, the program effects may be overstated. In commenting on the findings, Matthew Thompson of the Children's Hospital in New Orleans wrote:

The intervention group included only children whose caretakers were able or willing to enroll their children in the program and provide for sustained involvement in the program. The control group included children who did not participate in the preschool program because they did not live in the area that offered it. The control group thus includes both children who could have completed the program as well as children who would not have completed the program due to lack of parental involvement, transportation problems, medical problems, or other variables. The intervention group, by definition, included only children who were able to complete the program.²⁰

On the other hand, Reynolds notes, "To enroll children who are most in need and to reduce self-selection, the centers conduct extensive outreach activities such as distributing program descriptions in the community, visiting families door-to-door, and advertising locally."²¹

There was an additional issue of selection bias associated with the sites that became CPC providers, as those schools most interested or able to participate may have volunteered to become CPC sites. David Greenberg, professor of economics at the University of Maryland-Baltimore County, and Mark Shroder, economist at the U.S. Department of Housing and Urban Development, describe this problem as it pertains to similar studies:

Selection factors will also bias comparisons across sites that have and have not implemented an innovation, if, as is usually the case, the decision as to whether to adopt the innovation was made locally. For example, the sites may differ in terms of local economic conditions and in population characteristics.²²

²⁰Matthew D. Thompson, "Early Childhood Educational Intervention and Long-Term Developmental Outcomes," *Journal of the American Medical Association* 286, no. 15 (October 17, 2001): 1835.

²¹Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 24.

²²David Greenberg and Mark Shroder, *The Digest of Social Experiments* (Washington, DC: The Urban Institute Press, 1997), 12.

Reynolds argues, however that:

the decision to become a CPC site was determined by whether the school was located in a high poverty neighborhood and whether no other early childhood programs were available in the area. Thus, participation was need-based. Since its beginning in 1967, the program serves young children in the highest poverty and most disadvantaged neighborhoods of the city. Any bias here would likely be toward more conservative effects.²³

Differences in background characteristics. According to the Chicago team, “program and comparison groups experienced similar levels of social disadvantage.”²⁴ But, the measurement of actual background characteristics suggest far less confidence in the comparability of the two groups than would have been the case with a randomized experiment. For example, a comparison of the background characteristics of the CPC preschool group with the no CPC preschool group shows four statistically significant differences (percent female, percent in high schools with poverty rates at 60 percent or above, percent of parents who completed high school, and average number of siblings) out of twelve measured (at the 5 percent level).²⁵ In addition, two neighborhood characteristics—poverty and unemployment rates when the children were four years old—were statistically different from one another.²⁶ According to Reynolds, “These differences cut both ways; 3 favor the comparison group and three favor the program group. In effect, these influences cancel each other out.”²⁷ Yet, it is not at all clear that these influences cancel each other out, as some may have more bearing on a particular outcome than others. Moreover, since complete data on the background characteristics were limited (a problem discussed in more detail below), it is possible that the number of characteristics favoring one group or another would differ had data collection been more extensive. In a later study, the Chicago team notes that members of the treatment group were more likely to be female and more

²³Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, December 14, 2000.

²⁴Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 73.

²⁵Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2342.

²⁶Similarly, a comparison of the CPC and no-CPC groups shows two statistically significant differences out of seven possible at the 5 percent level, where one would expect only one difference in twenty if assignment of children was made randomly. CPC recipients had fewer siblings (2.4 vs. 2.7) and their parents were more likely to be high school graduates (59.7 percent vs. 50.9 percent). Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 56.

²⁷Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, May 29, 2001.

likely to suffer from acute poverty than were members of the control group.²⁸

In addition, the Chicago team provided the relevant information demonstrating the comparability of the groups for only a limited number of comparisons. For example, they presented the background characteristics for the preschool sample that was available for follow-up at age twenty, but not for any CPC vs. no CPC, school-age CPC vs. no school-age CPC, or extended CPC vs. nonextended CPC.²⁹ Although many of the same families were involved, there was considerable movement among the groups and it is not clear that the comparability across various CPC and comparison groups would apply to all analyses.

Unobserved differences. There may have been other unobserved differences between the two groups, such as the motivation of parents. Because the CPC was a voluntary program, it could be that the parents most interested in their children's well-being enrolled them in the CPC program. For the any CPC vs. no CPC comparison, the comparison group was primarily children in non-CPC areas. As the Chicago team observes:

Children in the comparison group of this quasi-experimental study did not enroll in the CPCs primarily because they did not live in a neighborhood containing such a center. Thus, geographic location, rather than family motivation or other self-selection factors determined nonparticipation.³⁰

Geographic location may have meant that comparison families could not get into the CPC program (at least initially), but it did nothing to eliminate the possible self-selection of the more motivated parents in the CPC areas. After all, the choice to place a child in preschool reflects selection factors, and it was not clear how many parents of children in the comparison group would have done so had they had the opportunity. If the program was attractive, it could have led some parents to move to a CPC schools (or at least could have been a factor in their migration decisions), also creating a selection issue.

In addition, for some comparisons, the geographic distinction had limited applicability. Indeed, for three of the comparisons (preschool CPC vs. no preschool CPC, school-age CPC vs. no school-age CPC, and extended CPC vs. nonextended CPC), some children who participated in

²⁸ Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers," *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 271

²⁹ Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools," *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2342.

³⁰ Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 57.

CPC were included in the comparison group. For example, in the preschool CPC vs. no preschool CPC comparison, 30 percent of the comparison group participated in CPC during their school-age years.

The Chicago team used a variety of statistical approaches for dealing with possible selection bias. Their estimates were consistent across these procedures. As with any statistical modeling approach, only those unmeasured variables that are associated with measurable proxy variables can be modeled. Given the data limitations described above, uncertainty remains.

Neighborhood differences. Data on and controls for neighborhood characteristics were also limited. While some of the Chicago team's tables suggested that the neighborhoods were comparable in terms of poverty in a given year, there may have been important differences in the *trends* in poverty. For example, the Chicago team compared the background characteristics of the program and comparison groups (for the any CPC vs. no CPC comparison) at school entry, giving the school poverty rate as 66.5 percent for the CPC group and 67.5 percent for the no-CPC group.³¹ Although this was an important indicator of economic conditions at the time of school entry, children's outcomes may also have been affected by changes in these conditions over time. The Chicago team's data on trends in poverty rates show that they were quite different across community areas served by CPCs (and presumably the areas serving the comparison group children).³² Thus, a comparison at a given point in time could be misleading.³³ Moreover, the poverty rate is just one of many important neighborhood indicators. There appears to have been many changes affecting Chicago neighborhoods throughout this period, including changes in unemployment, school spending, spending on other social supports, crime rates, and notably, a nearly 40 percent reduction in the number of residents living in the CPC communities in the 1970–1990 period. The evaluation attempted to statistically control for site variation, but whether it captured the many differences and changes over time is questionable.

Further compounding the difficulty in adjusting for neighborhood differences is that, depending on the comparison group, children did not remain in distinct neighborhoods. There

³¹Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 56.

³²Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 30. The poverty rate for CPC community areas grew from 25.9 percent in 1970 to 40.5 percent in 1990 (a 56 percent increase), whereas the poverty rate for non-CPC community areas grew from 7.7 percent in 1970 to 16.9 percent in 1990 (a 119 percent increase). The poverty rate in the comparison areas was not reported.

³³If children were randomly assigned, the concern about neighborhood differences over time would be diminished (unless the intervention affected migration decisions), but with a comparison site design, it is likely that children from the different groups will move to different kinds of communities throughout childhood, which in turn could affect many outcomes.

was a lot of mixing. For example, the Chicago team compared children with extended CPC participation (four to six years) to those with limited participation (one to three years). Those with extended participation probably remained in the same neighborhoods throughout most of their early childhood years, but many of those in the limited participation group either moved out of or into the CPC community area, which exposed them to different neighborhoods. If some of the preschool CPC participants moved to better neighborhoods (and schools), part of their superior performance at age twenty may have been due to their new surroundings, rather than to the CPC program itself, thus overstating the impact of the program. The Chicago team suggests that such migration may have been due to the program:

The CPC kids are more likely to go to schools with higher achieving students. But this is a mediator of the effects of the program on later achievement, etc. It can't be a source of selection bias because it happened after the program was over. The group went to better schools because they got a cognitive and family support advantage from program participation.³⁴

But attributing such migration to the program goes beyond the scope of the evaluation. It was quite possible that the parents of CPC children would have moved to better schools even without participating in the CPC program. With random assignment, it might have been possible to conclude that this was a consequence of the program but, with a comparison site design, this is quite difficult to judge with any certainty. As John Love and his colleagues at Mathematica Policy Research, Inc. point out:

Most longitudinal studies do not control for events related to child outcomes and center choice that occurred during the follow-up period. For example, if children who attended high-quality centers are more likely than children who attend lower-quality centers to subsequently attend better schools, then the estimated impacts of center quality on child outcomes will be confounded with the effects of these events. Consequently, the estimated center quality impacts are likely to be biased upward. We believe that more complete follow-up data on child experiences and environmental changes should be obtained in future studies employing longitudinal designs, because these events are likely to have a substantial effect on child development outcomes.³⁵

Similarly, Greenberg and Shroder described the problems comparison site designs have in controlling for these influences: “An obvious problem with the geographic and chronological comparison groups is that economic and social circumstances differ from place to place and

³⁴Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, May 11, 2001.

³⁵John M. Love, Peter Z. Schochet, and Alicia L. Meckstroth, *Are They in Any Real Danger? What Research Does—and Doesn't—Tell Us About Child Care Quality and Children's Well-Being* (Mathematica Policy Research, Inc., Princeton, NJ, May 1996), 41.

change over time; it is very difficult to control for these factors in a manner that allows the effects of the innovation to be isolated.”³⁶

Estimating preschool impacts. The Chicago team’s study attempts to measure the impact of preschool participation. This is done by comparing those children who participated in CPC preschool to those who did not. One complication, however, is that children in either group could have participated in the school-age component. Aside from differences in background demographic characteristics, there could be differences in school-age CPC participation that account for some of the differences in outcomes. If, for example, the CPC preschool group got more of the school-age intervention, this could overstate the impact by simply comparing the groups. The Chicago team indicated that the “effects of each program component were estimated while controlling for the influence of the other.”³⁷ This aggravates the possible selection bias problem, in that the program group includes some people who opted out of the CPC program while the comparison group includes some who opted in (during the school-age years). (The same problem arises in estimating the school-age component, since it required controlling for the impact of preschool.) Thus, the preschool and school-age findings should be treated much more cautiously than the CPC vs. no CPC comparisons, because of the difficulty of successfully disentangling the preschool and school-age components.

Limited covariates. A fairly narrow number of covariates was used to control for background differences between the program and comparison groups. For example, in the Chicago team’s *JAMA* paper, the covariates were sex of child, race/ethnicity, risk index, earlier/later program participation, and twenty dummy variables reflecting the sites of the program. The Chicago team notes: “Results were unaffected by alternative covariate specifications, such as individual risk indicators entered separately, and the addition of other indicators of family and neighborhood disadvantage.”³⁸ Much of the data for the covariates were imputed because of the amount of missing data. This problem affected the “risk index” as well.

Uncertainty. The Chicago team was aware of the potential selection bias problem and adopted sophisticated statistical models to deal with it. As with most (if not all) nonexperimental studies, however, there is disagreement about the success of efforts to control for possible selection bias. On the one hand, Janet Currie, professor of economics at University of California,

³⁶David Greenberg and Mark Shroder, *The Digest of Social Experiments* (Washington, DC: The Urban Institute Press, 1997), 12.

³⁷Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2340.

³⁸Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2342.

Los Angeles, is quite positive about their efforts:

Although his study cannot meet the gold standard of random assignment, Reynolds' study is interesting in part because he uses several different statistical methods to control for the possibly unobserved characteristics of the (non-randomly assigned) intervention and control children. Reynolds' results are robust to the use of different methodologies. Most notably he finds significant reductions in the rates of grade retention, special education, and delinquency in the treatment group, as well as higher reading scores.³⁹

On the other hand, Mark Lipsey, professor of public policy at Vanderbilt University's Peabody College, and his colleagues, Peter H. Rossi and Howard E. Freeman, express some uncertainty about the results:

Reynolds and Temple (1995) examined different approaches to selection modeling controls for producing quasi-experimental estimates of school achievement effects from Chicago's Child Parent Center preschool program. Two variations each of econometric modeling, latent-variable structural modeling, and ordinary least squares regression produced similar estimates of program effects. No results from a randomized design were available for this program to provide a standard, which leaves some ambiguity about how valid these estimates were despite their consistency.⁴⁰

Sample size. The data for the study came from the Chicago Longitudinal Study, with information on 1,150 children who enrolled in the twenty CPCs with preschool programs and kindergarten between 1983 and 1985, and 389 comparison group children. This was a relatively large sample that supported a number of different comparisons.

Attrition. At age twenty, educational attainment data were available for 83.2 percent of the original sample. For the preschool comparison, the recovery rate was 84.6 percent for the CPC group and 80.7 percent for the comparison group, "with no evidence of selective attrition."⁴¹ For some of the earlier studies, attrition was a little bit higher, but still quite low given the

³⁹Janet Currie, "Early Childhood Intervention Programs: What Do We Know?" (paper, Brookings Institution's Roundtable on Children, Washington, DC, January 2000), 19–20.

⁴⁰Peter H. Rossi, Howard E. Freeman, and Mark W. Lipsey, *Evaluation: A Systematic Approach*, 6th ed. (Thousand Oaks, CA: Sage Publications, 1999), 333.

⁴¹Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools," *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2341.

number of years that had passed.⁴² Although the overall rate of attrition was low, assessing whether the characteristics of those dropping out varied by group assignment was somewhat difficult. Ordinarily, the low attrition rate would suggest that attrition-related bias was not a problem, but as described earlier, the findings related to grade retention and special education placement changed substantially when additional data were collected, which raises the possibility of attrition-related problems.

Data collection. The data collection relied on a wide range of standardized tests (administered by schools), school records (including teacher ratings), criminal records, and student surveys. The data sources are appropriate for the questions being studied, but missing data were a problem for assessing the comparability of the program and comparison groups. For example, data were missing on either parent education or subsidized meal eligibility (both factors used in the calculation of the risk index) for 25 percent of the CPC preschool group and 30 percent of the comparison group.⁴³ (Even higher rates of missing data were reported in earlier studies for some background variables, with missing information on parental unemployment and single-parent status apparently exceeding 50 percent.)⁴⁴ Since relatively complete information existed for only a few background variables, it is not possible to determine with any certainty whether the CPC and comparison groups were comparable or not. And, as noted, the information that is provided suggests that there may have been important differences.

Given some of the observed differences, it would have been important to control for as many of them as possible, but the amount of missing data made this problematic. Reynolds contends that this problem should not be a worry:

In an analysis of the characteristics of those missing and not missing data we see no major differences between groups. Any differences that did exist seem to be a function of other measured factors, like early school performance. Because the groups were selected from a homogeneous population of high risk families in the same city, the effects of missing data

⁴²By age fourteen, 76 percent of the children were still in the Chicago public school system, although this percentage was larger for the program group than the comparison group (77 percent vs. 72 percent), although only 70 percent were still in the Chicago public school system at age fifteen. The Chicago team reports that there was no selective attrition: "Although CPC participants were more likely to be active in the study sample, the differences were not significant for the most part, and no selective attrition has occurred on youth outcomes in this study or in the previous ones." Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 56.

⁴³Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools," *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2342.

⁴⁴Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 72. Even these comparisons are based on just 76 percent of the original group, reduced due to attrition.

are nothing like you would find in a national study or in the use of convenience samples. The incomes and family structure of parents who participate in Title I programs in Chicago are very similar, especially in early intervention programs. That is why we've never been concerned about single parent family status and income in our models, and instead opt for the index.⁴⁵

An imputation procedure for children with missing data may have helped, but it did not erase uncertainty about the comparability of the groups. Other researchers have found that even within groups that are homogeneous with respect to poverty and other sociodemographic characteristics, populations can be quite heterogeneous with respect to their coping capacities.⁴⁶ These differences in parental coping capacities can have profound effects on the care, and thus the functioning, of children. These differences were also likely to affect the extent to which parents made use of various helping services, such as the CPC program, which required some parental effort in order to participate.

Although the data for most outcomes was relatively complete, the use of administrative data was limited. The confidence surrounding some of the survey findings, particularly those dealing with crime could have been strengthened by obtaining data from criminal records. This may become more important if a subsequent follow-up examines impacts on employment, earnings, and welfare use.

Measurement issues. No background characteristics were collected when the children enrolled in the CPC or were of preschool age (for the comparison group). Instead, they were collected at later points. For example, the parent interview in September 1997 (when the child was about seventeen years old), asked: "Think back to when your child started preschool or was preschool age, what was your employment status?" Similar questions were asked about the parent's highest level of education, the frequency of parental participation in preschool or kindergarten and other activities. Although the information for most children was collected during earlier stages, the fact that none was collected at enrollment raises questions concerning its accuracy, since respondent recall is likely to be less accurate the longer the gap between preschool and the survey. Of course, if there was a bias, it would probably have affected both program and comparison groups.

This time lag was not as serious for the key outcome variables, which were taken from standardized tests (administered by schools), school records (including teacher ratings), criminal

⁴⁵Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, May 29, 2001.

⁴⁶See, for example, David L. Olds, Charles R. Henderson, Harriet Kitzman, John Eckenrode, Robert Cole, Robert Tatelbaum, JoAnn Robinson, Lisa M. Pettitt, Ruth O'Brien, and Peggy Hill, "Prenatal and Infancy Home Visitation by Nurses: A Program of Research," in *Advances in Infancy Research: Volume 12*, ed. Carolyn Rovee-Collier, Lewis P. Lipsitt, and Harlene Hayne (Stamford, CT: Ablex Publishing Corp., 1998), 79–130.

records, and student surveys. Some bias, however, could have been introduced if the schools serving the CPC and comparison group children had different standards for special education and grade retention. Similarly, differences in arrest rates could reflect differences in law enforcement practices. Reynolds believes that such differences are likely to impart a conservative bias to the findings: “In our study, children went to very similar schools, usually the same schools. CPC kids are more likely to go to magnet schools but magnet schools have higher standards and it is easier to get retained or be provided with special ed (conservative bias).”⁴⁷ He may be correct, but the greater likelihood of attending a magnet school was a confounding factor.

Generalizability. Much of the early evidence for the success of early education programs comes from small model programs with special staff and extensive resources. The CPC program is a large public program with a large sample and thus is more representative of what programs really look like in the field. The Chicago team asserts that the evaluation’s large sample of children in an established program means that “generalizability extends to low-income children in many public programs in central cities.”⁴⁸ Subject to the underlying weakness of the study, this seems to be a reasonable assertion.⁴⁹

Replication. The CPCs have only been evaluated for one cohort of children in one set of Chicago schools. This is a serious limitation. Jonathan Crane, director of the National Center for Research on Social Programs, describes one possible replication:

the CPCs have essentially been replicated and scaled up. There are twenty sites in Chicago, which have served thousands of children. An analysis that defines each cohort at each site as a single case . . . should be carried out to replicate the basic results. It would be preferable for such a study to use different cohorts, so that those findings are completely independent . . .⁵⁰

Even more useful would have been an evaluation using an experimental design.

⁴⁷Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, May 29, 2001.

⁴⁸Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 63.

⁴⁹Reynolds noted: “Kids in the extended program are a little more advantaged as far as achievement goes. That is why we added kindergarten achievement as a control variable in the JAMA paper and in the book. As for the 4 groups, the only one that looks a little different is the preschool + school age group. They are more advantaged since they tended to move less. Controlling for kindergarten achievement, however, removes this difference. We do note the similarity of groups in the paper. You get a sense of this by examining the tables with 1,150 vs. 389 vs. the ones with 989 vs. 550.” Arthur J. Reynolds, University of Wisconsin-Madison, e-mail message to Peter Germanis, May 29, 2001.

⁵⁰Jonathan Crane, “Building on Success,” in *Social Programs that Work*, ed. Jonathan Crane (New York: Russell Sage Foundation, 1998), 16–17.

Evaluator’s description of findings. The Chicago team concludes that the program should be implemented on a larger scale to allow for further research into its effectiveness:

Based on the findings presented in the monograph and those of previous studies in the Chicago Longitudinal Study, early and extended childhood interventions like the Child-Parent Center Program should be implemented on a larger scale. . . . The findings of this study clearly indicate that federally funded early childhood programs can make a difference in the short and long term. . . . Replication of the program in other schools and cities would be valuable in determining the impact in other contexts.⁵¹

Given the potential selection bias problems, an additional caveat would be for some replications to be based on experimental design. As Matthew Thompson cautions, “If policy makers mistakenly accept the conclusion that preschool intervention results in less criminal activity later, they may mistakenly invest in these programs when the money might be better invested in parenting-skill programs and other interventions to increase parental involvement.”⁵²

Evaluator’s independence. The Chicago team consists of independent researchers located in academic settings.

Statistical significance/confidence intervals. Statistical significance was measured and reported at the 5 percent level.

Effect sizes. At age six, for children participating in the CPC preschool/kindergarten program, most reported cognitive effect sizes fell within the range of 0.35–0.58 SD. At age twelve, most reported cognitive effect sizes fell within the range of 0.26–0.38 SD. Beyond the age of twelve, cognitive effect sizes were apparently not reported for children participating in the CPC preschool/kindergarten program. At age nine, for children participating in the extended program, effect sizes were 0.55 and 0.48 SD for reading and math achievement tests, respectively. At age eleven, these effect sizes fell to 0.29 and 0.22 SD; and at age thirteen, they rose to 0.43 and 0.28 SD.⁵³

The Chicago team describes effect sizes of larger than 0.2 SD “as beyond the level of

⁵¹Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 191.

⁵²Matthew D. Thompson, “Early Childhood Educational Intervention and Long-Term Developmental Outcomes,” *Journal of the American Medical Association* 286, no. 15 (October 17, 2001): 1835.

⁵³These effect sizes were reported in a number of different studies, see Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 58–59.

practical significance”⁵⁴ or “educationally significant.”⁵⁵ In justifying this threshold, the Chicago team notes:

An [effect size] of 0.20 is equivalent to a correlation of 0.1 between program participation and outcome. It also is equal to a 22% improvement in success rate over the comparison group in the binomial effect size display. . . . As with statistical significance, thresholds of practical significance are inherently arbitrary. These identified thresholds, however, are consistent with those used in prior studies of this data set and with other analyses of social programs (Lipsey and Wilson, 1993).”⁵⁶

Under traditional demarcations, however, effect sizes in the range of 0.2 to 0.5 SD would be considered “small.”⁵⁷

Sustained effects. The evaluation examined impacts through age twenty-one, about ten to thirteen years after the intervention ended.

Benefit-cost analysis. A benefit-cost analysis was conducted by the Chicago team, and is discussed in the “Benefit-cost findings” section.

Cost-effectiveness analysis. Apparently not performed.

⁵⁴Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 135.

⁵⁵Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 58.

⁵⁶Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000), 135.

⁵⁷Jacob Cohen, *Statistical Power Analysis for the Behavioral Sciences* (Hillsdale, NJ: Lawrence Erlbaum, 1988), 535.

Commentary

Arthur J. Reynolds and Judy A. Temple*

The overall record of social programs in improving the lives of children and families is weak. Few well-evaluated programs have consistently demonstrated beneficial effects. To describe this state of affairs, sociologist Peter Rossi coined the “Iron Law of Evaluation,” which states that the expected impact of social programs is approximately zero. So when programs such as the Child-Parent Centers and others described in this book show positive long-term effects, the first question understandably is “why?”

Unlike in other areas of social research, early childhood interventions commonly demonstrate positive effects. For over four decades, participation in a wide variety of programs in the first few years of life has been associated with many positive outcomes for children and families including increased school readiness, cognitive ability, and school achievement in the short term, reduced need for special education services and grade retention in the middle term, and lower rates of delinquency and higher levels of educational attainment in the long-term.¹ Effect sizes are large, ranging from one-half to three-quarters of a standard deviation at the end of the program to 30 to 50 percent reductions over control groups in rates of grade retention and special education during the school-age years. These findings have occurred despite differences across programs in time period, location, organizational system, curriculum, and research methodology. The qualifying condition is that the intervention is implemented well, a key assumption of any evaluation of program impact.

Contrast this with the evidence base for other social programs, practices, and services—such as class size reductions, job training programs, child welfare services, and programs to prevent drug abuse, school dropout, and delinquency. These interventions provide at best inconsistent evidence of benefits, and most often an absence of long-term effects.²

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¹W. Steven Barnett, “Long-Term Effects on Cognitive Development and School Success,” in *Early Care and Education for Children in Poverty: Promises, Programs, and Long-Term Results* ed. W. Steven Barnett and Sarane S. Boocock (Albany, NY: State University of New York Press, 1998), 11–44; and Lynn A. Karoly, Peter W. Greenwood, Susan S. Everingham, Jill Hoube, M. Rebecca Kilburn, C. Peter Rydell, Matthew Sanders, and James Chiesa, *Investing in Our Children: What We Know and Don’t Know About the Costs and Benefits of Early Childhood Interventions* (Santa Monica, CA: RAND, 1998).

²James J. Heckman, “Policies to Foster Human Capital,” *Research in Economics* 54, no. 1 (March 2002), 3–56; and Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000).

Research in the Chicago Longitudinal Study of the Chicago Child-Parent Center (CPC) program is consistent with the overwhelming evidence that early childhood interventions promote children's success. CPC has all the features of effective programs. The program begins at age three, prior to the onset of school difficulties and other behavioral problems. It is intensive. One year of half-day preschool has 630 hours, including a 6-week summer program. Kindergarten has 630 to 1260 hours, based on three or six hours per day. The school-age component is a full-day program. At a minimum, children enrolled for two years and up to six years. Comprehensive services are provided including classroom instruction in literacy, parent training, and health and social services. In contrast, the total amount of contact time for other programs for young people rarely exceeds 30 hours and may last no more than a few months. For these reasons, early childhood programs like the CPCs should promote children's well-being more than other programs.³

These findings and program facts address two of the criticisms of our research by Douglas Besharov and his colleagues. First, findings of the CPC program are not unusual in the annals of early childhood intervention; they are largely consistent with those of other programs over the past four decades, among them the Consortium for Longitudinal Studies, the High/Scope Perry Preschool Program, and the Carolina Abecedarian Project. The difference is that the CPC program is an established federally funded program run by the Chicago public schools while the others were pilot projects designed for demonstration purposes. Second, given the scope, intensity, and length of services, the CPC program should promote children's school and social success over time. This is why the program violates the Iron Law of Evaluation.

In the rest of this response, we address two major criticisms of our study: (a) that missing data lead to uncertain estimates of effects and (b) that the quasi-experimental design compromises the capacity to make causal inferences about program effects. These issues have been addressed thoroughly in many previous studies but we take this opportunity to summarize the key findings, including with recent data. For parsimony, we emphasize findings for the CPC preschool component. Finally, we provide an alternative perspective on the benefits of randomized experiments.

Missing Data

Besharov and his colleagues indicate that our findings do not inspire confidence because of missing data on background variables. Data can be missing for many reasons (e.g., attrition), and can occur randomly or nonrandomly. Data missing randomly do not affect estimated program effects, only statistical power. Nonrandomly missing data affect estimates only if the participants with missing data are likely to have different outcomes and if they are different from participants without missing data in ways that are not accounted for by other measured variables.

³Arthur J. Reynolds, Margaret C. Wang, and Herbert J. Walberg, eds., *Early Childhood Programs for a New Century* (Washington, DC: CWLA Press, 2003).

Selective attrition from missing data is the greatest concern. Impact estimates would be biased upward (too high) if a greater percentage of the most disadvantaged children are lost from the program than the comparison group, independent of measured variables. Estimates would be biased downward (too low) if a greater percentage of the most advantaged children—who benefitted from the program—were lost or had missing data.

Neither situation has occurred in our studies. Indeed, all of our tests for selective attrition indicated that the type of child who left the study or had missing data is similar for program and comparison groups. To illustrate, we compare estimates of the effects of CPC preschool participation reported in the age fourteen/fifteen follow up⁴ to the most recent studies up to age twenty-one.⁵ In the age fourteen/fifteen study, only children who were active in the Chicago public schools at age fourteen were included (1,164 out of 1,539). In the most recent follow up, all available children were included (1,281 or more) and, compared to the earlier study, family information to determine risk status (e.g., parent education, family structure) was available for significantly more children from birth records and social service records.

Do these new data and larger sample sizes yield estimates similar to the age fourteen/fifteen study? As shown in Table 1, the answer is yes. For all analyses, regardless of changes to the family risk index, sample size, and the covariates, CPC preschool participants had significantly higher reading achievement test scores than the comparison group, and lower rates of special education placement and grade retention. Rates of high school completion also significantly favored the program group. Participation also was significantly associated with delinquency prevention.

Interestingly, the performance advantage of the CPC preschool group over the comparison group usually increased as sample size and age of assessment increased. For example, Reynolds⁶ found that the cumulative rate of special education by age fourteen for the preschool group was 5.2 percentage points lower than in the comparison group. This difference remained when the revised family risk index was included. The revised risk index included six indicators assessed by age eight (e.g., low parent education) and 4 percent of the sample had missing data for more than two indicators (86 percent were missing no more than one). The risk

⁴Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000).

⁵Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A Fifteen-Year Follow-Up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2339-2346; and Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers,” *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 267–303.

⁶Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000).

index in the age fourteen/fifteen follow up included eight indicators assessed by age twelve and 18 percent of the sample was missing more than two indicators. Group differences increased with a larger sample of children without regard to their school location. Finally, the rate of special education placement by age eighteen for the preschool group was 10.2 percentage points lower than in the comparison group.⁷ This latter finding was obtained by including program sites and race/ethnicity as covariates.⁸ Findings are nearly identical using different combinations of risk indicators and including risk indicators individually.

Overall, these results indicate that CPC program participation is associated with significant improvements in children's success across a wide range models and samples. Findings also indicate that missing data had little if any effect on the pattern of findings. Finally, for the outcomes of special education placement and grade retention, the effects of preschool participation increase rather than fade over time. This pattern also occurred for child maltreatment and delinquency.⁹

Quasi-Experimental Design

The second major issue raised by Besharov and his colleagues is that because our study is based on a quasi-experimental design, confidence in our findings is lower than if we used an experimental design. In many respects, this criticism is moot because a randomized design was not possible for our study. Given the program's record of success since opening in 1967 and the commitment to serve the families most in need, a true experimental design could not have been implemented successfully. We used the best available design, a matched-group quasi-experimental design in which the performance of a complete cohort of CPC kindergartners was contrasted with children of the same age and economic circumstances who participated in alternative interventions. Below we describe why the design of our study is strong and findings conclusive. We discuss our perspective on the use of randomized experiments toward the end of the paper.

⁷Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, "Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools," *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2339–2446.

⁸Adding program sites to the model, the adjusted rates of special education by age 14 for program and comparison groups were 13.7 percent and 24.1 percent (model 1) and 11.8 percent and 23.2 percent (model 2). The respective values for grade retention were 23.3 percent and 36.6 percent, and 24.1 percent and 38.0 percent; and for age 14 reading achievement they were 147.2 and 140.1, and 147.4 and 140.4.

⁹Arthur J. Reynolds, *Prevention and cost effectiveness in early childhood education* (paper, National Conference of State Legislatures, Denver, CO, July 24, 2002); and Arthur J. Reynolds and Dylan L. Robertson, "School-Based Early Intervention and Later Child Maltreatment in the Chicago Longitudinal Study," *Child Development* 74, no. 1 (February 2003): 3–26.

We believe that our findings are underestimated (conservative) rather than overestimated as indicated by Besharov and his colleagues. Besides the fact that intervention and comparison groups were well-matched on many family risk factors, the entire comparison group participated in an alternative intervention—full-day kindergarten—in which parental involvement also was emphasized. This point is worth underscoring. While all students were eligible for and participated in enriched early childhood programs that encouraged parent involvement, parents of the CPC students had access to more resources such as parenting skills workshops and opportunities for educational development that enhanced their involvement in children’s education. Had the comparison group participated in the most common alternative—no intervention or part-day kindergarten—effect sizes in our study would have been even larger. As noted in Reynolds et al., “findings in this report estimate the value added by the CPC program above and beyond participation in more typical programs.”¹⁰ Most program evaluations, including early childhood programs, contrast treatment with no treatment of any type. This conservative bias in our study strengthens the conclusions.¹¹

Besides the fact that the comparison group participated in an alternative intervention, the following study findings strengthen the validity of our findings:

1. Estimates of effects are robust against alternative models, corrections for attrition, for selection bias associated with unmeasured variables, and measurement errors. We have done formal studies of selection bias in the CLS using the best available econometric and psychometric methods.¹²
2. A dosage-response relation has been found for school achievement, grade retention, and special education in which children’s adjustment increases as a function of years of CPC participation.¹³

¹⁰Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Long-Term Effects of an Early Childhood Intervention on Educational Achievement and Juvenile Arrest: A 15-Year Follow-up of Low-Income Children in Public Schools,” *Journal of the American Medical Association* 285, no. 18 (May 9, 2001): 2341.

¹¹Arthur J. Reynolds and Judy A. Temple, “Early childhood educational intervention and long-term developmental outcomes,” *Journal of the American Medical Association* 286, no. 15 (October 17, 2001): 1835–1836.

¹²Arthur J. Reynolds and Judy A. Temple, “Quasi-Experimental Estimates of the Effects of a Preschool Intervention: Psychometric and Econometric Comparisons,” *Evaluation Review* 19, no. 4 (August 1995): 347–373; Arthur J. Reynolds and Judy A. Temple, “Extended Early Childhood Intervention and School Achievement: Age Thirteen Findings From the Chicago Longitudinal Study,” *Child Development* 69, no. 1 (February 1998): 231–246; and Judy A. Temple, Arthur J. Reynolds, and Wendy T. Miedel, “Can Early Intervention Prevent High School Dropout? Evidence From the Chicago Longitudinal Study,” *Urban Education* 35, no. 1 (March 2000): 31–56.

¹³Arthur J. Reynolds, “Effects of a Preschool Plus Follow-On Intervention for Children at Risk,” *Developmental Psychology* 30, no. 6 (November 1994): 787–804; and Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000).

3. The pattern of effects is consistent with the expectations of the theory of the program. The largest immediate effects are for the school readiness skills, parental involvement in school, and then later to school achievement and reduced need for special education and grade retention.¹⁴ Studies of earlier CPC cohorts also have found similar patterns of effects.¹⁵

4. Effect sizes for several education outcomes are largest for children most at risk: boys and those residing in the highest poverty neighborhoods.¹⁶

5. Mechanisms of effects have been identified and they are consistent with the theory of the program. They are the cognitive advantage, school support, and family support hypotheses. Program children enter kindergarten at higher levels of scholastic readiness than their comparison counterparts, they are more likely to attend better quality schools, and parents are more involved in their education. These inter-related links culminate in long-term educational and social effects.¹⁷

Thus, our confidence is high that the group differences that have been found over the past fifteen years reflect the effects of CPC participation.

Principles of Experimentation

We now turn to the larger issue of social experimentation. Random assignment to “treatments” is implemented primarily in situations when uncertainty is great about likely benefits and when there is a possibility of harmful effects. Efficacy trials of new medical drugs

¹⁴Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000); Arthur J. Reynolds, Suh-Ruu Ou, and James W. Topitzes, “Paths of Effects of Early Childhood Intervention on Educational Attainment and Delinquency: A Confirmatory Analysis of the Chicago Child-Parent Centers,” *Child Development* 75, no. 5 (September-October 2004):1299–1328.

¹⁵Kendon J. Conrad and Maurice J. Eash, “Measuring Implementation and Multiple Outcomes in a Child Parent Center Compensatory Education Program.” *American Educational Research Journal* 20 (1983): 221–236; J.S. Fuerst and Dorothy Fuerst, “Chicago Experience with an Early Childhood Program: The Special Case of the Child Parent Center Program,” *Urban Education* 28, no. 1 (1993): 69-96; and A. Jackson Stenner and Siegfried G. Mueller, “A Successful Compensatory Education Model,” *Phi Delta Kappan* 55, no. 4 (December 1973): 246–248.

¹⁶Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000); and Arthur J. Reynolds, Judy A. Temple, Dylan L. Robertson, and Emily A. Mann, “Age Twenty-One Cost-Benefit Analysis of the Title I Chicago Child-Parent Centers,” *Educational Evaluation and Policy Analysis* 24, no. 4 (Winter 2002): 267–303.

¹⁷Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000); and Arthur J. Reynolds and Dylan L. Robertson, “School-Based Early Intervention and Later Child Maltreatment in the Chicago Longitudinal Study,” *Child Development* 74, no. 1 (February 2003): 3–26.

are the best examples of the uncertainty principle in experimentation. At a basic level, random assignment to CPC program and comparison groups would have violated two key conditions for randomization. First, there was no uncertainty about the beneficial effects of participation. Previous evaluations of the program in the 1960s and 1970s found consistently positive effects and no evidence of harmful effects. Second, as a federal Title I program, CPC is designed to serve children with the greatest educational needs. Participation by lottery in an established program like CPC that is serving the public would raise serious ethical concerns not to mention dissatisfaction among many families.

Within this context, randomization is likely to introduce other biases such as administrative actions to provide CPC-like services to children randomly assigned to the comparison group (compensatory equalization) or decisions by parents to obtain similar intervention services for their children (compensatory rivalry or treatment imitation). Valid estimation of program effects is very difficult in these circumstances. These problems occurred in the recent evaluation of the National Head Start-Public School Transition Demonstration Project. Such artifacts of research design are more likely to be avoided in designs like ours.

In early intervention as in other areas of evaluation, those arguing for randomized experiments downplay or ignore the established wisdom of philosophy of science from Popper to Campbell that: (a) validity is a property of knowledge claims, not research methods; (b) experiments and other research approaches can only probe causal hypotheses, not prove them to be true in any absolute sense; and (c) causal inference is strengthened by the identification of mechanisms that account for estimated program effects. This last point is a major tenet of confirmatory program evaluation.¹⁸ Through systematic analysis of the theory of the program in relation to the pattern of findings, the confirmatory approach enhances the validity of knowledge and has been a key feature in interpreting findings in our Chicago studies.¹⁹

In the 1960s and 1970s randomized experiments were needed to demonstrate if early interventions could improve children's learning. This was the first generation of studies. Once empirical support became clear, the major question shifted to whether programs could be effective on a larger scale when they became established. For this second generation of studies, including our study of the CPC program, larger samples were needed within settings in which complete experimental control was not possible.

¹⁸Arthur J. Reynolds, "Confirmatory Program Evaluation: A Method for Strengthening Causal Inference," *American Journal of Evaluation* 19, no. 2 (June 1998): 203–221.

¹⁹Arthur J. Reynolds, *Success in Early Intervention: The Chicago Child-Parent Centers* (Lincoln, NE.: University of Nebraska Press, 2000); Arthur J. Reynolds, Nancy A. Mavrogenes, Nikolaus Bezruczko, and Mavis Hagemann, "Cognitive and Family Support Mediators of Preschool Effectiveness: A Confirmatory Analysis," *Child Development* 67, no. 3 (June 1996): 1119–1140; and Arthur J. Reynolds and Dylan L. Robertson, "School-Based Early Intervention and Later Child Maltreatment in the Chicago Longitudinal Study," *Child Development* 74, no. 1 (February 2003): 3–26.

Conclusion

Research in the Chicago Longitudinal Study has consistently found that participation in the Child-Parent Centers promotes children's educational and social success. While the CPC program is not perfect, research shows that benefits persist for up to two decades after the end of participation.

Today there is unprecedented evidence that early childhood intervention can enhance children's success in many domains of functioning over long periods of time. Although critical analysis of the findings of intervention studies are necessary, this should not be used to hinder progress on the most critical issues. The most important task for the future is to utilize evidence from the Child-Parent Centers and other programs to improve the quality of existing programs and to expand access to underserved families.

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