We evaluated the effects of participation in an extended program of compensatory education for 559 low-income, inner-city African American children up to seventh grade. The intervention is the federal and state-funded Chicago Child-Parent Center and Expansion Program, which began in 1967. Groups included 426 children who participated in the program from preschool to grades 2 or 3 and 133 children whose participation ceased in kindergarten. After taking into account initial differences in achievement at kindergarten entry and at the end of kindergarten, and after taking into account sample selection bias, program participation for 2 or 3 years after preschool and kindergarten was associated with significantly higher reading achievement up to seventh grade and with lower rates of cumulative grade retention and special education placement (4 to 5 years postprogram). Children participating in the follow-on program for 3 years had significantly higher reading achievement in seventh grade and a lower rate of grade retention than 3 year participants. Only 3 year participants had significantly higher math achievement than the comparison group. Study findings provide rare longitudinal evidence of the beneficial effects of a large-scale community-based program of extended early childhood intervention.

INTRODUCTION

Although 3 decades of research have indicated that well-implemented early childhood interventions improve children's cognitive development and school achievement in the short term and often enhance school competencies over the longer term (Barnett, 1995; Consortium for Longitudinal Studies, 1983; Haskins, 1989; McKey et al., 1985), it increasingly is recognized that a 1 or 2 year program cannot immunize children over the long term against poor academic outcomes. This especially is true for large-scale, government-initiated programs like Head Start. Unlike researcher-initiated model programs implemented as research demonstrations, large-scale interventions are public service programs and are implemented with limited human and financial resources. Commitment to research and evaluation often has been relatively low for large-scale programs (Zigler & Muenchow, 1992). Consequently, the evidence for the longer-term effects of large-scale programs like Head Start is relatively weak compared to that of model programs (Barnett, 1992, 1995; Haskins, 1989; Yoshikawa, 1995). Although studies of model programs suggest how effective early intervention can be, policy makers are most interested in knowing how effective large-scale, public service programs are.

To promote better school success and to prevent the erosion of preschool effects, educators and researchers increasingly are recommending that early-childhood interventions last longer, preferably from preschool to third grade (National Head Start Association, 1990; Zigler & Styfco, 1993). Duration of program participation is an important characteristic of effective early childhood programs (Ramey & Ramey, 1992; Wachs & Gruen, 1982). In recognition of the possible benefits of extended childhood interventions, the U.S. Department of Health and Human Services currently funds the Head Start–Public School Transition Demonstration Project (Zigler & Styfco, 1993). Although it is still too early to ascertain the longer-term effects of this initiative, the present study provides important evidence of the effects of an extended educational intervention offered through the Chicago Child-Parent Center and Expansion Program, one of the oldest federally initiated early childhood interventions in the United States. With the recent termination of Project Follow Through, the Chicago Child-Parent Center is now the longest running large-scale extended early childhood program in the United States.

Importance of Extended Childhood Intervention

Better performance of children who participate in extended early childhood interventions from preschool to the primary grades can be expected for three major reasons. First, longer duration of implementation may be necessary to promote greater and
longer-lasting changes in academic and social outcomes. Many economically disadvantaged children need more time to gain all the benefits the program has to provide. Early-childhood interventions provide many services to children and parents (e.g., health, educational, parent involvement activities) that require significant coordination and effort. Moreover, because the rate and negative consequences of poverty among young children are growing (Hernandez, 1994), existing programs may require expansion to be effective.

Second, developmental research overwhelmingly indicates that an important ingredient of normal cognitive and social functioning is the experience of a stable and predictable learning environment (Cole & Cole, 1993; Garmezy & Rutter, 1988). By giving children and their parents the opportunity to enroll in programs for up to 5 or 6 continuous years, the stability of the school learning environment may enhance school performance and social competence. Certainly, environmental forces continue to operate after the preschool and kindergarten years, and continuing intervention may enhance developmental functioning. One assumption of extended early childhood programs is that the postprogram learning environment after preschool is an important ingredient of long-term program effectiveness.

A third justification for extended childhood interventions is that the transition to formal schooling in kindergarten and first grade is a sensitive if not “critical” period in children’s scholastic development (Alexander & Entwisle, 1988; Entwisle & Alexander, 1993). It is expected that the provision of additional educational and social support services to children and families during this key transition would promote better adjustment. Research on early schooling indicates that the initial adjustment to school has significant, substantial effects on later school success (Entwisle & Hayduk, 1988; Reynolds, 1989, 1991). Extended intervention programs are designed to promote better transitions through this important period.

Although the rationale for extending preschool intervention programs into the primary grades is well grounded in theory, little empirical evidence exists of the effectiveness of extended interventions. Although Project Follow Through was originally designed to provide extended-intervention services for Head Start graduates, it was implemented as planned variations of different instructional models for children regardless of their participation in preschool intervention (Zigler & Styfco, 1993). Although some early studies of children attending Head Start and Follow Through (Abelson, Zigler, & DeBlasi, 1974; Seitz, Apfel, Rosenbaum, & Zigler, 1983) and similar extended intervention programs (Fuerst & Fuerst, 1993; Jordon, Grallo, Deutsch, & Deutsch, 1985) showed that children who participated in extended intervention had higher subsequent school achievement than children who did not participate, most studies have not distinguished between the effects of duration and timing. Important exceptions are recent studies of the Carolina Abecedarian Project in which the effects of preschool and follow-on programs were separated through random assignment to treatment groups. Campbell and Ramey (1994, 1995) traced 93 children who participated to varying degrees in the model intervention in Chapel Hill, NC, from birth to age 8. They found that participation in the 5 year preschool program was positively associated with higher cognitive ability and school achievement as well as lower grade retention and special education placement up to age 15. The 3 year school-age program, largely consisting of services provided by a home/school resource teacher, was found to have limited independent effect and was associated only with reading achievement at age 15.

Previous Findings from the Child Parent Center Program

Investigation of the Chicago Child-Parent Center and Expansion (CPC) Program has been carried out prospectively in the Chicago Longitudinal Study (Reynolds, 1994, 1995; Reynolds & Bezruczko, 1993). Children began their participation in preschool and could continue to receive services through second or third grade. The CPC program provides centered-based child-development and family-support services under the direction of a curriculum coordinator. Reynolds (1994) investigated the relation between the duration of participation in the Child-Parent Center program and child outcomes at the end of the program (grade 3) and at the 2 year follow-up assessment (grade 5) for 1,106 African American children. Longer duration of participation was significantly associated with higher reading and mathematics achievement and lower grade retention controlling for the influence of demographic factors (i.e., sex, school SES in kindergarten, parent education, eligibility for free lunch, and age). Analysis of seven intervention and comparison groups differentially exposed to the intervention revealed that participation in the follow-on intervention for 2 or 3 years significantly contributed to children’s adjustment (i.e., improved achievement, lowered incidence of grade retention) above and beyond preschool/kindergarten intervention after controlling for kindergarten...
Methodological Issues in Evaluating Longer-Term Effects

A major issue in evaluation research is the extent to which observed differences in performance outcomes between groups are due to program participation rather than to individual student attributes correlated with the decision to enroll in the program. Short of randomization, one approach to minimizing bias is to remove or reduce bias in the research design phase prior to data analysis. This strategy may include matching individuals or groups on key variables, pretesting groups, obtaining multiple comparison groups, and implementing prospective time series designs. This is the long-standing practice in psychological and educational research (Cook & Campbell, 1979; Cook & Shadish, 1994).

Alternatively, attempts to obtain estimates of program impact may be undertaken in the statistical analysis phase of the research. By measuring and incorporating explanatory factors that correlate with both program participation and outcomes, program effects can be estimated and potential unmeasurable factors taken into account. This is a prevalent approach in economics and is implemented through multiple equation modeling techniques such as two-stage nonlinear least squares and the Heckman sample selection method (e.g., Barnow & Cain, 1977; Heckman, 1979). In view of the lack of consensus about the best approach to measuring impact in quasi-experiments (Cook & Shadish, 1994; Winship & Mare, 1992), the current study explicitly attempts to control for potential selection bias through a combination of research design and statistical analysis strategies. As in previous reports from this study (Reynolds, Mehana, & Temple, 1995; Reynolds & Temple, 1995) and others (Lipsey & Wilson, 1993), quasi-experimental studies can lead to conclusions similar to those obtained by experimental studies. Moreover, large-scale quasi-experimental studies generally have more statistical power and produce results that are more generalizable than small-scale randomized experiments.

The second methodological problem in longitudinal studies is the treatment of missing observations, especially those observations that are missing due to the attrition of individuals in the sometimes lengthy time period covered by the study. A number of students who participated in the Child-Parent Centers at least through preschool and kindergarten are missing test scores in the follow-up years. The potential biases caused by nonrandom attrition in estimating the effects of educational interventions have been discussed by Becker and Walstad (1990). The current study employs a method for dealing with attrition that is prevalent in the economics literature to control for observed and unobserved differences between the students who remain in the sample and those who do not.

The Present Study

In the present study, we investigate the effects of extended participation in the Child Parent Center program on children’s school achievement up to seventh grade, 4 to 5 years after the end of the follow-on program component. The analysis will focus on four widely used measures of social competence: reading achievement, mathematics achievement, grade retention, and special education placement.

In a significant extension of previous efforts to ascertain program effectiveness, the current study includes in the analysis only those children who entered the Child-Parent Centers (CPC) in preschool and graduated from the kindergarten program in 1986. Given that all the sample children enrolled in the preschool and kindergarten component of the CPC program, we seek to determine the effects of continued enrollment through grades 2 or 3. All children in the sample obviously were eligible for intervention, resided in low-SES school neighborhoods, and participated in the program in preschool and kindergarten for at least 2 or 3 years. Both the extended intervention group and the comparison group consisted of children with similar backgrounds and similar low numbers of school moves through third grade. High numbers of school moves have been found in recent studies (General Accounting Office, 1994; Temple & Reynolds, 1997; Wood et
al., 1993) to be associated with and perhaps even cause lower academic achievement.

The main research question is: Does participation in extended childhood intervention from preschool to second or third grade enhance school adjustment at age 13 above and beyond participation in intervention offered in preschool and kindergarten only?

**METHOD**

**Design and Sample**

The Chicago Longitudinal Study (Reynolds & Bezruczek, 1993; Reynolds, Bezruczek, Mavrogenes, & Hagemann, 1996) traces the school performance of a large sample of minority children (95% African American, 5% Hispanic) who graduated from government-funded kindergarten programs in 1986 in the Chicago Public Schools. The original sample consists of 989 children who participated in all 20 sites of the Child-Parent Centers in preschool and kindergarten. The Chicago Longitudinal Study also tracks the school performance of children who were enrolled in non-CPC government-funded programs in kindergarten (also see Reynolds, 1994). The original and study samples are listed in the Appendix.

The study sample included 559 African American children who participated in the Child-Parent Center programs in preschool and kindergarten and who were active in the school system in grades 3 (1988–1989) and 7 (1992–1993). Of these, 426 children participated in the preschool and kindergarten program plus the follow-on program for 2 (n = 281) or 3 years (n = 155), and 133 children participated in only the preschool and kindergarten components of the CPC program. The study sample comprises 70% (426/551) of the 2 to 3 year follow-on group and 45% (133/309) of the non-follow-on group (prior to exclusion criteria). To enhance group comparability, frequent school movers from kindergarten through third grade were excluded from the comparison group. Comparison-group children necessarily moved once when they left the CPC program after kindergarten or first grade. To obtain a conceptually distinct comparison group, children who participated in the follow-on program for 1 year also were excluded. However, the characteristics of frequent movers and children with 1 year of program participation were taken into account in estimation.

Sample children are among the most disadvantaged in the Chicago school system. In their kindergarten year, the children attended CPCs located in schools in which 66% of the families were low income, compared to 42% for all elementary schools in Chicago. The schools attended by the children after the end of the follow-on program in grade 7 also appear to be substantially more disadvantaged than the typical school in Chicago. In the schools attended by the study sample in 1992, 84% of families were low income compared to 65% for other city schools. In 1992, the study children attended schools in which the typical racial composition was 73% African American, 18% Hispanic, and 8% European American compared with 50%, 29%, and 16%, respectively, for other Chicago K–8 schools.

**Program selection.** Entry into CPCs requires residency in school neighborhoods eligible for ESEA (Elementary and Secondary Education Act) Title I services, and applicants are accepted on a most-in-need basis. All children were eligible for intervention services due to economic and educational disadvantage. The existence of excess demand for the CPC program means that many eligible students were turned away. Parents voluntarily enrolled their child in the program, for which parental involvement in kindergarten and preschool was required and was strongly encouraged in the follow-on program. Variation in exposure to intervention is due to a variety of reasons, including family preferences, administrative differences across schools, and student or family mobility. School personnel report that some families enter their children in the CPC preschool and kindergarten with the expectation that they will leave the program after kindergarten to enroll in a more conveniently located grade school. Most of the students participated in the CPCs for the complete number of years that the services were offered to them. For administrative reasons unrelated to individual child attributes, 14 of the schools in the sample offered the follow-on program through grade 2, while six schools offered the program through grade 3. Hence the possibility that unmeasured student attributes are responsible for the observed differences in outcomes between those students with 2 versus 3 years of follow-on experience is unlikely.

**Child-Parent Center and Expansion Program**

In 1967, the federal government provided Title I funds for the establishment of four Child-Parent Center (CPC) education programs in the Chicago Public Schools for economically and educationally disadvantaged children. At present, 24 centers throughout the city are funded through federal block grants and the state of Illinois. In 20 of the 24 sites, the program provides for half-day preschool, half-day or all-day kindergarten, and all-day service in the follow-on program in the primary grades (grades 1–3). 

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like most other early childhood programs, the CPC program provides up to 6 years of continuous intervention. It emphasizes three major features: the provision of comprehensive services, parental involvement in school, and a child-centered focus on reading/literacy skills (for implementation history, see Chicago Public Schools, 1985, 1987; Conrad & Eash, 1983; and Reynolds, 1994).

The comprehensive services include (1) attending to children's nutritional and health needs (i.e., free breakfasts and lunches and health screening); (2) coordinated adult supervision from a CPC head teacher, parent resource teacher, and school-community representative, as well as a teacher aide for each class; (3) funds for in-service teacher training in child development as well as additional instructional supplies; and (4) emphasis on reading, mathematics, and use of language. The parent resource teacher organizes the parent-resource room, which is designed to initiate education activities for parents as well as to foster parent-child interactions. The school-community representative monitors parental as well as child school involvement and, if necessary, visits families in their home.

A main philosophy of the program is that parent involvement is a critical socializing force in children's development. At least one-half day per week of parent involvement in the center is required during preschool and kindergarten, while parental involvement in the follow-on program is strongly encouraged. Parent involvement includes volunteering as classroom aides to tutor children, accompanying classes on field trips, interacting with other parents in the center's parent resource room, participating in reading groups with other parents, attending school meetings and programs, doing craft projects for use in the school or at home, and taking trips to the library with teachers and/or their children. Schools also frequently sponsored night courses for parents to obtain additional education, including their high school certificates. In the primary grades, the head teacher and parent resource teacher are combined into one position (the curriculum parent resource teacher). The parent activities, however, remained the same. Mean teacher ratings (1 = poor/not at all to 5 = excellent/much) on parental participation in school during grades 1 to 3 of the follow-on program were, respectively, 3.1, 3.0, and 2.9, compared to 2.4, 2.4, and 2.3 for the non-follow-on comparison group (all significant at the .05 level).

The smaller class sizes and greater number of adult staff allow more individualized and child-centered attention to develop reading comprehension and writing skills. Children learn to read and write through a broad spectrum of experiences, such as small group activities, shared reading, and journal writing. Moreover, classes go on field trips to places such as the Museum of Science and Industry and the zoo. Regularly scheduled in-service teacher training sessions conducted by the Bureau of Early Childhood reinforce the emphasis on child-centered activities.

Average class size for the preschool component was 17, with an adult-to-child ratio of 1 to 8. For the kindergarten and primary-grade components, average class sizes are 25, with adult-to-child ratios of 1 to 12.

In the CPC primary-grade follow-on program, each curriculum-parent-resource teacher was given funds to purchase instructional materials and supplies for each room (e.g., basal reading programs and equipment), conduct an ongoing parent program in the elementary school (e.g., implement activities in the parent room, coordinate parent volunteers), and organize teacher in-services as well as field trips. These support services contrast significantly with the typical programs provided to children in the comparison group. For example, class sizes in the regular school programs were usually 30 or more, and teacher aides were rarely available. No extra funds were available to staff a noninstructional program coordinator, to implement a parent program in designated parent resource rooms, to conduct teacher in-services, or to purchase additional instructional materials. Nevertheless, the instructional objectives were similar between program and comparison groups in that language arts (especially reading comprehension) were emphasized, as well as mathematics, social studies, science, art, and music as specified in the Chicago comprehensive reading program guide. Overall, the CPC primary grade program provides a variety of educational resources to help children make a successful transition from kindergarten (see Conrad & Eash, 1983; Reynolds, 1994).

Outcome Measures

Reading and mathematics achievement (grades 3 and 7). Reading comprehension and mathematics total subtest scores on the Iowa Tests of Basic Skills (ITBS; Hieronymus & Hoover, 1990; Hieronymus, Lindquist, & Hoover, 1980) were used as measures of school achievement. School achievement is widely considered to be a major objective of early childhood programs. The ITBS is among the most reliable and valid of all standardized achievement tests (Linn, 1989). In grade 3, the reading comprehension subtest includes 47 items (KR-20 reliability = .92), and the mathematics total test (the sum of concepts, compu-
tation, and problem solving) includes 101 items (KR-20 reliability = .95). These scores were based on the 1978 norm-referenced test (Form 7 Level 9) but were converted to 1988 norms for the analysis. In grade 7, the reading comprehension subtest has 54 items (KR-20 reliability = .94), and the mathematics total subtest has 101 items (KR-20 reliability = .95). Scores were based on 1988 norms.

The ITBS is group administered each year in April by the Chicago Public Schools. Scores were reported in ITBS developmental standard scores. As standard scores, they can be used to determine cognitive growth from year to year as well as to assess differential group performance across test levels and grades. Like grade equivalents, individual and average scores increase by grade. Scores range from 18 to 260. The national average in third grade for reading and math is 108 and 108, respectively, and in seventh grade, 155 and 156, respectively.

Tests were administered under standardized procedures by school personnel other than the classroom teacher. Staff from the central offices of the school district as well as school counselors and other teachers conducted testing under the supervision of a school testing coordinator. Consequently, test administration was independent of the intervention experiences received.

Grade retention. Obtained from school records, grade retention was defined as a cumulative and dichotomous measure. Children were coded 1 if they were ever retained in grade from kindergarten to grade 7 or if they were not at the grade level of their same-age peers (grade 8) in September 1993. Children were coded 0 if they were on grade level (grade 8) at the beginning of the 1993–1994 year.

Special education placement. Like grade retention, this cumulative measure is coded as a dichotomous variable equal to 1 if students had ever been placed in special education classes from grades 1 to 7 (self-contained or mainstreamed); all others were coded 0. Data came from centralized school records. The major categories of assignment included specific learning disability, speech and language disorders, and behavioral disorder.

Explanatory Measures

Table 1 provides descriptive data on family, child, and school variables used as explanatory variables or covariates in the analysis of program effects. These included student gender, family SES (based on eligibility for a lunch subsidy, where family SES = 1 for full subsidy and 0 for a partial subsidy or no subsidy), and parent education (coded 1 for at least high school graduate and 0 otherwise). These indicators were obtained from parent surveys and telephone interviews in grades 2 and 4. Because roughly one-fourth of the CPC students had parents who did not provide information to the CPC program on family SES or parent education, a dummy variable also was constructed to represent whether the data on family SES and parent education were missing. If information on those two variables was missing, then the missing values were filled with the lowest value (the mode) for the available data, and the dummy variable for missing information was set equal to 1 to control for differences between students who had complete data in their records and those who did not.

Three measures of beginning school achievement (in ITBS standard scores) included cognitive readiness at kindergarten entry (a composite of language, word analysis, listening, and mathematics subtests) and word analysis (reading readiness) and mathematics achievement at the end of kindergarten (ITBS Level 5 Battery; Hieronymus et al., 1980). Finally, to control for school-level differences across CPC sites,

<table>
<thead>
<tr>
<th>Variable</th>
<th>Follow-on Group</th>
<th>Comparison Group</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female</td>
<td>.53</td>
<td>.56</td>
</tr>
<tr>
<td>Family low income</td>
<td>.82</td>
<td>.85</td>
</tr>
<tr>
<td>Parent education</td>
<td>.62</td>
<td>.55</td>
</tr>
<tr>
<td>Missing information</td>
<td>.26</td>
<td>.23</td>
</tr>
<tr>
<td>School poverty rate</td>
<td>66.38*</td>
<td>68.54</td>
</tr>
<tr>
<td>Mathematics achievement at</td>
<td></td>
<td></td>
</tr>
<tr>
<td>end of kindergarten</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unadjusted/adjusted group difference</td>
<td>5.2/2.1</td>
<td></td>
</tr>
<tr>
<td>Reading achievement at end of</td>
<td></td>
<td></td>
</tr>
<tr>
<td>kindergarten</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unadjusted/adjusted group difference</td>
<td>4.0/1.2</td>
<td></td>
</tr>
<tr>
<td>Cognitive readiness at kindergarten</td>
<td></td>
<td></td>
</tr>
<tr>
<td>entry</td>
<td>42.55*</td>
<td>39.34</td>
</tr>
<tr>
<td>Achievement growth in kindergarten</td>
<td></td>
<td></td>
</tr>
<tr>
<td>entry</td>
<td>19.56</td>
<td>18.18</td>
</tr>
</tbody>
</table>

Note: Follow-on group includes those students who participated in the full program through second or third grade. Follow-on comparison group includes students who participated in the Child-Parent Center Program during preschool and kindergarten only. Values for parent education and family low income are shown before imputation for missing data. Achievement growth in kindergarten is equal to mean of reading and math achievement at the end of kindergarten minus cognitive readiness at kindergarten entry.

* Indicates that differences between groups are significant at the .05 level.
a variable representing the school poverty rate in the students’ CPC school was incorporated into the analysis as a possible factor influencing student achievement.

Group Comparisons

A frequent problem in estimating the effects of educational treatments with nonrandomized or quasi-experimental data is the difficulty of controlling for differences between groups in characteristics that may be correlated with both program enrollment and achievement. Studies of the effectiveness of compensatory education programs often attempt to control for existing socioeconomic differences between participants and nonparticipants without having available any data on pretreatment achievement (Barnow & Cain, 1977). The importance of having multiple pretreatment observations on outcomes recently has been emphasized by Cook and Shadish (1994) and Moffitt (1991). The existence of achievement test scores measured both at the beginning and the end of kindergarten allows us to control for the initial level of achievement as well as investigate achievement growth. A comparison of achievement growth over the kindergarten year reveals no significant differences in test score changes for the two groups. Groups also were comparable on parent education, family low income, missing data, and gender.1

Table 1 reveals evidence of selection bias: Students who enrolled in the follow-on program through second or third grade tested significantly higher in both reading and math at the end of kindergarten than did comparison students. Importantly, however, the adjusted mean test scores are not statistically different from each other. An analysis of the determinants of this difference reveals that most, if not all, of the between-group difference in test scores can be explained by differences in covariates, including cognitive readiness at kindergarten entry, parent education, and family low income. In the case of reading, the adjusted mean difference between the follow-on and the comparison group after controlling for these variables is only 1.2 points instead of 4.0. The adjusted mean difference for the math test taken at the end of kindergarten is 2.1, whereas the unadjusted mean difference is 5.2.

1. Although not shown, groups were similar in family structure, as about 75% of children in the intervention and comparison groups lived in single-parent families. Groups also were similar in the number of siblings and age at kindergarten entry. Previous studies using this data set have shown that neither the family structure variable nor the siblings variable is correlated with student achievement.

Analytic Strategy

The estimates reported in this study correct for a censoring or sample selection problem that is common in longitudinal analyses when there is attrition from the original sample. To be included in this longitudinal study, individuals must have valid test scores in grades 3 and 7, be relatively school-stable, and have completed either 0 years of the follow-on program or 2 or 3 years. The possibility of nonrandom attrition is addressed by using a two-equation, simultaneous estimation method that takes into account both measurable and unmeasurable differences between individuals who remain in the sample through seventh grade versus those who do not. Correcting for sample selection is important because if an unobserved variable is correlated with both the probability of being in the sample and the academic outcomes of achievement and/or retention, then ordinary least-squares regression could generate biased estimates of the effects of the follow-on program. The sample selection correction method used here is a maximum likelihood version of Heckman’s (1979) correction technique as implemented through the software program LIMDEP (Greene, 1995).

The equations used to estimated the effects of the follow-on program on reading and mathematics achievement in grades 3 and 7, grade retention, and special education placement include a value-added specification in which a student’s achievement on a previous test is used as an explanatory variable:

\[ A = a_1 + a_2F + a_3X + a_4A_k + e, \]  

where \( A \) includes the outcomes of mathematics and reading achievement in grades 3 and 7, as well as measures of grade retention and special education placement as of grade 7. There are six of these equations—one for each outcome. The program variable \( F \) represents participation in the follow-on program (where \( F \) is a dummy variable equal to either 0 or 1); \( X \) is a vector of individuals, family, and school-level variables hypothesized to influence student achievement; and \( A_k \) is a measure of achievement at the end of kindergarten. Included in \( X \) is a variable \( A_{K-1} \), which is a measure of cognitive readiness at kindergarten entry.

As a result of the sample censoring for missing test scores, frequent mobility, and incomplete participation in the program, a larger sample of 883 African American children is reduced to 559. To control for the possible biases that the nonrandom sample selection might otherwise introduce, the equation above for each of the six outcomes is estimated simultaneously with another equation for sample selection.
In addressing sample selection, we employ a model for each outcome in which a bivariate classical regression applies to the equation above along with another equation shown below as equation (2).

\[ Z^* = aV + u. \]  

Equation (2) is the sample selection equation. This equation is estimated using the larger sample of 883 students. The error terms \( e \) and \( u \) in the two equations (1) and (2) are assumed to have a bivariate normal distribution, and the correlation between \( e \) and \( u \) is allowed to be nonzero. The latent variable \( Z^* \) represents the individual student’s probability of being included in the sample that is used to estimate equation (1). We do not observe \( Z^* \), but we do observe whether a student is in the sample. Hence \( Z^* \)'s observed counterpart \( Z \) is determined by: 
\[ z = 1 \text{ if } Z^* > \frac{1}{2}, \quad \text{and} \quad z = 0 \text{ if } Z^* \leq \frac{1}{2}. \]

As a result, equation (2) can be estimated as a probit equation in which the observed dependent variable is equal to 1 if the student will be included in the outcome regressions, and the dependent variable is equal to 0 if the student will not be included. Although it is not shown in the Results section, the probit equations for sample retention include all of the covariates included in the estimation of equation (1) as well as additional site variables that we use to identify the model.

The joint estimation of each outcome equation along with a sample selection equation is very important for obtaining unbiased estimates of the effects of the follow-on program. In the Results section, the estimated correlation between the error terms of the two equations will be reported and discussed. The sign and statistical significance of the correlation between \( e \) and \( u \) indicates whether unobservables affecting the individual student’s remaining in the sample are related to the student’s school performance. For example, a positive correlation would be found if poorer-performing students are more likely to be absent on test-taking days or if better students are more likely to remain in the sample because they are unlikely to change schools frequently. Ignoring the correlation between the sample selection decision and the school performance outcomes would generate biased estimates of the effects of the follow-on program as well as biased estimates of the effects of the other included variables in the outcome equations.

RESULTS

Table 2 shows the unadjusted group means for the achievement outcomes measured in grades 3 and 7. Students who participated in the full amount of the compensatory program through second or third grade, on average, outperformed children whose participation in the program ended after kindergarten. Also displayed are the entire Chicago public school and national averages for the ITBS taken by third- and seventh-grade students. Although students in the study come from the poorest neighborhoods in Chicago, the follow-on group performs approximately as well as the average Chicago student in both reading and math in grades 3 and just below the city-wide average in grade 7. These differences are probably conservative, because of the inclusion in the present study of children who were retained in grade. Participation in the CPC program from preschool through kindergarten or first grade allows the comparison group to test above the Chicago average only in reading in grade 3.

In Table 3, we provide estimates of equation (1) for each of the six school performance outcomes. Each outcome equation is estimated jointly with a probit equation for sample selection. This method of estimation generates consistent and efficient estimates of program effects in the presence of nonrandom selection into (or out of) the sample. The bivariate maximum likelihood estimation method allows us to control for unobservable characteristics that influence both the decision to remain in the sample and the student’s school performance. In Table 3, participation in the follow-on program after kindergarten and preschool is represented by a dummy variable equal to 1 if the student participated in the follow-on program for 2 or 3 years and 0 if the student did not

2. In practice, it is important to include in the sample selection (attrition) equation all exogenous variables from the outcome equations plus one or more other variables that help predict sample selection but are uncorrelated with the outcomes. Variables included in the sample selection equation that help predict whether or not a student remains in the sample include math achievement at the end of kindergarten (often negative), participation in the follow-on program for at least 2 years (positive, as expected), the missing data dummy variable (negative, as expected), as well as a handful of significant site variables. A good exposition of the importance in evaluating educational programs of controlling for sample selection due to data loss is found in Becker and Walstad (1990). For further description of the sample selection methods, see Greene (1993, 1995).

3. The numbers in parentheses in Table 2 are the grade equivalents associated with each standardized score. For example, the national average for reading comprehension in grade 3 is 3.8, meaning that the average students test at the level of the eighth month of the third year in school. (The tests are taken in April, which is the eighth month of the school year.) In Chicago, however, the typical student scores almost a complete year behind (scoring at the level of the ninth month of the second year).
Table 2  Descriptive Statistics for ITBS Scores, Grade Retention, and Special Education Placement

<table>
<thead>
<tr>
<th>Intervention Group</th>
<th>Grade 3 Achievement (Age 9)</th>
<th>Grade 7 Achievement (Age 13)</th>
<th>Percent Ever Retained in Grade</th>
<th>Percent Ever Placed in Special Ed</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Reading</td>
<td>Math</td>
<td>Reading</td>
<td>Math</td>
</tr>
<tr>
<td>Follow-on intervention group</td>
<td>101.0 (3.3)</td>
<td>103.9 (3.5)</td>
<td>142.7 (6.7)</td>
<td>143.6 (6.8)</td>
</tr>
<tr>
<td>Comparison intervention group</td>
<td>93.7 (2.8)</td>
<td>98.6 (3.1)</td>
<td>136.9 (6.3)</td>
<td>137.8 (6.2)</td>
</tr>
<tr>
<td>Chicago average</td>
<td>95.0 (2.9)</td>
<td>100.0 (3.2)</td>
<td>146.0 (7.0)</td>
<td>145.0 (6.9)</td>
</tr>
<tr>
<td>National average</td>
<td>108.0 (3.8)</td>
<td>108.0 (3.8)</td>
<td>155.0 (7.8)</td>
<td>156.0 (7.8)</td>
</tr>
</tbody>
</table>

Note: Values in parentheses are grade equivalents. Ns for extended and comparison intervention groups are, respectively, 426 and 133. ITBS = Iowa Tests of Basic Skills (1988 norms). Standard deviations of standard scores for the extended intervention group are, respectively, 15.8, 12.4, 19.8, and 17.8.

Table 3  Estimation of the Effects of the Follow-on Program on Achievement in Grades 3 and 7, Grade Retention, and Special Education Placement

<table>
<thead>
<tr>
<th>Participation in follow-on program (through second or third grade)</th>
<th>Math3</th>
<th>Reading3</th>
<th>Math7</th>
<th>Reading7</th>
<th>Grade Retention</th>
<th>Special Education</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participation in follow-on program (through second or third grade)</td>
<td>4.49 (2.11)*</td>
<td>7.84 (3.22)*</td>
<td>4.86 (1.54)</td>
<td>8.59 (2.90)*</td>
<td>-.148 (5.07)*</td>
<td>-.057 (2.17)*</td>
</tr>
<tr>
<td>Female</td>
<td>1.46 (1.59)</td>
<td>4.52 (3.86)*</td>
<td>3.49 (2.76)*</td>
<td>5.27 (3.53)*</td>
<td>-.122 (3.22)*</td>
<td>-.042 (1.56)</td>
</tr>
<tr>
<td>Family low income</td>
<td>-.73 (4.6)</td>
<td>-.65 (3.1)</td>
<td>-.236 (1.20)</td>
<td>-.276 (1.08)</td>
<td>(-.255 (2.55)*</td>
<td>(.64)</td>
</tr>
<tr>
<td>Parent education</td>
<td>3.82 (3.76)*</td>
<td>3.52 (2.61)*</td>
<td>4.74 (3.41)*</td>
<td>4.33 (2.52)*</td>
<td>-.050 (1.42)</td>
<td>.028 (1.92)</td>
</tr>
<tr>
<td>Missing information</td>
<td>-.128 (1.83)</td>
<td>-.225 (1.17)</td>
<td>-.515 (2.54)*</td>
<td>-.849 (3.57)*</td>
<td>.041 (1.89)</td>
<td>.108 (3.53)</td>
</tr>
<tr>
<td>School poverty rate</td>
<td>.02 (.35)</td>
<td>.02 (.02)</td>
<td>.01 (.19)</td>
<td>.04 (.45)</td>
<td>.001 (.24)</td>
<td>.001 (.58)</td>
</tr>
<tr>
<td>Mathematics achievement at end of kindergarten</td>
<td>.19 (4.53)*</td>
<td>.15 (2.73)*</td>
<td>.22 (4.14)*</td>
<td>.21 (3.06)*</td>
<td>-.003 (1.25)</td>
<td>-.004 (2.36)</td>
</tr>
<tr>
<td>Reading achievement at end of kindergarten</td>
<td>.24 (.544)</td>
<td>.34 (.556)*</td>
<td>.25 (.426)</td>
<td>.30 (.387)*</td>
<td>.003 (.209)</td>
<td>.000 (.12)</td>
</tr>
<tr>
<td>Achievement at kindergarten entry</td>
<td>.25 (4.36)*</td>
<td>.32 (4.37)*</td>
<td>.40 (5.62)*</td>
<td>.43 (4.92)*</td>
<td>.003 (2.19)</td>
<td>.001 (4.2)</td>
</tr>
<tr>
<td>Constant</td>
<td>58.47 (11.02)</td>
<td>44.74 (6.75)*</td>
<td>89.51 (14.03)*</td>
<td>78.60 (9.86)*</td>
<td>-.003 (3.08)</td>
<td>.108 (1.25)</td>
</tr>
<tr>
<td>Correlation (r,u)</td>
<td>.28 (1.16)</td>
<td>.40 (2.06)*</td>
<td>.26 (.99)</td>
<td>.54 (3.12)*</td>
<td>-.72 (3.46)</td>
<td>-.59 (2.12)</td>
</tr>
</tbody>
</table>

Note: N = 559 for each equation. Absolute values of t statistics are in parentheses. Participation in the follow-on program is coded 1 for students who participated in the program through second or third grade, 0 for students who participated through kindergarten only. Estimates shown here are obtained from jointly estimating an equation for sample retention along with each outcome equation using maximum likelihood estimation to control for nonrandom attrition from the larger sample of 883. Estimated coefficients are interpretable as marginal effects. Probit coefficients for grade retention and special education are available from the authors. *p < .10; **p < .05.
participate in the follow-on program. In estimating the effects of the follow-on program, we control for kindergarten achievement right before the follow-on program commences and at kindergarten entry as well as for parent education, family low income, missing data, gender, and school poverty.

Grade 3 School Achievement

Enrollment in extended intervention (preschool to grades 2 or 3) was associated with significantly higher test scores in reading and math in grade 3 above and beyond the child and family covariates. As shown in Table 3, children who participated in the follow-on program for either 2 or 3 years scored 4.5 points higher \( (t = 2.12, p = .034) \) in math achievement and 7.8 points higher \( (t = 3.22, p = .001) \) in reading achievement than children who left the CPC program after kindergarten. These findings are consistent with those of Reynolds (1994), and are not surprising given the commonly found short-term effects of compensatory education programs. These group differences correspond to effect sizes (Cohen’s \( d \)) of .48 and .35, respectively, in reading and math. Effect sizes are proportions of standard deviations and were calculated as the metric regression coefficient (adjusted mean difference) divided by the respective total sample standard deviation. Notably, controlling for kindergarten pretest scores in reading and math as well as the measure of cognitive readiness at kindergarten entry is important because these variables are significantly and positively associated with school achievement in grade 3.

In the reading equation, the statistically significant positive correlation between the error terms in the sample retention and reading achievement shows that unobservables influencing a student’s remaining in the sample are positively related to reading achievement. Because comparison group children were more likely to leave the study sample, correcting for attrition bias increased rather than reduced the size of the estimated program effect. In the math equation, however, there is no evidence that any unincluded variables are correlated with both sample selection and achievement.

Grade 7 School Achievement

Table 3 also provides evidence for longer-term effects of the follow-on program at age 13 (grade 7 for most children). Net of other factors, participation in the follow-on program for 2 or 3 years was associated with significantly higher reading achievement in grade 7, but not higher math achievement. Participation in extended intervention was associated with an 8.6 point advantage in reading achievement \( (t = 2.90, p = .004) \) and a 4.9 point advantage in math achievement \( (t = 1.54, p = .124) \). These translate into respective effect sizes of .43 and .28, or about 7 and 4 month gains in achievement. The larger program coefficient for reading is consistent with the focus of the program on literacy and language development. Although the math coefficient was not statistically significant, its effect size of .28 is within the range of being educationally important.

As with earlier achievement, sample selection bias due to attrition significantly influenced reading achievement, but not math achievement. This indicates that unmeasured factors that predict sample selection (recovery) are positively associated with reading achievement \( (\beta = .54, t = 3.12) \), and when taken into account increase the estimated effect of follow-on intervention. For example, the unadjusted mean difference in grade 7 reading favored the follow-on group by 5.8 points, about 3 points lower than the adjusted mean difference that takes sample selection bias and other factors into account.

Besides the three kindergarten achievement pretests, girls scored significantly higher in reading and math than boys, as did children of high school graduates relative to nongraduates. In all grade 7 outcome equations, the missing data dummy variable is an important predictor of school performance.

Grade Retention and Special Education Placement

As with school achievement, children participating in extended early childhood intervention for 2 or 3 years were significantly less likely to be retained by eighth grade and were less likely to receive special education services after taking into account group differences in kindergarten pretest scores, family background, and other factors. The probit regression coefficients in Table 3 indicate that net of all covariates and adjustment for sample selection bias, participation in extended intervention was associated with a 14.8 percentage-point reduction in grade retention (15.3% versus 30.1% for the comparison group) and with a 5.7 percentage-point decline in special education placement (10.0% versus 15.7% for the comparison group). These respective group differences correspond to 49% and 36% reductions in remedial or special services (relative to the comparison group).

These estimated effects also control for unobservable differences between the students who left the sample and those who stayed. The statistical significance of correlation \( (\varepsilon, u) \) for both grade retention and special education placement shows that unobserv-
able factors correlated with sample selection are significantly and negatively associated with these two outcomes—children who left the sample had a higher probability of grade retention and special education placement.

Estimation of the predictors of grade retention and special education placement was conducted recognizing the dichotomous nature of the dependent variable. With dichotomous dependent variables, unbiased estimates of the coefficients and their standard errors can be obtained by estimating a probit equation for grade retention (or special education placement) along with the probit equation for sample selection. This is called a bivariate probit model. A problem with probit equations, however, is that the estimated coefficients cannot be easily interpreted to determine the effect of a change in an independent variable on the dependent variable. In Table 3, the reported t statistics for grade retention and special education are the efficient estimates obtained from the bivariate probit model. For ease in interpretation, however, the bivariate probit coefficients have been replaced by the unbiased estimated coefficients assuming that grade retention and special education placement are continuous variables.

Alternative Estimates of Program Effects

In Table 4 estimates of the effects of extended early childhood intervention are presented for three additional measures of program participation: (1) duration of follow-on participation, (2) 2 years of follow-on participation versus no participation, and (3) 3 years of follow-on participation versus 2 years of participation. The effects of (2) and (3) were estimated in the same equation. The explanatory control variables for the analyses were identical to those reported in Table 3. To save space, the estimated coefficients of the other included explanatory variables are not shown.

Duration of follow-on intervention. Each year of participation in the follow-on program after preschool and kindergarten was associated with a 2.6 and 2.1 point increase in math achievement in grades 3 and 7, although the latter coefficient was only significant at the .10 level. Holding other factors constant, each additional year of the follow-on program was associated with about a 4 point increase in reading achievement in both grades 3 and 7. Increases in the number

of years of follow-on also were associated with a significant reduction in the rate of grade retention (by 7 percentage points), but were unrelated to special education placement ($B = -.015, t = 1.41$). This specification assumes that the effects of additional years of the follow-on program are linear.

Two years versus none. As shown in Table 4 (Row 2), children participating in the CPC follow-on program for 2 years had a 6.5 point higher mean reading achievement score in grade 7 ($ES = .32$), an 11.9 percentage-point lower rate of grade retention (18.2% versus 30.1%), and a 6.4 percentage-point lower rate of special education placement (7.6% versus 14.0%) than the preschool and kindergarten comparison group (adjusted for covariates). The 3.4 point advantage for the 2 year group in grade 7 math achievement was not significantly different from the comparison group ($t = 1.06, p = .29$).

Three years versus 2 years. As shown in Table 4 (Row 3), the estimated effect of 3 years of participation in the follow-on program relative to 2 years was significant at the .05 level for grade 3 reading (8.5 points, $ES = .52$) and math achievement (6.3 points, $ES = .48$), grade 7 reading achievement (5.9 points, $ES = .29$), and for cumulative grade retention (a 55% rate reduction—8.2% from 18.2%; all adjusted for covariates). The estimated effect was significant at the .10 level for grade 7 math achievement (3.2 points, $ES = .18$), but not significant for special education placement. The long-term positive effects of the third year suggest that program participation has a dosage-response effect rather than a recency effect.

It is important to note that the positive effects of the third year are even more likely than other findings to be free of selection bias. Participation in the follow-on program for 3 years versus 2 years is due to administrative requirements not to family self-selection. The follow-on program is offered through second grade in 14 CPC schools and through third grade in six CPC schools. Thus, the decision to enroll for 2 or 3 years is expected to be uncorrelated with child attributes.

DISCUSSION

This study investigated the added effect of participation in an established, large-scale, community-based intervention program after preschool and kindergarten on children’s school achievement and academic progress 4 to 5 years postprogram. Using a restricted comparison-group strategy that took into account sample selection and the availability of multiple measures of preprogram achievement, the results indicate that participation in extended early childhood
Table 4 Alternative Estimates of the Effect of the Follow-on Program

<table>
<thead>
<tr>
<th></th>
<th>Math3</th>
<th>Reading3</th>
<th>Math7</th>
<th>Reading7</th>
<th>Grade Retention</th>
<th>Special Education</th>
</tr>
</thead>
<tbody>
<tr>
<td>Specification I (N = 559):</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of years of participation</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>in the follow-on program (0, 2, or 3)</td>
<td>2.58</td>
<td>4.03</td>
<td>2.06</td>
<td>3.71</td>
<td>-.070</td>
<td>-.015</td>
</tr>
<tr>
<td></td>
<td>(3.56)*</td>
<td>(4.53)*</td>
<td>(1.71)*</td>
<td>(3.38)*</td>
<td>(6.48)*</td>
<td>(1.41)</td>
</tr>
<tr>
<td>Specification II (N = 559):</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Participation in the follow-on program through at least second grade</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1.95</td>
<td>4.15</td>
<td>3.35</td>
<td>6.51</td>
<td>-.119</td>
<td>-.064</td>
</tr>
<tr>
<td></td>
<td>(.97)</td>
<td>(1.75)*</td>
<td>(1.06)</td>
<td>(2.19)*</td>
<td>(4.05)*</td>
<td>(1.95)*</td>
</tr>
<tr>
<td>Participation in the follow-on program through third grade</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>6.31</td>
<td>8.48</td>
<td>3.20</td>
<td>5.87</td>
<td>-.101</td>
<td>.024</td>
</tr>
<tr>
<td></td>
<td>(4.87)*</td>
<td>(4.84)*</td>
<td>(1.80)*</td>
<td>(2.94)*</td>
<td>(2.98)*</td>
<td>(.52)</td>
</tr>
</tbody>
</table>

Note: This table lists the estimated coefficient estimates from maximum likelihood regressions using alternative formulations of the follow-on variable. To conserve space, the other covariates are not shown here. As in Table 3, each equation was estimated jointly with an equation for attrition. The covariates included in these regressions are identical to those included in the equations reported in Table 4. Absolute values of the t statistics are in parentheses.

* p < .10; *p < .05.

...
systems. A third possible source of the effects of extended intervention is parent involvement, a key component of this two-generational program. In the CPC program, follow-on parents were more likely than non-follow-on parents to participate in school activities, such as volunteering in the classroom, interacting with other parents in the staffed parent resource room, and receiving educational training. In previous studies (Reynolds, 1994; Reynolds, Mavrogenes, et al., 1996), parental involvement in school was directly or indirectly associated with greater school achievement and was a mechanism of educational achievement and reduced grade retention. Parent involvement may promote greater sensitivity to children’s scholastic needs as well as positive attitudes toward education leading to higher school achievement.

Our findings indicate that follow-on intervention adds significantly to the effect of preschool and kindergarten intervention. They do not indicate that preschool and kindergarten intervention are ineffective or that follow-on intervention alone is better than earlier intervention. Previous analyses in the Chicago Longitudinal Study (Reynolds, 1995; Reynolds & Temple, 1995) attest to the positive and significant influence of preschool participation on child outcomes. The current results show that enrollment through grades 2 or 3 in the follow-on program, given earlier enrollment in a Head Start-type early intervention in preschool and kindergarten, generates additional positive effects on several indicators of school performance that last through at least grade 7.

Present findings largely support those of the 2 year follow-up (Reynolds, 1994). Two differences occurred at the 4 year follow-up, however. In the 2 year follow-up, both 2 years and 3 years of follow-on participation (after preschool/kindergarten) were associated with higher math achievement, whereas in the 4 year follow-up, only 3 years of follow-on participation were associated with significantly higher math achievement. Perhaps the program emphasis on reading or postprogram instructional factors reduced the math gains, or perhaps the difference is due to the smaller sample used here. The second difference between the two studies was that follow-on participation for 2 or 3 years was associated with reduced special education placement at the present 4 year follow-up, but not at the 2 year follow-up (Reynolds, 1994). This finding supports the importance of continued longitudinal follow-up across several outcomes, even if differences are not observed at an earlier time.

Methodological Issues

A potential limitation of the findings is that assignment to the follow-on program was based on natural variation rather than random assignment. As is the case with all quasi-experiments, it remains possible that unmeasured individual, family, or school-level factors may be responsible for part of the observed statistical relation between program participation and academic outcomes.

Several features of our analytic approach, however, greatly increase the likelihood that these findings are interpretable as program effects. First, to create a comparison group that was most similar to the follow-on group, students who were highly mobile from kindergarten through third grade were excluded from the analysis. Thus, avoidance of frequent school mobility cannot be mistaken for a program effect. Second, the effects of the program were estimated controlling for three measures of kindergarten achievement at two different points in time. We are aware of no other study of early intervention that includes double or triple pretests in estimating program effects. The advantage of two or more pretests at different periods is that preprogram cognitive growth can be estimated to address the selection-maturation hypothesis (Cook & Campbell, 1979; Cook & Shadish, 1994), usually the most serious threat to the validity of quasi-experimental findings. Because the rate of cognitive growth during kindergarten was equivalent between groups, this hypothesis was ruled out. In addition, our two-equation estimation procedure corrected for sample selection bias due to attrition, and this led to a larger effect of extended intervention. Incorporating sample selection into the estimating of program effects also is rare in research on early intervention.

Finally, program and comparison groups were equivalent on several explanatory factors (see Table 1), including end-of-kindergarten achievement scores (adjusted) and family background. As reported at the 2 year follow-up (Reynolds, 1994), significant between-group differences were detected even when motivational factors, such as parent educational attitudes (i.e., parent expectations for children’s educational attainment), were controlled. These methodological strengths, plus the wide generalizability of findings to large-scale programs, greatly enhance the importance of the study.

Program Differences in Context

Although the findings of this study support some previous analyses of extended early childhood inter-
vention (Fuerst & Fuerst, 1993; Jordon et al., 1985; Seitz et al., 1983), they differ in some respects with the follow-up results from the Abecedarian Program. Both the Abecedarian Program (Campbell & Ramey, 1994, 1995) and a previous Child-Parent Center study (Reynolds, 1994) found positive associations between duration of intervention and school performance, and both programs found no significant longer-term effects of school-age intervention alone. The Abecedarian Program, however, found independent effects of extended intervention for reading achievement only. The present study (along with the 1994 study) indicates positive effects for reading achievement and for grade retention and special education placement.

Several explanations can be offered for the different findings between the Abecedarian Project and the Child-Parent Centers and Expansion Program. In addition to the head teacher, called the curriculum-parent-resource teacher (the home/school resource teacher in the Abecedarian Project), the CPC program also provided reduced class sizes and teacher aides for participating students, funds for extra instructional materials, and a staffed parent resource room in the elementary school. Thus, the CPC school-age program provided more comprehensive services. Second, the two programs were implemented at different ages. The Abecedarian Program provides school-age services from kindergarten to second grade; the Child-Parent Center Program provides services from first grade to third grade. Timing of program services could make some difference, especially because in the present study, the 3 year follow-on program group had better school performance than the 2 year group. Third, the effect of school-age intervention may interact with the extent and quality of earlier intervention experiences. In the Abecedarian Project, all children participated for 5 years in a preschool program prior to the school-age program. In the CPC program, children had less extensive center-based experiences prior to the school-age program (2 or 3 years, including kindergarten). These varying experiences could produce differential effects. Finally, the socioeconomic context of the programs was different. The Abecedarian Program was implemented in schools serving mostly middle-income European American children, whereas the CPCs are located in inner-city communities serving mostly poor African American children. Extended early childhood programs in high-poverty schools may have a greater impact than programs implemented in more socioeconomically advantaged contexts. Additional research is warranted to examine the characteristics of different school-age programs, their duration, and the extent to which effects vary by preprogram, contemporaneous, and postprogram characteristics.

Another issue in investigating the effects of early childhood programs is the fundamental difference in the interpretation of the effects of preschool programs versus programs for school-aged children. In the evaluation of preschool programs, treated children often are compared to children with no center-based intervention of any kind. This represents an absolute comparison approach contrasting the effects of a program versus no program. In school-age interventions, the comparison is relative rather than absolute: the target treatment versus another treatment, which is regular school instruction. For school-aged programs, significant differences between program participants and nonparticipants are only observed to the extent that the programs add value above and beyond regular instruction. Thus, the effects of school-age programs may be smaller, not because they are less efficacious, but because the comparison group is receiving an alternative intervention.

**Conclusion**

We suggest that the reason that extended early childhood programs may be more effective than programs of shorter duration is not just because a larger "dose" of compensatory education is more effective than a smaller dose. Extended intervention programs encourage stability in school and home learning environments, and they occur at a very important time in children’s development—the transition to formal schooling. In a time of increased interest in compensatory education programs that begin in infancy, findings that extra investments in programs that extend to the early school-age years also can be effective should not be overlooked.

**ACKNOWLEDGMENTS**

This research was supported by the National Institute of Child Health and Human Development (grant no. HD34294), the University of Wisconsin—Madison Graduate School, and by a faculty fellowship from the NIU Social Science Research Institute. Special thanks go to the Departments of Early Childhood Education and Research, Analysis, and Assessment of the Chicago Public Schools for cooperation in data collection. We are grateful to several anonymous reviewers for their valuable comments. An earlier version of the paper circulated as UW—Madison Institute for Research on Poverty discussion paper no. 1095-96.
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APPENDIX

Composition of Study Sample and Original Sample in the Chicago Longitudinal Study

<table>
<thead>
<tr>
<th>Child Attribute</th>
<th>Study Sample</th>
<th>Original Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>CPC in preschool and kindergarten</td>
<td>989</td>
<td>989</td>
</tr>
<tr>
<td>No CPC participation in preschool and/or</td>
<td></td>
<td></td>
</tr>
<tr>
<td>grades 1 to 3</td>
<td>0</td>
<td>550</td>
</tr>
<tr>
<td>Follow-on participation for 1 year only</td>
<td>(139)</td>
<td>...</td>
</tr>
<tr>
<td>Changed schools one or more times</td>
<td>(107)</td>
<td>...</td>
</tr>
<tr>
<td>Missing test scores in grades 3 or 7</td>
<td>(159)</td>
<td>...</td>
</tr>
<tr>
<td>Hispanic children</td>
<td>(37)</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>559</td>
<td>1,539</td>
</tr>
</tbody>
</table>

Note: Parentheses denote subtractions from the original sample. Numbers in parentheses would be different if the subtractions were made in a different order. For example, the number of Hispanics in the original sample is larger than 37, but some were removed at earlier steps. Follow-on participation includes grades 1 to 3. CPC = Child-Parent Center Program.

REFERENCES


