Head Start Impact Study Technical Report

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Prepared by:

Westat
1600 Research Blvd.
Rockville, MD 20850

Chesapeake Research Associates
708 Riverview Terrace
Annapolis, MD 21401

Abt Associates
4550 Montgomery Avenue
Bethesda, MD 20814

Ronna Cook Associates
5912 Rossmore Drive
Bethesda, MD 20814

The Urban Institute
2100 M Street, N.W.
Washington, DC 20037

American Institutes for Research
1000 Thomas Jefferson Street, N.W.
Washington, DC 20007

Decision Information Resources, Inc.
2600 Southwest Freeway, Suite 900
Houston, TX 77098

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Authors

Prepared by:

Michael Puma
Stephen Bell
Ronna Cook
Camilla Heid

Contributing Authors:

Gary Shapiro
Pam Broene
Frank Jenkins
Philip Fletcher
Liz Quinn
Janet Friedman
Janet Ciarico
Monica Rohacek
Gina Adams
Elizabeth Spier

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Chapter 1: Overview of the Head Start Impact Study

Introduction

Since its beginning in 1965 as a part of the War on Poverty, Head Start's goal has been to boost the school readiness of low-income children. Based on a "whole child" model, the program provides comprehensive services that include preschool education; medical, dental, and mental health care; nutrition services; and efforts to help parents foster their child's development. Head Start services are designed to be responsive to each child's and family's ethnic, cultural, and linguistic heritage.

In the 1998 reauthorization of Head Start, Congress mandated that the US Department of Health and Human Services (DHHS) determine, on a national level, the impact of Head Start on the children it serves. As noted by the Advisory Committee on Head Start Research and Evaluation (1999), this legislative mandate required that the impact study address two main research questions:

- "What difference does Head Start make to key outcomes of development and learning (and in particular, the multiple domains of school readiness) for low-income children? What difference does Head Start make to parental practices that contribute to children's school readiness?"
- "Under what circumstances does Head Start achieve the greatest impact? What works for which children? What Head Start services are most related to impact?"

The *Head Start Impact Study Final Report* (U.S. Department of Health and Human Services, January 2010) addresses these questions by reporting on the impacts of Head Start on children and families during the children's preschool, kindergarten, and 1st grade years. This Technical Report provides detail to support the analysis and findings presented in the Final Report.

Overview of Study Methods

To reliably answer the research questions outlined by Congress, a nationally representative sample of Head Start programs and newly entering 3- and 4-year-old children was selected, and children were randomly assigned either to a Head Start group that had access to Head Start services in the initial year or to a control group that could receive any other non-Head

Start services available in the community, chosen by their parents. In fact, approximately 60 percent of control group parents enrolled their children in some other type of preschool program in the first year. In addition, all children in the 3-year-old cohort could receive Head Start services in the second year. Under this randomized design, a simple comparison of outcomes for the two groups yields an unbiased estimate of the impact of access to Head Start in the initial year on children's school readiness. This research design, when properly implemented, would ensure that the two groups did not differ in any systematic or unmeasured way except through their access to Head Start services. It is important to note that, because the control group in the 3-year-old cohort was given access to Head Start in the second year, the findings for this age group reflect the added benefit of providing access to Head Start at age three, *not* the total benefit of having access to Head Start for two years.

In addition to random assignment, this study is set apart from most program evaluations because it includes a nationally representative sample of programs, making results generalizable to the Head Start program as a whole, not just to the selected samples of programs and children. However, the study does not represent Head Start programs serving special populations, such as tribal Head Start programs, programs serving migrant and seasonal farm workers and their families, or Early Head Start. Further, the study does not represent the 15 percent of Head Start programs in which the shortage of Head Start slots was too small to allow for an adequate control group.

Selected Head Start grantees and centers had to have a sufficient number of applicants for the 2002-03 program year to allow for the creation of a control group without requiring Head Start slots to go unfilled. As a consequence, the study was conducted in communities that had more children eligible for Head Start than could be served with the existing number of funded slots.

At each of the selected Head Start centers, program staff provided information about the study to parents at the time enrollment applications were distributed. Parents were told that enrollment procedures would be different for the 2002-03 Head Start year and that some decisions regarding enrollment would be made using a lottery-like process. Local agency staff implemented their typical process of reviewing enrollment applications and screening children

for admission to Head Start based on criteria approved by their respective Policy Councils. No changes were made to these locally established ranking criteria.

Information was collected on all children determined to be eligible for enrollment in fall 2002, and an average sample of 27 children per center was selected from this pool: 16 who were assigned to the Head Start group and 11 who were assigned to the control group. Random assignment was done separately for two study samples—newly entering 3-year-olds (to be studied through two years of Head Start participation i.e., Head Start year and age 4 year, kindergarten, and 1st grade) and newly entering 4-year-olds (to be studied through one year of Head Start participation, kindergarten, and 1st grade).

The total sample, spread over 23 different states, consisted of 84 randomly selected Head Start grantees/delegate agencies, 383 randomly selected Head Start centers, and a total of 4,667 newly entering children, including 2,559 in the 3-year-old group and 2,108 in the 4-year-old group.[1]

Data collection began in the fall of 2002 and continued through the spring of 2006, following children from entry into Head Start through the end of 1st grade. Comparable data were collected for both Head Start and control group children, including interviews with parents, direct child assessments, surveys of Head Start and non-Head Start teachers, interviews with center directors and other care providers, direct observations of the quality of various care settings, and care provider assessments of children. Response rates were consistently quite high, approximately 80 percent for parents and children throughout the study.

Although every effort was made to ensure complete compliance with random assignment, some children accepted into Head Start did not participate in the program (about 15 percent for the 3-year-old cohort and 20 percent for the 4-year-old cohort), and some children assigned to the non-Head Start group nevertheless entered the program in the first year (about 17 percent for 3-year-olds and 14 percent for 4-year-olds), typically at centers that were not in the study sample. These families are referred to as "no shows" and "crossovers." Statistical procedures for dealing with these events are discussed in this report and the Final Report. The study

[1] The sample of 3-year-olds is slightly larger than the sample of 4-year-olds to ensure that an adequate sample size was maintained, given the possibility of higher study attrition resulting from an additional year of longitudinal data collection for the younger children.

findings provide estimates of both the impact of access to Head Start using the sample of all randomly assigned children and the impact of actual Head Start participation (adjusting for the no shows and crossovers) as well as subgroup impact estimates.

Contents of Report

This Technical Report is designed to provide technical detail to support the analysis and findings presented in the *Head Start Impact Study Final Report* (U.S. Department of Health and Human Services, January 2010). Chapter 1 provides an overview of the Head Start Impact Study and its findings. Chapter 2 provides technical information on the analytical sampling weights used in the analysis. A description of the outcome measures and their psychometric properties is provided in Chapter 3 and the description of the data collection procedures is provided in Chapter 4. Chapter 5 provides a description of the impact analysis methods including ITT (intent-to-treat) impact estimates, IOT (impact on the treated) impact estimates, and subgroup impact estimates.

References

Advisory Committee on Head Start Research and Evaluation (1999). *Evaluating Head Start: A Recommended Framework for Studying the Impact of the Head Start Program.* Washington, DC: US Department of Health and Human Services.

U.S. Department of Health and Human Services, Administration for Children and Families. (January 2010). *Head Start Impact Study. Final Report.* Washington, DC: Author.

Chapter 2: Analytical Sampling Weights

Overview

Sampling weights were calculated for each child to allow estimates based on the sample to represent the national population of newly entering Head Start participants for 2002. Because children were randomly assigned to Head Start (i.e., the "program or Head Start" group) and non-Head Start (i.e., the "control" group) groups within each Head Start center, the two groups represents the same Head Start population of newly entering children when appropriately weighted. The only difference, theoretically, is that the Head Start group was allowed access to attend Head Start at the time of random assignment, while the control group was not.

Each study child was assigned a base weight that reflected his/her overall probability of selection, including the sampling of broad geographic areas used as primary sampling units (PSUs), Head Start grantees/delegate agencies, and centers (see below). These base weights were then adjusted for nonresponse to the child assessment and parent interview at each wave of data collection, to produce separate fall 2002, spring 2003, spring 2004, spring 2005, and spring 2006 weights.[2] The nonresponse-adjusted weights of children in the 4-year-old group were poststratified to the Head Start National Reporting System (HSNRS) newly entering enrollment totals for 4-year-olds (comparable totals for 3-year-olds were not available). Extremely large weights were then trimmed for both age groups. The final child and parent weights are the product of the overall base weight, a nonresponse adjustment factor, a poststratification factor, and a trimming factor. For variance estimation, a set of 76 jackknife replicate weights was created for each child.

These cross-sectional child weights are used for most analyses in this report; the analyses focus on impacts at different time points and include only children and families for whom spring data are available. Fall 2002 weights are used to examine distributions of child and family characteristics at the beginning of the analysis period, in fall 2002. Two sets of longitudinal child weights were also created for use in fitting growth curves. The first set applies to children

[2] The 4-year-olds do not have spring 2006 weights because they were in second grade in 2006 and not included in this wave of data collection.

with assessments at two or more time points, and the second set applies to children with three or more assessments in the fall 2002 to spring 2006 data collection period.

Primary Sampling Unit (PSU) Weights

The frame of 161 PSUs, or geographic clusters, covering all Head Start grantees in the U.S. and Puerto Rico was classified into 25 approximately equal-sized strata based on the following: 1) the level of services for low-income preschool children in the state; 2) the percentage of minority Head Start enrollment in the PSU; 3) the Head Start region; and 4) the percentage of Head Start enrollment in an MSA (a U.S. Census Bureau metropolitan statistical area). One PSU in each stratum was sampled with probability proportional to the total Head Start enrollment of 3- and 4-year-olds in the PSU. The source of enrollment was the 1999-2000 Head Start Program Information Report (PIR). The PSU weight is the inverse of the PSU probability of selection:

> PSU weight = (Total Age 3 & 4 Enrollment in Stratum h) / (Total Age 3 & 4 Enrollment in PSU) where h = 1, 2, ….25.

There was one certainty PSU whose probability of selection was 1 due to its large Head Start enrollment.

Head Start Program Weights

Program Sampling

There were two stages of sampling within most PSUs, and three stages within three extremely large PSUs. Prior to sampling, small programs were collapsed into groups consisting of two to four programs. These were sampled as a unit; thus, the within-PSU probability of selection for each program in a given group is the same.

Prior to telephone screening, programs and program groups (referred to henceforth simply as program groups,[3]) were sampled within the three large PSUs to reduce screening costs. In each of these three PSUs, 12 program groups were sampled with probability proportional to total age three and four enrollment from the 1999-2000 PIR and only these program groups were screened. With this one exception, all programs in the sample PSUs underwent screening,

[3] Note that most "program groups" consisted of a single grantee or delegate agency.

during which study staff collected information on additional characteristics of each program and its community. A major purpose of this screening was to identify situations in which Head Start "saturated" the community, that is, where the local program was large enough that all of the interested and eligible families in the community could be enrolled, making selection of a non-Head Start study group impossible without simultaneously leaving some of the program's capacity unused. After screening, program groups were sampled within the 25 PSUs from among those determined to be neither saturated nor closed. Within each PSU, four program groups were sampled with probability proportional to the total newly entering children ages three and four enrollment. From these, three program groups were subsampled with equal probabilities to be the main sample, and the remaining program group was assigned as a reserve sample. The main sample consisted of 76 program groups (in one PSU, all four program groups were sampled with certainty into the main sample) which comprised 90 individual programs. The reserve sample consisted of 30 programs.

Program Base Weights, Adjustments for Saturation, Raking

Each of the 90 programs in the main sample received a base weight. The program base weight was the inverse of the overall probability of selection for that program, including the PSU probability of selection and the sampling of program groups within the PSU.

The base weights were adjusted for undercoverage due to the deletion from the frame of eight Head Start programs involved in the most recent FACES study (in order to minimize burden on these programs) and 28 programs discovered to be saturated during the screening. Because these programs had no chance of selection, an undercoverage adjustment was needed to correct for bias, in case the deleted programs were systematically different from those retained on the frame (see discussion below) and to prevent weighted enrollment totals from the sample from being too low. The undercoverage adjustment factor was calculated as the ratio of the estimated total newly entering enrollment (including saturated programs) in the PSU to the estimated newly entering enrollment from the sampled programs in the PSU, using enrollment information collected during the telephone screening. This adjustment corrected for differences between saturated and non-saturated programs on broad geographic factors and size in terms of enrollment, but not for other types of differences between the two types of programs within

PSUs—differences that could result in larger or smaller Head Start impacts in the studied sites than in the nation as a whole.

Additionally, the adjusted program weights for all 90 main sample programs were then raked using marginal ages three and four enrollment totals from the 1999-2000 PIR. The raking dimensions were urban status (central city, noncentral city, rural), Head Start region (Northeast, North Central, South, Plains, West), and level of pre-K services in the state (state has Head Start-like programs, state has other types of programs, state has no programs). This procedure served to further match the analysis sample to the full national Head Start program frame on these factors. Since the number of sampled programs in each cross-classification was generally small, raking, or iterative proportional fitting, rather than poststratification, was used (Oh & Scheuren, 1987). In raking, the weights are consecutively ratio-adjusted to marginal totals, typically from an external data source, until the resulting weighted totals converge to the totals for each dimension. The adjustment factor at each iteration is the ratio of the PIR total for the marginal dimension to the sample estimate of the same total, where the weight in the sample estimate is the program weight from the previous raking iteration. This ratio adjustment reduces the sampling error associated with the sampling of PSUs and programs for estimates of Head Start children by urban status and Head Start region (Cochran, 1977). However, it is not intended to result in sample estimates that will agree with external totals of newly enrolled Head Start children, since no such counts exist.

After these undercoverage and raking adjustments were performed, the program weights in two PSUs were further adjusted to compensate for dropping two eligible programs from the sample because of their participation in another Head Start study, the Quality Research Consortium (QRC) and for dropping three programs because they were found to be saturated after sampling. Another program was discovered to have closed, reducing the number of participating programs to 84. The adjustment factor was calculated as the ratio of estimated total newly entering enrollment in the PSU based on the sample of programs in the PSU (excluding one program that had closed) to the weighted newly entering enrollment for the sampled nonsaturated, non-QRC programs in the PSU. None of the programs refused to participate, thus no nonresponse adjustment or reserve programs were needed.

Final Program Weight

Eighty-four programs received a final program weight. The final program weight can be written as:

Final program weight = PSU weight x $(1/ P_1)$ x $(1/ (1-P_{FACES}))$ x $(1/ P_2)$ x $(1/ P_3)$ x F_{Sat1} x F_{RK} x $F_{QRC, Sat2}$

where,

P_{FACES} = probability of selection in FACES,
P_1 = probability of being subsampled prior to telephone screening in three large PSUs,
P_2 = probability of being sampled in PSU,
P_3 = probability of being subsampled for main sample,
F_{Sat1} = adjustment factor for dropping 28 saturated programs from frame before sampling,
F_{RK} = raking adjustment factor to reduce sampling error,
$F_{QRC, Sat2}$ = adjustment factor for dropping two programs participating in QRC and three saturated programs from the sample,

where,

P_1 = 12*(Total Age 3 & 4 Enrollment in Program Total Age 3 & 4 Enrollment in PSU),
P_2 = 4*(1st Yr Age 3 & 4 Enrollment in Program 1st Yr Age 3 & 4 Enrollment in PSU),

$$F_{Sat1} = \frac{\sum_{i=1}^{n+m} w_i * \text{Newly Entering Age 3,4 Enrollment in Program i}}{\sum_{i=1}^{n} w_i * \text{Newly Entering Age 3,4 Enrollment in Program i}}$$ where n is the number of

eligible (nonsaturated) sampled programs in the PSU and m is the number of saturated programs in the PSU that were excluded from sampling. For the n programs, w_i is the program weight that reflects all stages of sampling through P_3. For the m saturated programs, w_i reflects all stages of sampling through P_1 (note P_1=1 except in three very large PSUs, where subsampling was done to reduce the burden of telephone screening).

$$F_{QRC, Sat2} = \frac{\sum_{i=1}^{n} w_i * \text{Newly Entering Age 3,4 Enrollment in Program i}}{\sum_{i=1}^{n-m} w_i * \text{Newly Entering Age 3,4 Enrollment in Program i}}$$ where w_i is the program weight

reflecting all stages of sampling, the F_{Sat1} adjustment, and the raking; n is the number of sampled programs in the PSU (excluding one program that had closed), and m is the number of QRC and saturated programs discovered in the sample in the PSU.

The final program weights for the sample of 84 programs sum to 1,216 with a 95% confidence interval of [959, 1,472].

Head Start Centers

Center Sampling

Within each program, a list of the centers was obtained, and the centers were screened using a Center Information Form (CIF) to collect various statistical data. In addition to screening for saturation at the program level, any centers that were determined to be saturated were dropped from the frame in each program.[4] Prior to sampling, small centers were combined into groups that ranged from two to eight centers and were treated as a unit for sampling purposes. Therefore, each center in a given group had the same probability of selection, namely that of the group. An initial sample of center groups was selected with probability proportional to newly entering age three and four enrollment in the center group. The initial sample of center groups was then subsampled with equal probabilities. The subsample was retained as the main sample in each program, while the remaining center groups formed a reserve sample. In general, three center groups per program (or program group) were selected for the main sample and two for the reserve. However, in very large programs four to six center groups were allocated for the main sample and three for the reserve. Within a program group, the total number of centers was allocated proportionally to the programs based on their newly entering enrollments. A total of 221 center groups (consisting of 448 individual centers) were selected for the main sample, and 114 center groups (consisting of 237 individual centers) were selected for the reserve sample.

Center Base Weights and Adjustments for Saturation and Nonresponse

The center base weight was calculated as the inverse of the overall probability of selection for each center, including the sampling of PSUs, programs, and centers within programs. The center base weights were adjusted for deleting 154 saturated centers and 2 centers participating in a QRC study from the frame prior to center sampling. These adjusted weights were further adjusted for the refusal of five sampled centers to participate in the study,

[4] Hence a center might be excluded from sampling due to saturation, even if the grantee or delegate agency running that center was included in the study (and other centers in that same program or delegate agency were eligible for sampling).

and for the loss of 56 centers discovered to be saturated after sampling. In these centers, no sampling of children was possible. In addition, six centers had closed, and 13 were ineligible for other reasons, such as merging with another center. For the merged centers, where appropriate, an adjustment was made to the base weight of the newly merged center to account for its increased probability of selection, since the individual centers had been listed separately on the center frame.

The adjustment factor for dropping saturated centers from the frame was calculated as the ratio of the estimated total newly entering enrollment (including from saturated centers) in the program to the newly entering enrollment estimated from the sampled centers in the program. The newly entering enrollment was collected on the CIF during center screening and updated during October through December 2002 for all centers where possible. The adjustment factor was calculated separately for each program, unless this resulted in a very large adjustment, in which case the factor was calculated for the PSU.

The adjustment factor for the loss of five refusing and 56 saturated centers was calculated as the ratio of the weighted newly entering enrollment for the entire center sample in the program (excluding those that had closed or merged) to the weighted newly entering enrollment for the nonsaturated, cooperating sampled centers in the program. Overall, these procedures adjusted for size differences between included and excluded centers, but not for other center differences that could lead to different-sized impact estimates.

Final Center Weight

The final center weight can be written as:

Final Center Weight= Final Program Weight x $(1/P_{c1})$ x $(1/P_{c2})$ x F_{QRC} x F_{Sat1} x $F_{Refusal, Sat2,}$

where,

P_{C1} = probability of selection for initial center sample (both main and reserve),
P_{C2} = probability of selection for main center sample,
F_{QRC} = adjustment factor for dropping two centers participating in QRC from frame,
F_{Sat1} = adjustment factor for dropping 154 saturated centers from frame,
$F_{Refusal, Sat2}$= adjustment factor for dropping 56 saturated centers and 5 refusing centers from sample,

$$P_{C1} = \frac{\text{Newly Entering Age 3 \& 4 Enrollment in Center Group}}{\text{(Newly Entering Age 3 \& 4 Enrollment in Program for Eligible, Nonsaturated Centers)}/n_{M+R}},$$

$$P_{C2} = \frac{n_M}{n_{M+R}} = \frac{\#\ Center\ Groups\ Subsampled\ \ for\ Main\ Sample\ in\ the\ Program}{\#\ Center\ Groups\ Sampled\ \ for\ Both\ Main, Reserve\ in\ the\ Program},$$

$$F_{QRC} = \frac{\text{Newly Entering Age 3,4 Enrollment in Program}}{\text{(Newly Entering Age 3,4 Enrollment in Program) - (Newly Entering Enrollment in 2 QRC Centers)}}$$

Note that F_{QRC} = 1.25103 for centers in the one program that contained the two QRC centers and is equal to one for centers in all remaining programs.

$$F_{Sat1} = \frac{\sum_{i=1}^{n+m} w_i * \text{Newly Entering Age 3,4 Enrollment in Center}_i}{\sum_{i=1}^{n} w_i * \text{Newly Entering Age 3,4 Enrollment in Center}_i}$$

where n is the number of sampled centers in the program, m is the number of saturated centers on the frame in the program that were excluded from sampling, w_i is the center weight through the F_{QRC} adjustment for sampled centers, and w_i is the final program weight for saturated centers excluded from the frame in the program prior to center sampling.

$$F_{refusal,Sat2} = \frac{\sum_{i=1}^{n} w_i * \text{Newly Entering Age 3,4 Enrollment in Center}_i}{\sum_{i=1}^{n-m} w_i * \text{Newly Entering Age 3,4 Enrollment in Center}_i}$$

where n is the number of sampled centers in the program, m is the number of refusing and saturated centers discovered among those sampled in the program, and w_i is the center weight through the F_{sat1} adjustment.

The final center weight reflects the PSU and program probabilities of selection. In four programs, all reserve centers were brought into the sample when the original centers were found to be saturated or partially saturated and hence unable to provide the planned number of control group children. In these centers, P_{C2} was set to one in the above formula. When this resulted in a census of eligible centers in the program, both P_{c1} and P_{c2} were set to one. In six programs where some, but not all, of the reserve centers were activated to offset saturation in the main sample, n_M includes the reserves that were activated as well as the main sample centers. In this situation,

centers were randomly subsampled from among the reserve centers selected for that particular program or program group. The total number of centers in the final sample, including main sample and activated reserves was 458. The sample was reduced to 378 after losing 19 centers identified following selection as ineligible (closings, mergers), five identified as noncooperating, and 56 found to be saturated.

Reserve centers were picked at random from the same pool as the main sample centers, from the same program where possible, but with no other attempt to match them with the characteristics of the centers they were replacing. The purpose of the reserve sample was primarily to prevent a sample size shortfall due to loss of centers, rather than to reduce the bias caused by exclusion of saturated and refusing centers from the study. The weighting adjustments to the center weights were designed to accomplish the latter.

The final center weights for the 378 centers sum to 12,705 with a 95% confidence interval of [10,290, 15,119].

Comparison of Head Start Grantees/Delegate Agencies and Centers in Saturated and Non-Saturated Communities

As discussed in Chapter 2, there is potential for undercoverage bias due to the exclusion from the sampling frame of Head Start grantees/delegate agencies and centers in communities saturated by the program, that is, communities with too few families who are able, eligible or interested in accessing Head Start (beyond those the program can accommodate) to provide a randomly selected control group for the study. Newly entering Head Start children in these saturated communities had no chance of selection and therefore are not represented by our sample. Consequently, the potential for bias arises if the saturated grantees/delegate agencies and centers are systematically different from the non-saturated grantees/delegate agencies and centers we retained in the sampling frame and if the characteristics on which they differ are correlated with the outcome measures for and impact estimates on the children they enroll. However, if the children in these excluded grantees/delegate agencies and centers represent only a small percentage of the Head Start population, then the potential for bias is much less. Based on the sample coverage rate reported in Chapter 2 of the Final Report, 15.5 percent of the children served by Head Start nationally are omitted from the study. This noncoverage rate is based on grantees and centers identified in the sample frame and samples that were excluded due

to saturation. It equals 1 minus the product of four coverage rates: program frame x program sample x center frame x center sample. Mathematically, this equates to 1-(0.962 x 0.975 x 0.952 x 0.947) = 1-0.845 = 0.155.

Head Start Grantees/Delegate Agencies

Exhibits 2.1 and 2.2 compare saturated and non-saturated grantees/delegate agencies by a few characteristics available on the Head Start Program Information Report (PIR) database (and, for newly entering enrollment and additional center information, telephone screening confirmation calls to grantees and delegate agencies prior to sampling). The grantees/delegate agencies were weighted to account for sampling of broad geographic areas (i.e., PSUs) and for the subsampling of grantees/delegate agencies in three large urban cities prior to the telephone screening (see Chapter 2 of the Final Report). This was necessary to draw conclusions about the entire population of children served by Head Start and not merely the children served by grantees/delegate agencies in the 25 sampled PSUs that were screened to determine saturation. Tests of statistical significance were performed to reduce the possibility of drawing false conclusions from differences that may have been due to sampling error. The hypothesis testing was done in WesVar using jackknife replicate weights to account for the study's complex sample design.

Exhibit 2.1: Comparison of Saturated and Non-Saturated Head Start Grantees/Delegate Agencies by Enrollment

Enrollment Variable	Saturated Programs	Non-Saturated Programs	*P*-Value (*t*-Test of Difference)
Percent Hispanic Enrollment	9%	26%	**0.001**
Percent Black Enrollment	20%	33%	0.134
Age 3 Enrollment as Percent of Total Enrollment	52%	49%	0.535
Average Total Enrollment	188	571	**<0.001**
Average Newly Entering Enrollment	113	388	**<0.001**

Exhibit 2.2: Comparison of Saturated and Non-Saturated Head Start Grantees/Delegate Agencies by Location Characteristics

Characteristics	Saturated Programs	Non-Saturated Programs	*p*-Value (Chi-Square Test of Association)
School-based			0.018
Yes	66%	21%	
No	34%	79%	
Metro Status			0.91
MSA	66%	68%	
Non-MSA	34%	32%	
Level of Pre-K Services in State			0.60
Similar to Head Start	35%	25%	
Some Head Start-Like	27%	20%	
Remaining States	38%	55%	
Head Start Region			0.15
Northeast	24%	25%	
North Central	48%	24%	
South	28%	39%	
Plains	0%	4%	
West	0%	8%	

As shown in these tables, the saturated grantees/delegate agencies are much smaller, much more likely to be school-based, and have smaller percentages of Hispanic enrollment than the non-saturated grantees/delegate agencies. Although they appear to be more often located in the Midwest, differences in the distribution of saturated vs. non-saturated grantees/delegate agencies by Head Start regions are not statistically significant. A cautionary note is that variances at the program level are not very stable because the number of saturated grantees/delegate agencies is small. In addition, variances do not include the between-PSU component of variance due to sampling PSUs; thus, they are underestimates, and the p-values may be slightly overstating the significance of the differences.

Head Start Centers

Exhibits 2.3 and 2.4 compare saturated and non-saturated centers by various qualitative characteristics and enrollment variables available from the CIFs completed by all centers in the sampled grantees and delegate agencies. All hypothesis testing was again done in WesVar using jackknife replicate weights to account for the study sample design. The replicate weights do not include the between-PSU variance component; therefore, the p-values in these tables may

slightly overstate the significance of the difference. In Exhibit 2.3, the chi-square test was not able to detect a significant difference for type of program option offered, whether staff members are school employees, metro status, region, or level of Pre-K services available in the state. With respect to enrollment, Exhibit 2.4 shows that the saturated centers are smaller, have fewer Hispanic children, and have a larger percentage of first-year 3-year-olds than the non-saturated centers. As expected, these centers do not have waiting lists, a significant difference from non-saturated centers.

Exhibit 2.3: Comparison of Saturated and Non-Saturated Head Start Centers Operated by Non-Saturated Programs, by Program and Location Characteristics

Characteristics	Saturated Centers	Non-Saturated Centers	*p*-Value (Chi-Square Test of Association)
Program Option			0.44
Full-Day Only	35%	28%	
Part-Day Only	52%	50%	
Other	13%	22%	
Staff Are School Employees			0.249
Yes	17%	11%	
No	83%	89%	
Metro Status			0.64
MSA	74%	70%	
Non-MSA	26%	30%	
Head Start Region			0.376
Northeast	32%	27%	
North Central	34%	20%	
South	17%	31%	
Plains	12%	11%	
West	4%	11%	
Level of Pre-K Services in State			0.212
Similar to Head Start	40%	22%	
Some Head Start-Like	15%	18%	
Remaining States	45%	60%	

Exhibit 2.4: Comparison of Saturated and Non-Saturated Head Start Centers Operated by Non-Saturated Programs, by Enrollment

Enrollment Characteristic	Saturated Centers	Non-Saturated Centers	*p*-Value (t-test of Difference)
Percent Hispanic Enrollment	17%	30%	**0.005**
Percent Black Enrollment	38%	26%	0.204
Percent Newly Entering Enrollment	65%	66%	0.985
Age 3 Enrollment as Percent of Newly Entering Enrollment	54%	47%	**0.037**
Number of Children on Waiting List as Percent of Total Enrollment	0%	15%	**<0.001**
Average Number Funded Slots	37	48	**0.036**
Average Total Enrollment	26	47	**<0.001**
Average Newly Entering Enrollment	16	31	**<0.001**
Average Number on Waiting List	0	9	**<0.001**

Two graphs follow Exhibit 2.4 that show the percentage of centers that are saturated for each of the 84 grantees/delegate agencies with less than 100 percent saturation rate. The saturation rate was calculated two ways: as the percentage of centers in each program that are saturated (Exhibit 2.5) and as the percentage of newly entering enrollment in saturated centers for each program (Exhibit 2.6). The average percentage of saturated centers is 16.6 percent, while the average percentage of newly entering enrollment in saturated centers is 13.2 percent, another indication that the saturated centers tend to be smaller. The graphs show the variation among grantees/delegate agencies in the share of centers operating in saturated communities and the share of newly entering children served by those centers.

Exhibit 2.5: Percentage of Centers That Are Saturated for Each Grantee/ Delegate Agency

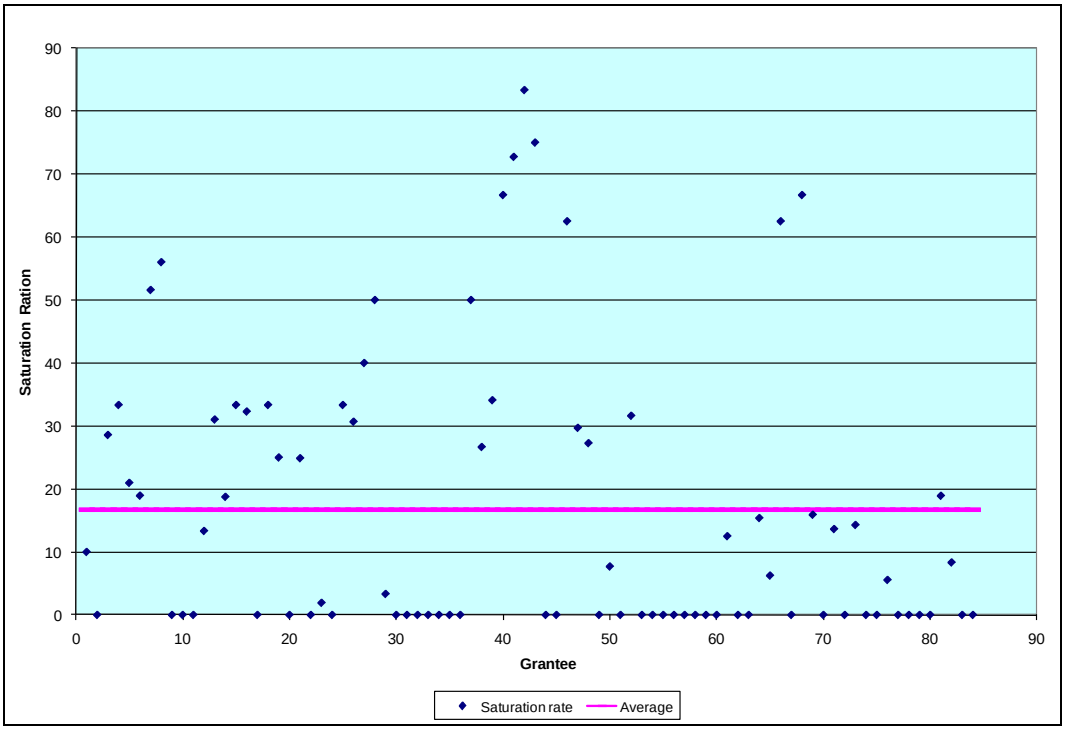
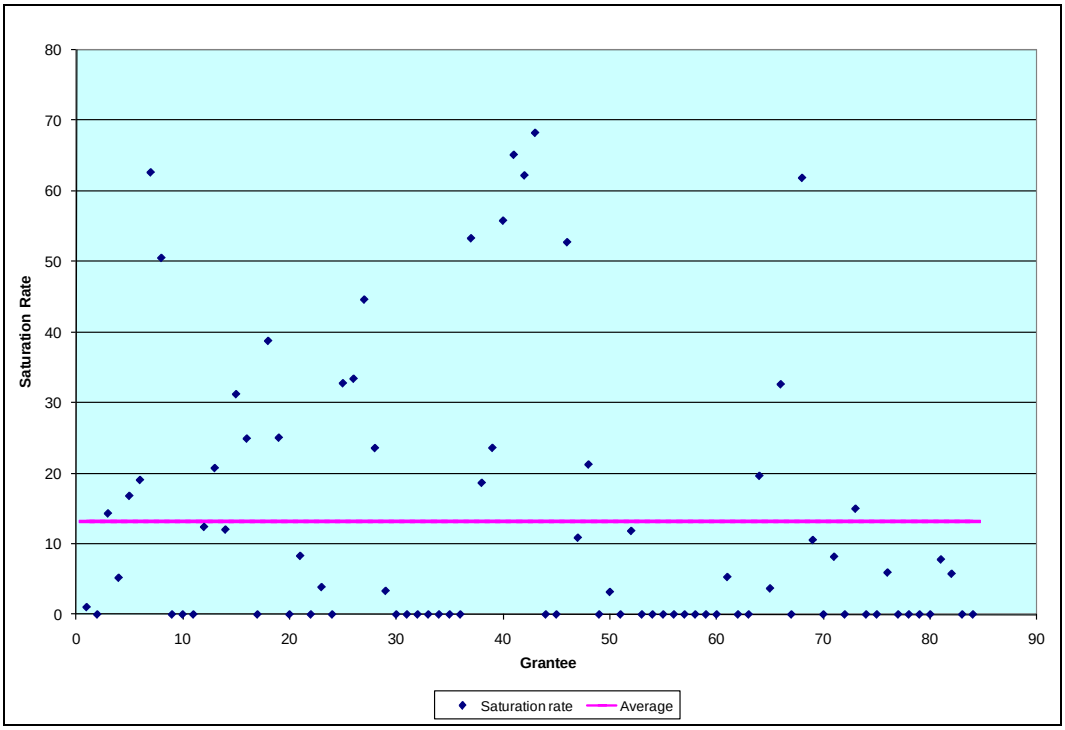


Exhibit 2.6: Percentage of Newly Entering Enrollees in Saturated Centers



Child Weights

Random Assignment of Children Within Centers

The random assignment of children, that is, the sampling of children into the Head Start and control groups, began with the acquisition of information on every applicant for the 2002-03 program year and the number of available slots in the center. Eligible returning children (who were not subject to random assignment) were allowed to attend Head Start. The remaining eligible newly entering children on the center's applicant list were then sorted based on child need (using local program criteria), as they normally would be to determine which children to admit and which to put on a waiting list, and the list was truncated at exactly the number of children needed to both fill the center's remaining slots and supply a non-Head Start group sample of the desired size for the study. A sample of children was randomly selected with equal probabilities from the truncated list to fill the center's slots. Those not selected to fill a slot from the truncated list were assigned to the control group. The children sampled to fill the center's slots were then subsampled to obtain the targeted number of Head Start group children. This resulted in three categories of children: (1) those sampled to attend the Head Start program who would not be included in the study, (2) those sampled for the study's Head Start group, and (3) those sampled for the study's control group. All remaining applicants (including those coming in during the year) were put on the waiting list; these children had no chance of selection for either study sample but could enter the Head Start program later (once sampling ended) to replace children who dropped out of the program over the course of a year. The targeted number of Head Start and control group children was 16 and 11, respectively, at most centers and center groups, cumulating to an average of 48 Head Start group members and 32 control group cases for each sampled program group. This uneven balance only slightly reduces the statistical precision of the impact estimates and hence the probability of detecting as statistically significant any impact that does occur compared to a perfectly balanced design;[5] its advantage is in reducing the number of control group children excluded from the program at each center or center group. In center groups, the 16 Head Start group children and 11 control group children were proportionally allocated to the centers in the group based on newly entering enrollment. In three of the 84 programs, children applied directly to the program rather than the center, so it was

[5] Standard errors of impact estimates and minimum detectable effect sizes increase by about two percent.

necessary to randomly assign children at the program level and sample 48 Head Start group and 32 control group cases to obtain 80 children for the program in total. The total target sample size was approximately 3,600 Head Start group children and 2,400 control group children.

The random assignment of children was spread out over the spring/summer 2002, because most centers took applicants on a flow basis and preferred to let their families know soon whether their child had been accepted to attend the Head Start program. This meant children were sampled in batches or rounds, and the sampling process described above took place more than once in most centers. An additional complication was that stratification by program option (e.g., part- vs. full-time) was used in many centers. The allocation of the total number of Head Start and control group children across program options and rounds at each center was approximately proportional to the newly entering enrollment in each program option and the number of slots filled in each round. The actual probabilities of selection for each child were stored electronically for weighting purposes. However, the probabilities can vary greatly because of the difficulty in allocating across rounds. There were many rounds where children were sampled to fill slots but no Head Start or control group children were selected because the target sample sizes of Head Start and control group children had already been obtained. None of these children had a chance of selection for the study, meaning child weights based on the actual probabilities of selection would underestimate the size of the first-year Head Start population. Therefore, the within-center child probabilities of selection were calculated as a simple sampling fraction: the number of children sampled in the center divided by the newly entering fall 2002 age 3 & 4 enrollment in the center.

Child Base Weights

The within-center child base weight was calculated as:

$$\frac{\textit{Newly Entering Age 3 \& 4 Enrollment in Center}}{\textit{\# Head Start Children Sampled in Center}}$$

for the sampled Head Start group children, and as

$$\frac{\textit{Newly Entering Age 3 \& 4 Enrollment in Center}}{\textit{\# Control Groups Children Sampled in Center}}$$

for the control group children. Note that the numerator is the same for both groups, since estimates are to be made for the universe of newly entering Head Start children using either sample. For centers where the updated fall 2002 newly entering enrollment was not obtained, the newly entering enrollment figure for the previous program year was used. When this was missing, and for three programs where children were randomly assigned at the program level rather than at the center level, the inverse of the actual probability of selection for children in the center was used as the base weight.

The overall child base weight reflecting all stages of sampling can be written as:

Overall Child Base Weight = (Final Center Wt) x (Within-Center Child Base Wt.)

where the final center weight reflects the PSU and program probabilities of selection and includes an adjustment for centers where no children were sampled because of center noncooperation or saturation.

Nonresponse Adjustments

Nonresponse adjustments were performed separately for fall 2002 and at each subsequent spring, using multiple definitions of a respondent at each time point. The first two definitions are (1) child is considered a complete for the child assessment, and (2) child is considered a complete for the parent interview. This results in two nonresponse-adjusted child weights at each time point, to be used in the analysis according to the source of the outcome variable (child assessment or parent interview). Additional weights, described below, are used for more secondary analyses.

The nonresponse adjustment helps control nonresponse bias by compensating for different data collection response rates across various demographic and geographic groups of children. This is due to the fact that the nonresponse adjustment factor is calculated within nonresponse adjustment cells formed by the demographic and geographic variables. The nonresponse adjustment factor spreads the weight of the nonresponding children over the responding children in that cell, so that they represent not only children who were not sampled, but also the nonresponding sampled children. This maintains the same mix of the sample across cells along these particular characteristics as would have been present had there been no nonresponse.

To capture the variation in response rates, cells were created based on characteristics that correlated with response rates. For the fall 2002 nonresponse adjustments, a nonresponse analysis using chi-square tests and logistic regression in WesVar showed high correlation between response rates and Head Start versus control group assignment and program option for the control group. This result, combined with a desire to capture individual Head Start program differences as much as possible, led to nonresponse adjustment cells formed by crossing PSU x state x program for the Head Start group, and PSU x program option x state x program for the control group. Collapsing across program and state was done as needed to prevent excessively large nonresponse adjustment factors.

To determine the nonresponse adjustment cells for the spring data collections, an unweighted nonresponse analysis was done using a software package called CHAID (Chi-squared Automatic Interaction Detector) separately for the child assessment and the parent interview, to determine what variables are correlated with propensity to respond. The following variables were used as candidates in the analysis:

- Head Start group versus control group,
- Child's race/ethnicity (White, Black, Hispanic, Other),
- Child's language (English, Spanish, Other),
- Language spoken at home (English, Spanish, Other),
- Child's gender,
- Program option applied for (full-day, part-day, both, home-based),
- Child's age (3 or 4),
- Metro status for county containing Head Start program office (MSA, nonMSA based on Census data),
- Urban location for county containing Head Start program office (Central City, Urban Fringe of Central City, Outside Central City based on USDA Beale codes),
- Level of pre-K services in the state (has Head Start or Head Start-like programs, has other types of pre-K programs, remaining states),
- Head Start region (Northeast, North Central, South, Plains, West),
- State containing Head Start program office,
- Response status for fall 2002 child assessment (spring 2003, 2004 only),
- Response status for fall 2002 parent interview (spring 2003, 2004 only),
- Head Start participation status in 2002-03 (crossovers) for control group children (yes/no),

- Head Start participation status in 2002-03 (no-shows) for Head Start group children (yes/no),
- PSU.

These variables were chosen because they were available for nearly every sampled child. Fall 2002 response status was dropped after the spring 2004 CHAID analysis because it produced cells which were too small for nonresponse adjustments. A small number of missing values for the variables used in the nonresponse analysis were imputed via hot deck imputation using procedures described in Chapter 5. Variables with missing values were child's language, home language, child's race, and child's gender. In spring 2003, weighted logistic regression and chi-square tests were also run in WesVar to confirm the CHAID results. The variables that were identified by CHAID as correlated with spring response propensity each year are provided in Exhibit 2.7. The strongest association was found for the Head Start group/control group indicator, No-Show/Crossover status, Fall 2002 response status, and PSU. The tree structure identified by CHAID, based on the variables identified in Exhibit 2.7, was used to create the nonresponse adjustment cells for each spring data collection. Note that in spring 2006 no data were collected for the age four cohort as they were in second grade and at that point were no longer eligible for the study.

Some collapsing of cells was required to prevent excessively large nonresponse adjustment factors, which cause the weights to become more variable and the variance of most estimates from the data to increase. A final set of collapsed cells for each nonresponse adjustment was chosen based on a compromise between limiting the increase in weight variability and the need to control for nonresponse bias by limiting the amount of cell collapsing.

Exhibit 2.7: Variables Identified by CHAID as Correlated with Child Assessment (CA) and Parent Interview (PI) Nonresponse

	Spring 2003		Spring 2004		Spring 2005		Spring 2006	
Variables	CA	PI	CA	PI	CA	PI	CA	PI
Head Start group/control group	X	X	X	X	X	X	X	X
Child's race	X	X	X	X		X		X
Child's language							X	X
Home language		X		X				
Child's gender	X	X		X				X
Child's age		X	X	X		X		
Program option applied for			X					
Metro status	X	X						
Level of pre-K services	X	X						
Head Start region	X	X						
State			X					X
Crossover status					X	X	X	X
No-show status	X			X	X	X	X	X
PSU	X	X	X	X	X	X	X	
Fall 2002 response status	X	X	X					

NOTE: X identifies in which years CHAID identified a variable as correlated with nonresponse (prior to nonresponse adjustment).

Exhibit 2.8 provides unweighted response rates for the child assessment and parent interview, by child and Head Start program characteristics identified by CHAID as correlated with response rates. These variables were used to construct nonresponse adjustment cells, thus differences in response rates among groups of children are compensated for in the nonresponse-adjusted weights. The nonresponse adjustment reduces nonresponse bias when the outcome assessment and parent interview variables are correlated with the variables used to create the nonresponse adjustment cells, by restoring the sample of responding children to the original representative distribution.

Exhibit 2.8: Unweighted Response Rates for Child Assessment (CA) and Parent Interview (PI) by Child and Program Characteristics

	Spring 2003				Spring 2004				Spring 2005				Spring 2006			
	CA		PI		CA		PI		CA		PI		CA		PI	
	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C
Child Cohort																
3 = Age three cohort	89	79	87	80	86	79	86	78	82	75	85	78	80	72	83	75
4 = Age four cohort	87	76	85	77	81	72	82	73	80	72	82	75	NA	NA	NA	NA
Child's Race																
1 = White	88	78	87	79	86	77	86	79	81	73	84	78	77	68	83	73
2 = Black	88	73	86	75	84	76	83	74	81	71	83	73	81	73	84	74
3 = Hispanic	87	80	87	81	83	74	84	76	81	75	83	77	80	74	82	77
4 = Other	87	81	87	83	82	79	80	78	79	78	82	79	83	74	87	74
Child's Language																
1 = English	88	76	86	78	84	76	84	76	80	72	83	76	79	70	83	73
2 = Spanish	87	79	87	80	83	76	84	77	82	77	85	78	81	78	83	81
3 = Other	80	85	80	85	82	82	76	79	86	74	86	74	82	81	82	81
Home Language																
1 = English	88	76	86	78	84	76	84	76	80	73	83	76	79	70	83	73
2 = Spanish	87	79	86	80	84	75	85	76	83	76	85	77	81	77	83	80
3 = Other	86	89	87	92	86	81	84	79	80	76	85	76	79	78	84	78
Child's Gender																
0 = Female	86	77	85	79	82	76	83	76	80	73	82	76	80	70	83	73
1 = Male	89	78	88	79	85	76	85	77	82	74	85	77	79	74	83	77
Program Option Applied For																
1 = Full day	87	75	85	76	84	74	83	73	81	71	83	73	79	70	83	73
2 = Part-day only	89	79	88	81	85	78	85	79	82	75	84	78	80	74	84	76
3 = Both Full and Part-day	80	72	84	75	69	72	73	75	71	78	80	78	70	76	76	76
4 = Other	86	81	86	80	80	76	84	76	80	77	83	81	83	77	85	79
Metro Status																
0 = nonMSA	89	80	88	82	86	80	87	82	82	76	86	80	77	74	84	80
1 = MSA	88	77	86	78	83	75	83	75	81	73	83	75	80	72	83	74

Exhibit 2.8: Unweighted Response Rates for Child Assessment (CA) and Parent Interview (PI) by Child and Program Characteristics (continued)

	Spring 2003				Spring 2004				Spring 2005				Spring 2006			
	CA		PI		CA		PI		CA		PI		CA		PI	
	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C
Level of State-funded Pre-K Services in State																
1 = State has programs similar to Head Start	85	81	84	84	80	76	81	75	77	76	81	78	78	73	82	75
2 = State has programs with some components of Head Start	89	75	85	76	85	75	86	76	83	71	85	74	76	68	81	71
3 = Remaining States	88	77	87	78	85	76	85	76	82	74	84	77	82	73	85	77
Head Start Region																
Northeast (1,2,3)	86	77	85	79	84	78	85	77	80	75	84	77	78	71	83	74
South (4,6)	89	77	87	78	85	76	84	75	82	72	83	75	80	72	82	74
North Central (5)	87	77	85	79	81	72	83	74	81	73	85	77	81	72	86	76
Plains (7,8)	85	80	85	80	82	75	84	78	75	73	82	76	67	71	72	71
West (9,10)	87	78	88	80	84	79	84	79	81	76	83	79	83	77	86	83
Crossover Status																
0 = Control child did not enroll in Head Start	NA	75	NA	77	NA	75	NA	75	NA	72	NA	75	NA	70	NA	73
1 = Control child did enroll in Head Start	NA	93	NA	93	NA	85	NA	84	NA	84	NA	86	NA	85	NA	87
No-show Status																
0 = Head Start group child enrolled in Head Start	94	NA	92	NA	89	NA	89	NA	86	NA	88	NA	84	NA	87	NA
1 = Head Start group child did not enroll in Head Start	63	NA	63	NA	61	NA	62	NA	60	NA	64	NA	58	NA	63	NA
Fall 2002 Response Status																
0 = No	40	36	40	37	43	43	45	42	42	40	45	43	45	43	50	43
1 = Yes	96	93	94	93	91	88	91	88	88	86	90	88	85	83	89	86

Exhibit 2.8: Unweighted Response Rates for Child Assessment (CA) and Parent Interview (PI) by Child and Program Characteristics (continued)

	Spring 2003				Spring 2004				Spring 2005				Spring 2006			
	CA		PI		CA		PI		CA		PI		CA		PI	
	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C	HS	C
PSU																
102	87	81	87	81	88	82	88	82	88	83	89	85	88	90	88	90
108	88	78	89	78	84	79	85	78	80	72	82	78	84	75	87	85
112	85	80	85	80	82	75	84	78	75	73	82	76	67	71	72	71
115	86	77	88	82	78	76	79	79	73	73	77	74	71	62	79	62
127	95	89	96	89	89	81	89	85	86	77	90	85	83	77	89	83
206	97	85	95	85	85	80	87	83	82	83	84	83	74	86	83	91
208	84	67	81	69	86	67	81	71	81	60	84	71	78	59	80	63
210	87	88	87	91	82	86	87	87	80	82	86	87	82	85	91	89
224	91	77	84	74	73	71	70	71	86	77	86	81	85	75	85	79
241	84	64	81	67	78	51	81	56	81	61	84	64	84	56	88	61
311	87	78	83	78	87	81	89	82	84	76	88	80	66	54	77	61
323	90	76	87	78	82	74	83	74	86	73	87	74	84	86	87	86
330	88	71	86	71	84	65	82	70	83	63	85	70	76	64	81	70
338	89	80	85	83	86	81	88	80	85	72	85	72	88	76	88	76
356	88	84	88	88	75	73	75	71	77	73	77	71	85	61	88	61
358	90	70	87	73	85	76	86	74	74	72	78	72	73	74	77	74
367	92	78	88	76	87	76	83	65	82	69	83	71	79	62	82	64
368	88	74	87	78	91	84	91	83	90	80	89	85	84	80	86	83
380	83	72	82	73	77	68	77	68	72	67	72	68	71	76	71	73
406	87	73	77	70	83	66	86	64	77	70	82	70	81	72	83	72
417	79	83	78	83	73	70	70	70	63	67	66	69	71	67	73	69
421	93	84	93	84	93	85	94	84	94	84	94	85	90	84	92	86
423	75	75	81	81	73	76	77	75	73	75	81	76	64	72	70	74
427	83	56	83	61	87	71	84	71	76	62	80	62	85	62	88	62
502	94	90	94	90	88	89	90	89	91	89	92	90	86	78	89	83

HS indicates Head Start group. C indicates control group. NA indicates not applicable.

Poststratification

To reduce the sampling error for estimates of the newly entering Head Start population, the nonresponse-adjusted child weights for children in the 4-year-old group were poststratified to fall 2003 HSNRS newly entering enrollment totals by race/ethnicity. (The HSNRS is a census of Head Start programs, so there should be no sampling error associated with its enrollment totals. However, race reporting may differ somewhat between the HSNRS and the current study, as the Head Start programs were given no specific instructions on how to code the variable in the HSNRS, and the poststratification target data describe patterns one year later than study sample enrollment in fall 2002.) Comparable enrollment totals were not available for 3-year-olds. The three race/ethnicity categories were Hispanic, non-Hispanic Black, and White/Other. An adjustment factor was calculated for each category, and the appropriate factor applied to each child weight depending on the race of the child, as reported on the HSIS child roster. The numerator of each factor was the proportion of HSNRS total newly entering age four enrollment in the race/ethnicity category; the denominator was the sample estimate of this proportion using the 84 programs sampled for the current study, the final program weight, and the HSNRS newly entering age four enrollment reported for each program:

$$F_{PS,k} = P_k \,/(\sum_{i=1}^{84} w_i E_{i,k} \,/ \sum_{i=1}^{84} w_i (E_{i,1} + E_{i,2} + E_{i,3})$$

where w_i is the final program weight, $E_{i,k}$ is the age four newly entering enrollment in the k-th race/ethnicity category in the i-th program from the HSNRS, and P_k is the proportion of age four newly entering enrollment in the k-th race/ethnicity category from the HSNRS, using the 1,717 programs remaining on the HSNRS after restriction to the same types of programs included on the PIR frame for the HSIS.

The poststratification factors were 0.80 for Hispanic, 1.45 for non-Hispanic Black, and 1.036 for White/Other, indicating an overrepresentation of Hispanic children and underrepresentation of Black children in the current study sample for the age four cohort as compared to the HSNRS. Chapter 5 provides a detailed analysis of the race/ethnicity composition of the sample and its comparison to national Head Start data.

Trimming

A final trimming adjustment was made for inordinately large child weights. Very large weights can substantially increase sampling error, so weights were trimmed back to four times the average weight to avoid large sampling errors, even though this introduces a small amount of bias into the survey estimates. However, the amount of trimming was very slight: two percent or fewer of the child assessment and parent interview weights were trimmed back each year. An analysis of the trimmed cases showed that most extremely large weights were due primarily to some large centers being undersampled, that is, only a few children were sampled, perhaps due to near-saturation. The final child weight can be written as:

Final Child Weight = (Overall Child Base Wt) x (Child Nonresponse Adjustment Factor) x (Poststratification Factor) x (Trimming Factor)

where the overall child base weight reflects the probability of selecting the PSU, program, center, and child within center. When the final child weight is applied, the Head Start and control groups, each separately represent the entire newly entering Head Start population in fall 2002. Sample estimates of the size of the newly entering Head Start population that year are given in Exhibit 2.9 in the "Sum of Final Weights" column; Exhibit 2.10 contains unweighted and weighted response rates at each data collection period from fall 2002 through spring 2006 for the child assessment and the parent interview. These response rates are conditional on the sampled centers and programs where random assignment of Head Start applicants was permitted; they do not represent coverage rates of the Head Start newly entering population.

Exhibit 2.9: Final Sampling Weights, Fall 2002 through Spring 2006

Time Period	Number of Respondents	Sum of Final Weights	95% Confidence Interval	Coefficient of Variation (CV) of Final Weights (%)
Fall 2002				
Child Assessment				
Head Start Group	2,360	422,686	(356,049, 489,323)	86
Control Group	1,363	413,258	(351,102, 475,415)	77
Parent Interview				
Head Start Group	2,489	423,086	(357,343, 488,829)	85
Control Group	1,526	414,214	(350,637, 477,792)	78
Spring 2003				
Child Assessment				
Head Start Group	2,441	426,834	(355,935, 497,733)	86
Control Group	1,457	418,907	(357,034, 480,781)	88
Parent Interview				
Head Start Group	2,404	427,536	(358,052, 497,020)	86
Control Group	1,483	419,772	(357,437, 482,107)	88
Spring 2004				
Child Assessment				
Head Start Group	2,331	426,911	(361,442, 492,380)	88
Control Group	1,431	421,590	(355,310, 487,869)	91
Parent Interview				
Head Start Group	2,342	427,732	(363,504, 491,959)	88
Control Group	1,433	423,218	(359,210, 487,225)	91
Spring 2005				
Child Assessment				
Head Start Group	2,254	428,291	(363,741, 492,842)	83
Control Group	1,385	418,834	(353,454, 484,215)	83
Parent Interview				
Head Start Group	2,327	428,137	(362,525, 493,750)	83
Control Group	1,438	419,759	(355,596, 483,921)	84
Spring 2006 (age 3 cohort only)				
Child Assessment				
Head Start Group	1,218	225,766	(189,555, 261,977)	84
Control Group	742	224,475	(183,758, 265,191)	84
Parent Interview				
Head Start Group	1,274	225,766	(188,671, 262,861)	85
Control Group	772	224,475	(186,606, 262,344)	82

Exhibit 2.10: Unweighted and Weighted Cross-Sectional Response Rates by Wave

Unweighted Cross-Sectional Response Rates (%) by Wave										
	Fall 2002		2003		2004		2005		2006	
Instrument	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4
Child Assessment										
Head Start Group	87	85	89	87	86	81	82	80	80	NA
Control Group	74	74	79	76	79	72	75	72	72	NA
Parent Interview										
Head Start Group	92	89	87	85	86	82	85	82	83	NA
Control Group	84	82	80	77	78	73	78	75	75	NA
Weighted Cross-Sectional Response Rates (%) by Wave										
	Fall 2002		2003		2004		2005		2006	
Instrument	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4
Child Assessment										
Head Start Group	87	86	89	87	87	81	82	79	81	NA
Control Group	76	77	80	77	79	74	77	73	74	NA
Parent Interview										
Head Start Group	93	90	88	85	86	82	85	82	85	NA
Control Group	84	84	81	79	79	75	79	75	76	NA

NA indicates not applicable.

Teacher Survey/Teacher Child Rating Weights

Children who were attending a pre-K program (either Head Start or some other type of program), kindergarten, or first grade were eligible for the Teacher Survey and Teacher's/Care Provider's Child Report (see Chapter 2 in the Final Report for a description of these forms). Children who were receiving only their parent's care at home were not eligible for these surveys. A cross-sectional weight was created each spring for children with a completed Teacher Survey and Teacher Child Report, and a completed child assessment and parent interview as well. The child's base weight was first adjusted for nonresponse to both the child assessment and parent interview, then poststratified and trimmed as described above for the child assessment and parent interview weights. This weight was then adjusted for nonresponse to both the teacher survey and teacher child rating, using CHAID as described above to identify variables correlated with nonresponse.

Classroom Observation Weights

In spring 2003 and 2004, only pre-K children attending a Head Start or other type of pre-K center (including another home) were eligible for classroom observations (see Chapter 2 in the Final Report for a description of this data type). Thus in spring 2004, members of the age four cohort who were in kindergarten do not have classroom observations. No classroom observations were conducted in spring 2005 or 2006 since most children were in kindergarten or first grade by then. A cross-sectional classroom observation weight was calculated for every child with a completed classroom observation, child assessment, and parent interview that year, in spring 2003 and 2004. This was done by adjusting the child's base weight first for nonresponse to the child assessment and parent interview each spring, poststratifying and trimming as before, then adjusting for "nonresponse" to the classroom observations, using CHAID to identify variables correlated with nonresponse.

Director Interview Weights

The director interview was conducted at Head Start centers or other types of pre-K centers in spring 2003 and 2004 where the sampled children attended (see Chapter 2 in the Final Report for a description of this instrument). No director interviews were conducted in spring 2005 and 2006 as most children were attending school by then. A cross-sectional director interview weight was calculated for every child with a completed classroom observation, child assessment, and parent interview that year, in spring 2003 and 2004. This was done by adjusting the child's base weight first for nonresponse to the child assessment and parent interview each spring, poststratifying and trimming as before, then adjusting for "nonresponse" to the director interview, using CHAID to identify variables correlated with nonresponse.

Response Rates and Variables Correlated with Teacher, Classroom, and Director Interview Response Rates

The response rates for the teacher survey/teacher child report, classroom observations, and director interview are provided in Exhibit 2.11. The response rates are based on only the children eligible for each instrument among those with a complete child assessment and parent interview that year. The variables identified by CHAID as correlated with having a completed teacher survey and teacher child report, classroom observations, or a completed director

interview, conditional on response to the child assessment and parent interview each year, are provided in Exhibit 2.12. Note that for the age four cohort, no data were collected in spring 2006.

Exhibit 2.11: Unweighted and Weighted Response Rates by Wave for Teacher Survey/ Teacher Child Report (TS/TCR), Classroom Observation, and Director Interview, Conditional on Child Assessment and Parent Interview Response

Spring 2003-2006 Cross-Sectional Response Rates (%)								
Unweighted Response Rates by Wave								
	Spring 2003		**Spring 2004**		**Spring 2005**		**Spring 2006**	
Instrument	**Age 3**	**Age 4**	**Age 3**	**Age 4**	**Age 3**	**Age 4**	**Age 3**	**Age 4**
Teacher Survey/Teacher Child Report								
Head Start Group	89	90	86	67	83	78	84	NA
Control Group	61	65	80	67	83	79	87	NA
Classroom Observation								
Head Start Group	91	92	87	NA	NA	NA	NA	NA
Control Group	61	66	83	NA	NA	NA	NA	NA
Director Interview								
Head Start Group	88	89	80	NA	NA	NA	NA	NA
Control Group	74	67	75	NA	NA	NA	NA	NA
Weighted Instrument Response Rates by Wave								
	Spring 2003		**Spring 2004**		**Spring 2005**		**Spring 2006**	
Instrument	**Age 3**	**Age 4**	**Age 3**	**Age 4**	**Age 3**	**Age 4**	**Age 3**	**Age 4**
Teacher Survey/Teacher Child Report								
Head Start Group	88	90	87	64	82	78	86	NA
Control Group	64	70	79	68	84	81	88	NA
Classroom Observation								
Head Start Group	91	92	87	NA	NA	NA	NA	NA
Control Group	66	68	84	NA	NA	NA	NA	NA
Director Interview								
Head Start Group	86	91	78	NA	NA	NA	NA	NA
Control Group	81	73	73	NA	NA	NA	NA	NA

NA indicates not applicable.

Exhibit 2.12: Variables Correlated with Teacher Survey (TS), Teacher Child Report (TCR), Classroom Observation (CO), and Center Director Interview (DI) Nonresponse

Variable	Spring 2003			Spring 2004			Spring 2005	Spring 2006
	TS/TCR	CO	DI	TS/TCR	CO	DI	TS/TCR	TS/TCR
Head Start group/ control group	X	X	X	X	X	X	X	X
Child's race	X		X	X		X		
Child's language								
Home language	X	X						
Child's gender	X					X		
Child's age	X	X	X	X	X			
Program option applied for		X		X				
Metro status	X	X	X	X			X	
Level of pre-K services			X	X				
Head Start region	X	X		X	X			
State	X		X		X			
Crossover status	X		X		X	X		
No-show status								
Type of care setting	X	X	X	X				
PSU	X	X	X	X		X	X	X

NOTE: X identifies in which years CHAID identified a variable as correlated with nonresponse (prior to nonresponse adjustment).

These variables were used by CHAID to form nonresponse adjustment cells according to a tree-like structure. Note that once all the children are attending school in spring 2005, the significant predictors of nonresponse no longer include child characteristics, but only the location and metro status of the Head Start program applied for.

Exhibit 2.13 presents the unweighted response rates for the Teacher Survey (TS)/Teacher Child Report (TCR), Classroom Observations (CO), and Director Interview (DI), conditional on the Child Assessment and Parent Interview respondents. The response rates are calculated by characteristics of the child and Head Start program to which the child applied that were significant predictors of nonresponse, as identified by CHAID. Response rates are consistently lower for the control group than for the Head Start group, and higher for children in a center-based setting as opposed to a private home. Variation in response rates can also be seen across regions, metro status, race/ethnicity groups, program option applied for, the child's language and the language spoken at home. These variables were used to construct nonresponse adjustment

Exhibit 2.13: Unweighted Response Rates for Teacher Survey (TS), Teacher Child Report (TCR), Classroom Observation (CO) and Center Director Interview (DI), Conditional on Child Assessment and Parent Interview Respondents

	Spring 2003						Spring 2004						Spring 2005		Spring 2006	
	TS/TCR		CO		DI		TS/TCR		CO		DI		TS/TCR		TS/TCR	
Variables	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**
Child Cohort																
3 = Age three cohort	89	61	91	61	88	74	86	80	87	83	80	75	83	83	84	87
4 = Age four cohort	90	65	92	66	89	67	67	67	NA	NA	NA	NA	78	79	NA	NA
Child's Race																
1 = White	91	67	93	63	90	81	82	83	85	85	83	81	85	88	88	90
2 = Black	85	59	90	64	84	70	74	68	85	77	73	69	79	80	81	85
3 = Hispanic	91	64	91	64	91	65	75	70	83	76	80	68	79	77	86	86
4 = Other	94	57	97	62	94	71	87	87	91	82	89	74	82	87	80	93
Child's Language																
1 = English	88	61	91	62	87	71	79	76	86	81	79	75	82	83	84	87
2 = Spanish	92	66	92	67	92	66	74	68	82	73	78	63	78	75	85	85
3 = Other	84	79	97	80	95	84	79	78	79	86	79	59	86	96	84	90
Home Language																
1 = English	89	61	92	62	88	72	79	76	86	81	79	75	82	83	84	88
2 = Spanish	91	64	91	65	92	65	74	68	81	73	77	64	78	75	86	85
3 = Other	79	73	90	73	85	81	76	79	83	83	83	63	81	85	89	91
Child's Gender																
1 = Female	90	63	92	64	89	72	78	74	86	80	80	68	82	81	85	87
2 = Male	89	62	91	64	88	68	76	74	83	78	78	76	80	81	84	87
Program Option Applied For																
1 = Full day	88	62	92	66	89	71	78	72	86	79	79	72	82	83	85	87
2 = Part-day only	90	64	94	63	89	71	76	75	84	81	79	73	80	80	84	87
3 = Both Full and Part-day	77	56	77	56	80	56	67	59	76	68	64	61	81	68	77	74
4 = Other	87	51	73	49	90	66	81	75	80	58	85	69	79	81	90	92
Metro Status																
0 = nonMSA	94	73	93	69	85	84	85	83	87	85	86	82	80	84	87	88
1 = MSA	88	60	91	62	90	68	75	71	84	77	77	70	81	80	84	87

Exhibit 2.13: Unweighted Response Rates for Teacher Survey (TS), Teacher Child Report (TCR), Classroom Observation (CO) and Center Director Interview (DI), Conditional on Child Assessment and Parent Interview Respondents (continued)

	Spring 2003						Spring 2004						Spring 2005		Spring 2006	
	TS/TCR		CO		DI		TS/TCR		CO		DI		TS/TCR		TS/TCR	
Variables	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**
Level of State-funded Pre-K Services in State																
1 = State has programs similar to Head Start	79	61	83	64	81	67	70	67	79	72	70	56	75	76	80	85
2 = State has programs with some components of Head Start	86	51	95	59	83	67	80	78	86	87	76	78	82	84	90	93
3 = Remaining States	93	67	93	65	93	73	79	75	87	79	83	76	82	82	84	86
Head Start Region																
Northeast (1,2,3)	86	61	88	60	85	69	66	62	83	75	76	63	76	76	82	85
South (4,6)	88	58	94	64	86	65	81	77	87	84	79	76	81	84	84	88
North Central (5)	96	76	94	69	95	89	80	78	85	71	82	72	78	77	83	82
Plains (7,8)	83	23	88	25	94	43	77	77	80	63	73	100	86	78	100	100
West (9,10)	95	80	89	75	98	81	86	82	75	81	87	81	91	87	98	96
Crossover Status																
0 = Control child who did not enroll in Head Start	NA	58	NA	58	NA	64	NA	73	NA	77	NA	72	NA	82	NA	88
1 = Control child who did enroll in Head Start	NA	76	NA	80	NA	84	NA	76	NA	88	NA	73	NA	78	NA	84
No-show Status																
0 = Head Start group child who enrolled in Head Start	92	NA	94	NA	91	NA	79	NA	87	NA	81	NA	81	NA	85	NA
1 = Head Start group child who did not enroll in Head Start	58	NA	64	NA	61	NA	64	NA	69	NA	62	NA	78	NA	83	NA
Focal Care																
1 = Head Start, Other center	90	68	93	74	89	70	86	78	85	81	79	72	85	78	NA	NA
2 = Own Home w/Relative or Non-relative, Relative or Non-Relative's Home	51	46	21	23	NA	NA	26	36	28	22	NA	NA	NA	NA	NA	NA
3 = Parent Care	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA
4 = Attending School or Home-Schooled	NA	NA	NA	NA	NA	NA	67	70	NA	NA	NA	NA	81	81	84	87

Exhibit 2.13: Unweighted Response Rates for Teacher Survey (TS), Teacher Child Report (TCR), Classroom Observation (CO) and Center Director Interview (DI), Conditional on Child Assessment and Parent Interview Respondents (continued)

	Spring 2003						Spring 2004						Spring 2005		Spring 2006	
	TS/TCR		CO		DI		TS/TCR		CO		DI		TS/TCR		TS/TCR	
Variables	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**	**HS**	**C**
PSU																
102	98	90	97	83	99	93	97	97	83	88	92	88	96	97	96	100
108	95	94	81	84	99	96	88	83	73	81	98	86	95	91	98	95
112	83	23	88	25	94	43	77	77	80	63	73	100	86	78	100	100
115	91	55	92	57	95	56	69	66	67	70	42	56	76	71	100	88
127	92	53	97	46	87	30	86	83	92	89	84	79	87	86	98	96
206	98	38	96	33	100	50	84	70	93	76	100	75	88	94	88	84
208	98	81	97	75	100	91	63	71	89	85	92	85	62	55	58	63
210	98	100	91	81	89	100	91	95	83	73	92	85	79	80	92	95
224	94	78	97	88	94	93	84	57	75	71	43	50	100	96	100	100
241	92	68	94	64	93	93	74	67	84	50	78	50	76	64	80	57
311	83	58	94	64	90	83	81	75	75	79	75	78	84	93	80	87
323	98	78	98	78	98	81	89	88	96	91	92	85	85	85	93	92
330	98	69	100	74	84	100	94	91	97	97	88	97	82	85	97	94
338	78	30	94	42	72	53	77	71	85	83	81	80	93	92	96	100
356	74	43	84	69	85	71	61	49	88	74	84	39	54	72	66	72
358	70	24	85	32	56	25	50	55	79	88	32	44	67	70	83	90
367	91	65	93	71	96	72	81	73	88	75	75	65	76	77	63	61
368	98	76	97	77	97	84	96	94	88	89	95	94	92	94	81	94
380	89	73	94	85	94	70	80	67	80	68	80	73	81	85	80	79
406	81	70	82	70	91	81	74	85	84	84	53	42	78	84	85	90
417	53	63	65	57	67	57	56	60	76	71	64	35	89	81	83	89
421	99	68	96	59	80	88	96	97	95	95	96	100	96	96	95	98
423	66	49	73	56	76	56	59	56	70	60	68	59	78	69	69	76
427	90	43	92	46	82	58	63	53	91	75	91	75	71	76	86	100
502	95	81	95	68	98	89	33	33	76	65	67	49	48	48	67	58

HS indicates Head Start group. C indicates control group. NA indicates not applicable.

cells for the calculation of nonresponse adjustment factors, thus differences in response rates among groups of children are compensated for in the nonresponse-adjusted weights. The nonresponse adjustment reduces nonresponse bias when the outcomes are correlated with the variables used to create the nonresponse adjustment cells, by restoring the sample of responding children to the original representative distribution.

Longitudinal Weights

Two longitudinal weights were also calculated for each child for use in growth curve analysis with multi-level models (see Chapter 5). Children with two or more completed assessments in the time period from fall 2002 through spring 2006 were given a weight so they could represent the population of newly entering Head Start applicants in growth curve analysis requiring at least two time points per child. Two time points are the minimum for representing linear growth. Most students will have greater than two time points. Another weight was calculated for children who completed three or more assessments in the same time period for use in growth curve analysis requiring at least three time points per child. Three time points are the minimum for representing quadratic growth. Most students will have more than three time points so average growth can be adequately estimated. The variables identified by CHAID as correlated with response for the longitudinal weights were the Head Start indicator, no-show status (children assigned to Head Start but did not participate), crossover status (children assigned to the control group but who participated in Head Start anyway), program option applied for, level of pre-K programs in the state, urban status, Head Start region, and PSU. Of the original sample of 4,667 children, 87 percent completed two or more assessments, and 81 percent completed three or more assessments. Each longitudinal weight was created by adjusting the child's overall base weight for longitudinal nonresponse, poststratifiying the nonresponse-adjusted weight to the HSNRS (for the age four cohort), and trimming the poststratified weight for 1.5 percent of the respondents whose weight exceeded four times the average weight.

Exhibit 2.14 contains unweighted longitudinal response rates for children with two or more child assessments in the fall 2002-spring 2006 period, and for children with three or more assessments. It can be seen that attrition rates are higher for the control group and there is considerable variation across locations in attrition rates.

Exhibit 2.14: Unweighted Longitudinal Response Rates

Variables	Fall 2002 – Spring 2006			
	2 or More Assessments		3 or More Assessments	
	Head Start Group	Control Group	Head Start Group	Control Group
Child Cohort				
3 = Age three cohort	92	84	88	79
4 = Age four cohort	89	79	82	71
Child's Gender				
0 = Female	89	82	84	76
1 = Male	92	82	86	76
Mother's Education				
Less than high school (1,2)	91	91	84	87
High school/GED (3,4)	90	90	85	86
Voc Tech/Some College/Assoc Degree (5,6,7)	90	96	84	91
College Degree (7,8,9)	93	91	88	86
Program Option Applied For				
1 = Full Day	90	81	86	73
2 = Part-Day Only	91	82	85	77
3 = Both Full and Part day	84	84	71	78
4 = Other	91	88	80	78
Level of State-Funded Pre-K Services in State				
1 = State has programs similar to Head Start	88	84	82	78
2 = State has programs with some components of Head Start	91	80	87	73
3 = Remaining States	91	82	86	76
Head Start Region				
Northeast (1,2,3)	89	82	84	76
South (4,5,6)	91	82	87	75
North Central (5)	91	80	84	74
Plains (7,8)	89	85	80	75
West (9,10)	91	85	85	78
Crossover Status				
0 = Control group child who did not enroll in Head Start	NA	80	NA	74
1 = Control group child who did enroll in Head Start	NA	94	NA	88
No-Show Status				
0 = Head Start group child who enrolled in Head Start	96	NA	91	NA
1 = Head Start group child who did not enroll in Head Start	68	NA	60	NA

NA indicates not applicable.

Exhibit 2.14: Unweighted Longitudinal Response Rates (continued)

Variables	Fall 2002 – Spring 2006			
	2 or More Assessments		2 or More Assessments	
	Head Start Group	Control Group	Head Start Group	Control Group
Urban Location				
1 = Urban	88	79	82	72
2 = Suburban	92	84	87	78
3 = Rural	91	83	86	77
PSU				
102	93	86	87	82
108	92	85	86	78
112	89	85	80	75
115	88	83	80	74
127	95	89	91	82
206	97	85	87	85
208	88	67	85	65
210	91	91	83	84
224	93	87	91	77
241	89	67	81	58
311	90	82	86	76
323	92	80	88	74
330	91	75	87	65
338	91	83	89	76
356	88	79	77	77
358	93	81	89	76
367	93	83	88	78
368	95	85	90	81
380	83	78	79	67
406	91	80	85	73
417	76	78	70	69
421	94	86	93	85
423	82	82	73	76
427	90	70	85	61
502	97	91	92	90

Importance of Using Weights

The weights presented above play a critical role in ensuring that the sample is representative of newly enrolling 3- and 4-year-olds in Head Start. The formulas for producing weights are quite complex and can result in substantial differences in weights among sample children. If certain types of children tend to have much larger weights than other types of

children, and if the weights are not used in the analysis, then the types of children with large weights will be underrepresented in the analysis relative to the population of all newly entering Head Start children. This can lead to serious bias in impact estimates. Thus, we strongly recommend that weights be used in all analyses.

Calculating Correct Standard Errors

Estimates obtained from the Head Start Impact Study will differ from the true population parameters because they are based on a randomly chosen subset of the population, rather than on a complete census of all newly entering Head Start children. This type of error is known as sampling error or variance. The differences between the estimates and the true population values can also be caused by nonsampling error. Nonsampling errors can result from many causes, such as measurement error, nonresponse, sampling frame errors, respondent error, and differences among interviewers. In general, the magnitude of nonsampling error is difficult to assess from the sample. The precision of an estimate is measured by the standard error (defined as the square root of the variance due to sampling). The calculation of the standard error must reflect not only the sample size on which the estimate is based, but the manner in which the sample was drawn. Otherwise, the standard errors can be misleading and result in incorrect confidence intervals and p-values in hypothesis testing. The study's sampling involved stratification, clustering, and unequal probabilities of selection, all of which must be reflected in the standard error calculations.

Two commonly used variance estimation methods for complex surveys involving multi-stage sampling are replication and linearization (Wolter, 1985). Replication methods work by dividing the sample into subsample replicates that mirror the design of the sample. A weight is calculated for each replicate using the same procedures as for the full-sample weight. This produces a set of replicate weights for each sampled child. To calculate the standard error of a survey estimate, the estimate is first calculated for each replicate using the replicate weight and the same form of estimator as for the full sample. The variation among the replicates is then used to estimate the variance for the full sample estimate. In the linearization approach, a nonlinear estimator is approximated by a linear function and a formula derived for the variance of the linear approximation. Replication has the advantage that it can reflect the different features of the weighting and estimation by simply repeating all steps separately for each

replicate. For linearization, a specific formula is needed for each estimator, and the formula will differ depending on the type of estimator and sample design. On the other hand, finite population correction factors are often easier to account for using linearization estimators. However, for linear estimators, or nonlinear estimators that are formed by combinations of linear functions, replication variance estimators are often little different numerically from linearization variance estimators.

For the current study, a set of 76 jackknife replicate weights was created for each child for use in the calculation of all standard errors. Normally, stratified jackknife replicate weights are created by dropping out one PSU at a time, setting the replicate weights for sampled units in the dropped PSU to zero, multiplying the full-sample weights of sampled units in the remaining PSUs in the stratum by a factor of $n_h / (n_h-1)$, where n_h is the number of PSUs in the h-th stratum, and leaving the full-sample weights for sampled units in the remaining strata unchanged. However, because only 25 PSUs were sampled at the first stage (one per stratum), only 27 replicate weights could be created (in the one certainty PSU, two additional replicates could be formed by forming two "pseudo-PSUs" based on program groups). To improve the stability of the variance estimates, the second-stage sampling units, namely Head Start program groups, were used as the "drop unit" in creating replicates. This resulted in 76 replicate weights per child and 51 degrees of freedom for variance estimation (i.e., 76 PSUs – 25 strata). Because the between-PSU component of variance is ignored in doing this, the resulting variance estimates will be slight underestimates if the between-PSU variability is small relative to the within-PSU variability. The validity of this hypothesis was investigated by creating a second set of 27 replicate weights based on the 25 PSUs, which includes the between-PSU component, but has fewer degrees of freedom. By calculating the average ratio of the variance from the set of replicate weights based on the 25 PSUs to the variance from the set based on the 76 program groups, we were able to estimate the relative size of the between-PSU component. The ratio of variances was calculated for several child assessment means (PPVT, Elision, Woodcock-Johnson Applied, Oral Comprehension, Spelling, and Letter-Word) by age and gender within the combined test language groups English and Spanish, then averaged across tests. For fall 2002 scores, the between-PSU component was estimated to be 15 percent of the total variance, and for spring 2003 scores, this component was estimated to be 28 percent of the total variance. Thus, the standard error estimates for means produced from the set of 76 replicate weights may be too

small. However, for estimates of differences between Head Start group and control group means, the between-PSU component of variance is expected to be very small because of the high correlation between PSU level estimates, since the Head Start and control groups come from the same PSUs. Formally stated, $\sigma^2_{PSU}(\bar{y}_T - \bar{y}_C) = \sigma^2_T + \sigma^2_C - 2\rho\sigma_T\sigma_C$ so that as ρ approaches one, the between-PSU component of the total variance, σ^2_{PSU}, approaches zero. Therefore, the standard error estimates for differences between Head Start group and control group means using the 76 replicate weights can be expected to differ from the total variance by less than 15 percent. For spring 2003, the between-PSU component of variance for differences between Head Start group and control group means was estimated to be less than seven percent of the total variance for seven child assessment outcomes.

Estimation for Puerto Rico as a separate analysis domain is problematic due to small sample sizes. As it turns out, there were three Head Start programs sampled in Puerto Rico, 22 centers, and 180 to 190 children (roughly equally split between age three and four) with completed assessments and a parent interview each spring. From this sample, it is generally possible to produce estimates of unadjusted mean impacts for assessment scores and parent interview outcomes. These unadjusted impact estimates are simple differences in means between the Head Start and control groups. However, the sample sizes in Puerto Rico are too small to permit adjusted estimates from a regression model containing child covariates to be made. Variance estimation for Puerto Rico as a separate analysis domain is especially problematic because only three programs were sampled, leaving insufficient degrees of freedom to estimate standard errors. (The degrees of freedom are based on the number of first-stage sampling units, not the number of sampled children.) A special set of 22 jackknife replicate weights was created for children in Puerto Rico to permit variance estimation for child assessment and parent interview outcomes by treating the 22 centers as the first-stage sampling units and the three programs as strata. The special replicate weights provide 19 degrees of freedom for variance estimation (22-3=19). While this is still quite small, calculation of standard errors becomes at least feasible. However, these standard error estimates omit the between-program component of variance and may be underestimates of the true standard error. They are also likely to be quite unstable, especially for the age three cohort, where the number of centers in Puerto Rico was reduced from 22 to just the 16 that admitted three-year-olds; consequently the

number of replicates is reduced from 22 to 16, with only 13 degrees of freedom available for variance estimation. Therefore, caution should be used in interpreting results of hypothesis testing based on these estimates.

Another issue arising in the calculation of standard errors is the application of the finite population correction factor (fpc), where the fpc is defined as one minus the sampling fraction. For multistage designs, the application of the fpc in jackknife replication will cause underestimation of the variance. On the other hand, ignoring the fpc will lead to a slight overestimation of the standard errors, which is a generally accepted practice. In the current study, the average sampling fraction for sampling PSUs was about 0.2, while the average sampling fraction for sampling program groups within PSUs was about 0.4. However, as discussed above, standard errors for impact estimates are already slight underestimates due to the omission of the between-PSU component of variance in setting up the replicate weights; therefore, it did not seem advisable to incorporate an fpc, which would have increased the negative bias in the standard error estimates. In any case, the SUDAAN software we used (see below) does not allow for incorporation of an fpc with replication methods.

Incorporating Weights and Standard Errors in the Impact Analyses

The easiest way for analysts to incorporate the weights and correct standard errors into their analyses is to use software designed for analysis of complex survey data. Such software packages include WesVar, SUDAAN, Stata, and the new survey procedures (proc surveymeans, proc surveyfreq, proc surveyreg, proc surveylogistic) in SAS version 9. Most estimation and modeling can be done with one of these packages. WesVar uses replication methods (jackknife, Balanced Repeated Replication (BRR)), and SAS version 9 uses linearization. SUDAAN and Stata offer both linearization and replication.

All analyses in this report were done using SUDAAN version 9 with jackknife replication, with the exception of the subgroup regression-adjusted impact estimates for binary outcomes and multi-level modeling. Due to a bug in the SUDAAN software pertaining to the calculation of predicted marginals for subgroups, the regression-adjusted subgroup impact estimates for binary outcomes and their standard errors were calculated using an in-house SAS program with jackknife replication, following the formula for subgroup-predicted marginals in Graubard & Korn (1999) (see Chapter 5). Hypothesis testing of differences in the regression-

adjusted subgroup estimates for binary outcomes also was conducted using the in-house SAS program. For multi-level modeling with survey weights, we used the software package HLM version 6 because it uses the Pfeffermann (see Pfeffermann, et al., 1998) method of handling survey weights. A recently available alternative that we did not use but that also uses the Pfeffermann method of applying survey weights in multilevel models is GLLAMM, a user-written Stata procedure (Rabe-Hesketh Skrondal, 2005). Both HLM and GLLAMM use model-based variance component estimation assuming a super population model.

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Chapter 3: Outcome Measurement and Psychometrics

Introduction

In this study, child outcomes provide measures of how well Head Start and non-Head Start preschool programs, or other child care, are achieving the goal of assisting children to be physically, socially, and educationally ready for success in school. This study used direct child assessments, as well as parent and teacher assessments of children's skills and achievement. The direct child assessment battery in the Head Start Impact Study focused on language and literacy including vocabulary, reading and writing skills, oral comprehension and phonological awareness, as well as math skills. The 45-60 minutes child assessment battery was typically administered one-on-one by specially trained assessors in the child's main care setting during the preschool years (i.e., where the child spent the most time Monday through Friday between the hours of 9 am and 3 pm) and in the child's home during the kindergarten and 1st grade years.

This chapter provides detailed information regarding the cognitive assessments utilized in this study, as well as psychometric and ICC (intraclass correlations) information on all domains measured. The chapter provides information on: (1) discussion of the treatment of non-English speaking children; (2) description of the various assessments used throughout the period of the Head Start Impact Study (fall 2002 through spring 2006); (3) discussion of certain test adaptations that were implemented to reduce the burden of testing on individual children; (4) review of IRT scoring used for the PPVT, TVIP, and CTOPPP tests; (5) a review of scoring procedures used for the few non-standardized tests that were included in the test battery; (6) description of composite outcome measures that used combinations of selected direct assessment scales; (7) description of socio-emotional, parenting, and health outcomes; and (8) psychometric and ICC information for the all outcome measures.

Language of Assessment

At the time of the baseline assessments in fall 2002, the assessor asked the main care provider (i.e., the teacher or other care provider if the child was in child care or the parent if the

child was not in child care),[6] the following questions (i.e., the Language Decision Form) to determine the appropriate language of assessment:

- What language does the child speak most often at home (English, Spanish, or other specified language)?
- What language does the child speak most often at this child care setting (English, Spanish, or other specified language)?
- What language does it appear this child prefers to speak (English, Spanish, or other specified language)?

If two or more of the three responses to the above questions were English or Spanish, the child was tested in that language. For children requiring assessment in Spanish, the assessor administered a bilingual child assessment that included the complete fall 2002 Spanish assessment battery and two English tests (the Peabody Picture Vocabulary Test (PPVT) and the Woodcock-Johnson III Tests of Achievement Letter-Word Identification test). In spring 2003 and in subsequent data collection periods, the bilingual assessment included the complete English assessment battery and two Spanish tests (the Test de Vocabulario en Imágenes Peabody (TVIP) and the Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada Identificación de letras y palabras test). One exception was Puerto Rico where, because all instruction is in Spanish, children were assessed with the complete Spanish assessment battery at each data collection point.

In fall 2002, if the responses to the Language Decision Form indicated that the child's primary language was other than English or Spanish (e.g., Creole, Vietnamese, Mandarin, Arabic, etc.), the assessor asked the child's teacher or main care provider if the child could understand and answer questions in English. If yes, the child was assessed using the English assessment battery. If no, and the assessor was fluent in the child's language, the assessor translated the directions on four tests (McCarthy Draw-A-Design, Color Names and Counting, Leiter-R-adapted, and Story and Print Concepts), and administered those tests to the child. When the assessor was not fluent in the child's language, the assessor would arrange for a local translator to administer the four tests. For all cases, assessors or translators were available who were fluent in the child's language. It should be noted that very few children (N=54) were tested

[6] The correlation between parent reported child language and the language selected using the Language Decision Form was high (95% for English, 97% for Spanish, and 87% for other languages).

in a language other than English or Spanish. A majority of the "other language" children were tested in Creole or Mandarin. Assessors fluent in these languages were hired and trained to administer the assessments.

Four tests (McCarthy Draw-A-Design, Color Names and Counting, Leiter-R-adapted, and Story and Print Concepts) were selected for translation because (1) the administration of each test required limited verbal interaction between the child and the assessor, (2) the translations were not complex and for the most part, required the translation of simple directions (e.g., Point to the colored bears that you know and tell me what color they are"), and (3) national norms were not reported for the study children on these tests. The McCarthy Draw-A-Design test requires the child to copy simple designs while the Leiter-R-Adapted, a non-verbal test, requires the child to find and mark matching images. If a translator was needed, the translator provided directions for the test and the assessor scored the tests based on the child's response. The Color Names and Counting test requires the child to identify colors by name and to count 10 bears while the Story and Print Concepts test measures the child's familiarity with books and understanding of print. For these tests, the translator provided directions to the child in the child's language and then provided the assessor with the child's response in English. In spring 2003, and in subsequent data collection periods, these initially non-English speaking children were all tested using the complete English assessment battery.

Description of Tests

A variety of tests were included in the child assessment battery to measure the cognitive domains of reading, writing, vocabulary, oral comprehension, phonological awareness, and math skills. The battery consists of both standardized tests developed by recognized test publishing companies and non-standardized tests developed for use in other early childhood studies (e.g., the Head Start Family and Child Experiences Study (FACES)). As the children developed, new tests were added to the child assessment battery or existing tests were extended to include more difficult items. Preschool level tests were dropped as the children entered school. Exhibit 3.1 provides the list of tests used in the Head Start Impact Study for the combined sample (i.e., all study children other than those in Puerto Rico, each of whom was administered the English or bilingual child assessment battery) and the time when each test was administered to each cohort. Exhibit 3.2 provides the list of tests used in the Spanish child assessment battery for children in

Exhibit 3.1: Direct Child Assessment Measures by Cohort and Year for the Combined Sample

Test	Cohort	Fall 2002	Spring 2003	Spring 2004	Spring 2005	Spring 2006
Color Names and Counting*	3	X	X	X		
	4	X	X			
McCarthy Draw-A-Design*	3	X	X	X		
	4	X	X			
Story and Print Concepts*	3	X	X	X		
	4	X	X			
Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP) Print Awareness	3	X	X	X		
	4	X	X			
Leiter*	3	X	X	X	X	
	4	X	X	X		
Letter Naming	3		X	X	X	
	4		X	X		
Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP) Elision	3	X	X	X	X	
	4	X	X	X		
Peabody Picture Vocabulary Test (PPVT)	3	X	X	X	X	X
	4	X	X	X	X	
WJ III Letter-Word Identification	3	X	X	X	X	X
	4	X	X	X	X	
WJ III Spelling	3	X	X	X	X	X
	4	X	X	X	X	
WJ III Oral Comprehension	3	X	X	X	X	X
	4	X	X	X	X	
WJ III Applied Problems	3	X	X	X	X	X
	4	X	X	X	X	
Writing Name Task	3				X	
	4			X		
WJ III Word Attack	3				X	X
	4			X	X	
WJ III Quantitative Concepts	3				X	X
	4			X	X	
WJ III Calculation	3					X
	4				X	
WJ III Passage Comprehension	3					X
	4				X	
WJ III Writing Samples	3					X
	4				X	
Test de Vocabulario en Imágenes Peabody**	3	X	X	X	X	X
	4	X	X	X	X	
Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada Identificación de letras y palabras**	3	X	X	X	X	X
	4	X	X	X	X	

* Indicates the four tests administered to the children who spoke neither English nor Spanish in fall 2002.

** Indicates tests that are only included in the Bilingual Child Assessment Battery (see note below).

Note: In fall 2002, the bilingual Child Assessment included the following tests: PPVT, WJ III Letter-Word Identification, TVIP, CTOPPP Print Awareness (Spanish), CTOPPP Elision (Spanish), McCarthy Draw-A-Design (Spanish), Color Names and Counting (Spanish), Leiter (Spanish), Story and Print Concepts (Spanish), Batería Woodcock-Muñoz Identificación de letras y palabras, Batería Woodcock-Muñoz Problemas aplicados, and Batería Woodcock-Muñoz Dictado.

Exhibit 3.2: Direct Child Assessment Measures by Cohort and Year for the Spanish Sample in Puerto Rico

Test	Cohort	Fall 2002	Spring 2003	Spring 2004	Spring 2005	Spring 2006
Color Names and Counting (Spanish)	3	X	X	X		
	4	X	X			
McCarthy Draw-A-Design (Spanish)	3	X	X	X		
	4	X	X			
Story and Print Concepts (Spanish)	3	X	X	X		
	4	X	X			
Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP) Print Awareness (Spanish)	3	X	X	X		
	4	X	X			
Leiter (Spanish)	3	X	X	X	X	
	4	X	X	x		
Letter Naming (Spanish)	3		X	X	X	
	4		X	X		
Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP) Print Awareness (Spanish)	3	X	X	X	X	
	4	X	X	X		
Test de Vocabulario en Imágenes Peabody	3	X	X	X	X	X
	4	X	X	X	X	
Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada Identificación de letras y palabras	3	X	X	X	X	X
	4	X	X	X	X	
Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada Dictado	3	X	X	X	X	X
	4	X	X	X	X	
Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada Problemas aplicados	3	X	X	X	X	X
	4	X	X	X	X	
Writing Simple: Writing Name (Spanish)	3				X	
	4			X		

Puerto Rico and the time when each test was administered to each cohort. Each test is briefly described below:

- ***Peabody Picture Vocabulary Test (PPVT) Third Edition.*** The PPVT measures receptive vocabulary, i.e., listening comprehension for the spoken word in standard English. The child is instructed to look at four pictures and point to the picture that best represents the meaning of the stimulus word presented orally by the assessor. (Published reliability = 0.95). The *Test de Vocabulario en Imágenes Peabody (TVIP)* was used with the Spanish-speaking children (Published reliability = 0.93). For the Head Start Impact Study, an adaptive, shorter version of the PPVT and the TVIP were developed using Item Response Theory (IRT). (See IRT Development and Scoring later in this chapter.)
- ***Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP), Print Awareness Subtest.*** The CTOPPP Print Awareness, adapted for the Head Start Impact Study, measures the recognition of letter symbols and sounds. The four letter discrimination items and four letter-sound identification items were included in the adapted Print Awareness subtest. The child is asked by the assessor to point to a

letter (letter discrimination) and to point to the letter that represents the stimulus sound provided orally by the assessor (letter-sound identification). Print Awareness also measures print concepts, word discrimination, letter-name identification, letter-name identification free response, and letter-sound identification free response. These concepts were eliminated due to overlap with other tests in the child assessment battery. No published reliability is available. The instrument was translated for the Spanish version. The subtest was dropped from the analysis due to poor psychometric properties and difficulty in interpretation of the small number of items.

- ***Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP), Elision Subtest.*** The CTOPPP Elision measures the ability to remove words, syllables, and sub syllables as part of words or compound words. Both multiple choice and free-response items are included in the subtest. The child is asked to respond by pointing to pictures and to respond verbally to the assessor's oral directions to make a new word out of words provided (e.g., Say seesaw without see). No published reliability is available. The instrument was translated for the Spanish version.
- ***McCarthy Scales of Children's Abilities, Draw-A-Design Task.*** The Draw-A-Design task is a measure of perceptual motor skills. The child is asked to draw a series of increasingly complex figures. The published reliability for the Perceptual-Performance subscale, of which the Draw-A-Design is one component, is 0.84. The task was translated into Spanish for use in the Family and Child Experiences Survey (FACES) for the 1997 cohort and also used in the Head Start Impact Study.
- ***Color Names and Counting.*** This was a subtest from the *CAP (Comprehensive Assessment Program) Early Childhood Diagnostic Instrument* used by FACES and developed by Marie Clay (1979), William Teale (1988), and Mason and Stewart (1989) as a battery of emergent literacy and school readiness measures. The subtest measures color recognition (Color Names), and early numeracy skills of counting, and one-to-one correspondence (Counting). The child is asked to identify 10 colors by name and to count 10 pictures of bears and arrive at the correct sum. No published reliability is available. This test was translated into Spanish for use in FACES and also used in the Head Start Impact Study.
- ***Leiter Revised, Sustained Attention Task.*** This task measures the child's ability to pay sustained attention to a repetitive task and to pay attention to detail. This is a timed test with a targeted picture at the top of each page. The child is asked to cross as many of the target pictures as possible during the allotted time. The targeted pictures are interspersed among non-target pictures. The Attention Sustained task is one of 10 tasks in the Attention and Memory battery. The overall published reliability is 0.83. The task is a nonverbal task but the directions were translated into Spanish for use in FACES and also used in the Head Start Impact Study. Each task is the same but the targeted pictures become more complex (e.g., from a stick man to a flower) and the matching target and non-target pictures are smaller and more diverse (i.e., more choices, rotated pictures, etc.). During the pilot test, the entire task was administered to the children. It was determined that the task was too long in that the children lost interest early in the task. To lessen the burden, it was decided to use

only one practice item and one test item for each age cohort. This limitation made it difficult to interpret the scores and thus the Leiter was eliminated from the analysis.

- ***Story and Print Concepts.*** The Story and Print Concepts task, used in FACES, was based on earlier prereading assessment procedures developed by Marie Clay (1979), William Teale (1988), and Mason and Stewart (1989). This test measures emerging literacy relative to knowledge of books and print concepts. For this test, the assessor asks questions relative to print concepts, such as "Show me the front of the book" and reads passages from a book to the child asking questions as the passage is read, such as "Where do I go next to read?" No published reliability is available. This test was translated into Spanish for use in FACES and also used in the Head Start Impact Study. The books used to assess the child's story and print concepts were as follows:
  - Alborough, J. (1992). *Where's My Teddy?* Cambridge, MA: Candlewick Press. (English version)
  - Alborough, J. (1995). *¿Dónde Está Mi Osito?* (translated by M. Castro) Miami, FL: Santillana USA Publishing Company, Inc. (Spanish version)

  To reduce the burden on the child, it was decided to reduce the number of questions from the FACES version. This test was eliminated from the analysis due to the difficulty in scoring the test and interpreting the results.
- ***Letter Naming.*** This task was modified by the FACES Research Team from a test used in the Head Start Quality Research Center's (QRC) curricular intervention studies. The Letter Naming task measures the child's ability to recognize the upper case letters of the alphabet. The letters of the alphabet are divided into three plates with the easiest letters printed on the first plate. Children are asked to identify each letter on the plate. No published reliability is available. This task was translated into Spanish for use in the Head Start Impact Study. Although this task was administered in English to the bilingual children, responses in English or Spanish were acceptable.
- ***Writing Name task.*** This task was modeled after the Name Writing tasks in *The CAP Early Childhood Diagnostic Instrument (*Mason and Stewart, 1989) and the Writing Samples test in the *Woodcock-Johnson III Tests of Achievement* (2001). The task measures the child's basic writing skills. For this task, the child is asked to write his or her name. No published reliability data is available. This task was translated into Spanish for use in the Head Start Impact Study. Although this test was administered to children at the end of kindergarten, 98 percent of the children could write their name, so the data was not included in the analysis.
- ***Woodcock-Johnson III Tests of Achievement, Letter-Word Identification.*** The Letter-Word Identification test measures letter and word identification skills. The items measure a child's reading identification skills in identifying letters and words as they appear in the test easel. The published median reliability is 0.91 in the 5 to 19 age range. The *Batería-R Woodcock-Muñoz Pruebas de aprovechamiento-Revisada* Identificación de letras y palabras test is used for the Spanish and bilingual test administration.

- ***Woodcock-Johnson III Tests of Achievement, Spelling.*** The Spelling test measures the child's ability to correctly write orally presented letters and words. For the initial items, pre-writing skills are measured through tasks such as drawing lines and copying letters. As the items progress in difficulty, the child is asked to write specific upper and lower cases of the alphabet and specific words. The published median reliability is 0.90 in the 5-19 age range. The *Batería-R Woodcock-Muñoz Pruebas de aprovechamiento-Revisada* Dictado is used for the Spanish test administration.
- ***Woodcock-Johnson III Tests of Achievement, Applied Problems.*** This test measures the child's ability to analyze and solve practical math problems. In order to solve the problems that are read by the assessor to the child, the child must recognize the procedure to be followed and then count and/or perform simple calculations. The published median reliability is 0.92 in the 5-19 age range. The *Batería-R Woodcock-Muñoz Pruebas de aprovechamiento-Revisada* Problemas aplicados is used for the Spanish test administration.
- ***Woodcock-Johnson III Tests of Achievement, Oral Comprehension.*** This test measures the child's ability to comprehend a short spoken passage and to provide a missing word based on syntactic and semantic clues. The test requires the child to use listening, reasoning and vocabulary skills. The assessor reads an analogy or passage with one word missing, the child is asked to respond orally with the correct word that completes the passage or analogy. The published median reliability is 0.80 in the 5-19 age range. No Oral Comprehension test was administered in Spanish.
- ***Woodcock-Johnson III Tests of Achievement, Word Attack.*** This test measures the child's ability to apply phonic and structural analysis skills to the pronunciation of printed nonwords. The initial items require the child to produce the sounds for a single letter. The remaining items require the child to read aloud nonwords that become increasingly more difficult. The published median reliability is 0.87 in the 5-19 age range. No Word Attack test was administered in Spanish.
- ***Woodcock-Johnson III Tests of Achievement, Quantitative Concepts.*** This test consists of two subtests: Concepts and Number Series. Concepts measures the child's understanding of counting, identifying numbers, shapes and sequences, and knowledge of mathematical terms and formulas. Number Series measures the child's ability to look at a series of numbers, determine the pattern, and provide the missing number in the series. The published median reliability is 0.90 in the 5-19 age range. No Quantitative Concepts test was administered in Spanish.
- ***Woodcock-Johnson III Tests of Achievement, Calculation.*** This test measures the ability to perform mathematical computations. The initial items require the child to write single numbers. The items progress in difficulty from basic operations to geometric, trigonometric, logarithmic, and calculus operations. The calculations include operations with whole numbers, percents, fractions, decimals and negative numbers. The published median reliability is 0.85 in the 5-19 age range. No Calculation test was administered in Spanish.

- ***Woodcock-Johnson III Tests of Achievement, Passage Comprehension.*** This test measures the child's ability to match a pictographic representation of a word (rebus) with the actual picture of the object and to read a short passage and identify a missing key word based on the passage context. The items become more difficult by removing pictures and increasing passage length, level of vocabulary, and the complexity of semantic and syntactic clues. The published median reliability is 0.83 in the 5-19 age range. No Passage Comprehension test was administered in Spanish.
- ***Woodcock-Johnson III Tests of Achievement, Writing Samples.*** This test measures the child's ability to respond in writing to requests such as completing written passages or writing responses to pictures. The child is asked to respond to simple tasks such as completing the sentence, "My name is _____" to more complex tasks such as writing a sentence to describe a picture (e.g., picture of a bird in a cage singing). The published median reliability is 0.84 in the 5-19 age range. No Writing Samples test was administered in Spanish.

Test Adaptations

Three types of adjustments were made to several tests to significantly reduce the time required to test individual children (i.e., reducing the burden on the child): (1) adapted or shortened versions of the PPVT and TVIP were created using item response theory (IRT; described below); (2) the stopping rules for the Woodcock-Johnson III Tests of Achievement were changed from six consecutive incorrect responses used in the standard administration of these tests to three consecutive incorrect responses for determining the ceiling; and (3) only selected sections or scales were administered for some tests. The first adaptation using IRT procedures is described in the next section; the remaining adaptations are discussed below.

The stopping rule instructs the assessor when to stop the test because the items have become too difficult for the child and thus the ceiling is established. The basal items are the easiest items to be administered while the ceiling items are the most difficult items to be administered in order to measure the child's ability in a given cognitive area or construct. This rule (i.e., three consecutive incorrect responses) was implemented in the FACES child assessment battery as a means to reduce the time burden on young children with short attention spans and to reduce the frustration that occurs when a child is asked to answer many difficult items. This stopping rule was retained in the Head Start Impact Study for comparison purposes. Changing the stopping rules may result in slightly lower scores when comparing the scores to normed scores but the same procedures were implemented for both the Head Start and control group and should not affect the Head Start and control group differences. Due to normal

cognitive growth and increased attention spans, the standard administration (six consecutive incorrect responses) was implemented in the first-grade data collection period.

For the Leiter, McCarthy Draw-A-Design and Print Awareness, only selected items, sections or scales were used to avoid overlap with other tests and to reduce the burden on the child. The Leiter Attention Sustained Task is one of 10 tasks in the Leiter Attention and Memory Battery, only one age-appropriate teaching plate and one age-appropriate testing plate (out of four plates) from the Attention Sustained Task were administered to each child. The McCarthy Scales of Children's Abilities consists of 18 subtests organized into six scales (i.e., verbal, perceptual performance, quantitative, general cognitive, memory, and motor). The Draw-A-Design task is one component of the perceptual performance subscale from the McCarthy Scales of Children's Abilities and was the scale used in FACES. The CTOPPP Print Awareness test measures print concepts, letter discrimination, word discrimination, letter-name identification, and letter-sound identification, only the letter discrimination and letter sound identification items were included in the child assessment battery.

At the end of each data collection period, the item response patterns were reviewed for consistency in the administration of stopping rules, ceiling effects, and patterns of non-response. If problems were identified, the raw data was reviewed for coding or data entry errors. No cases were eliminated due to such data problems.

IRT Development and Scoring

Introduction

Shortened versions of the PPVT and the TVIP were developed to reduce the testing burden imposed on the young study children, building on work from the FACES study using a statistical procedure called maximum likelihood Item Response Theory (IRT).[7] IRT has gained increasing attention in the development of standardized academic tests, particularly when there is an interest in equating and comparing tests. Some of the issues faced by test developers, and the solutions that can be addressed through IRT modeling, include the following:

[7] There are many books and articles available on the theory and application of IRT. A good reference is: Hambleton, R.K., Swaminathan, H. & Rogers, H.J. (1991). *Fundamentals of Item Response Theory.* Newbury Park, CA: Sage Press.

Concern	IRT Approach to a Solution
Create tests with developmentally appropriate, subject-area coverage	Create parallel short form editions of nationally-normed publisher tests. (See below for more detail on the creation of the short form.) These forms include items of appropriate difficulty for the children in the study. Also, the shortened test is appropriate for younger children who are more easily fatigued by testing than older children.
Reporting test results in a scale that is comparable across tests.	Equate short forms to full-length publisher test.
Assure fairness by assessing items for bias against language and other minority groups.	Assess differential item- and test-functioning. That is, determine if the item (or test) is more difficult for minority children compared with majority children who are at the same level of proficiency.

The basic idea of IRT is to model a relationship between a hypothesized underlying trait or construct − which is unobserved − and an individual's responses to a set of items on a test (e.g., assessing a child's reading and math ability). The results of the IRT analysis can be used to determine the extent to which the items included in the test are "good" measures of the underlying construct, and how well the items "hang together" (show common relationships) to characterize the underlying, and unobserved, construct.

In IRT models, the underlying trait or construct of interest (e.g., an individual's reading ability) is designated by theta (θ) − individuals with higher levels of θ have a higher probability of getting a particular test item correct than do individuals with lower levels of θ. The modeled relationship between θ and the individual test items is typically based on 2-parameter logistic function: (1) the first parameter is the item difficulty, or "b," which captures individual differences in their ability to get an item correct; and (2) the second parameter is the slope, or discrimination, parameter "a" which captures how well a particular item differentiates between individuals on the underlying construct or trait.[8] This parameter indicates how strongly individuals with different levels of ability perform on the item (e.g., do nearly all children with high ability get the item correct, while those with lower ability mostly get it wrong). In other words, the IRT model estimates the probability of getting a particular item correct on a test

[8] A 3-parameter model, actually used in the Head Start Impact Study, adds a consideration of possible child guessing.

conditional on their underlying trait level, e.g., the higher a person's ability level, the greater the probability that the person will provide a correct answer to a particular item.

More traditional methods of creating scales often involve just counts of individual item-level responses, an approach that assumes that each item is equally related to the underlying trait. IRT, on the other hand, uses all of the available information contained in an individual's responses and uses the difficulty and discrimination parameters to estimate an individual's test or scale score. As a result, two individuals can have the same summed score (e.g., the same number of correct test items) but they may have very different IRT scores if they had a different pattern of responses. For example, in a test of academic ability one child might answer more of the highly discriminating and difficult items than another child and would receive a higher IRT-derived score than another child who correctly answered the same number of items but scored correctly on items with lower difficulty.

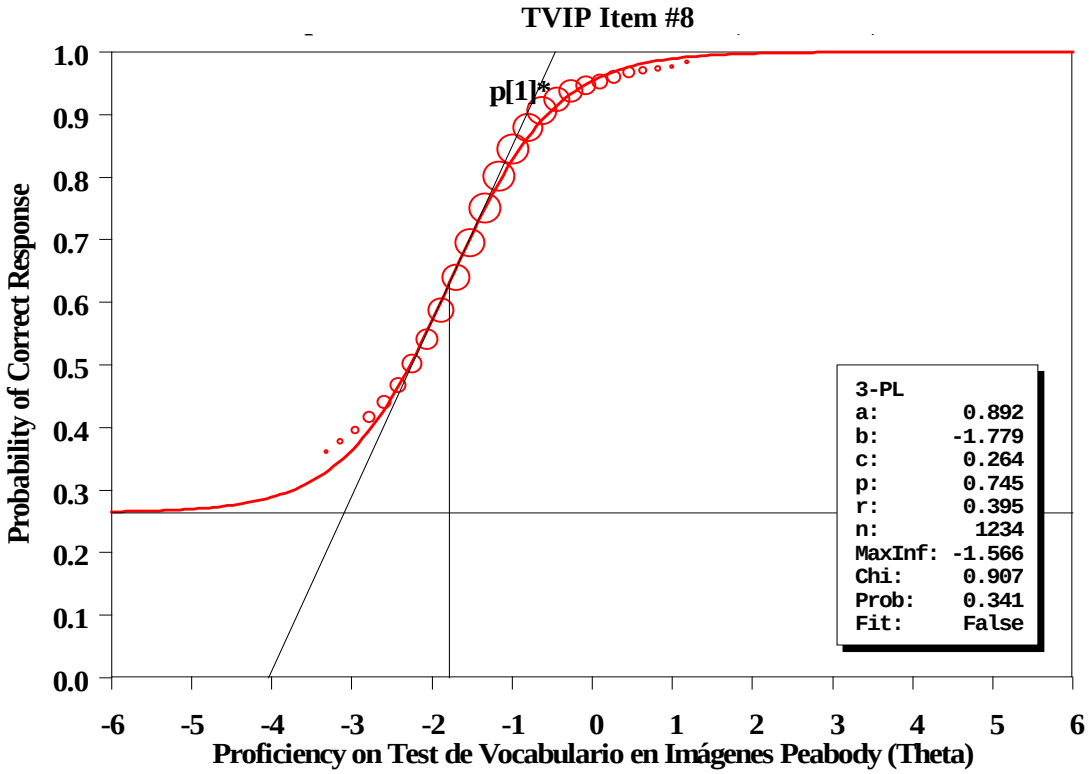
Another important advantage of IRT models is that it can produce reliable scale estimates even when an individual doesn't respond to all items, i.e., the model yields a valid estimate of the individual's score even when there is a moderate amount of missing data.

IRT Details

In item response theory, we begin by characterizing how people respond to a particular test item. In the simplest case of a dichotomous item, i.e., one that is scored right or wrong, we estimate the probability of getting the item right for each level of ability. This is the "item response" part of IRT. Exhibit 3.3 provides an example of an Item Response Curve (IRC) representing the probability of a correct response across all levels of ability.

In this figure, an individual's proficiency or ability is along the x axis (in a standardized scale) and the probability of getting the item correct is shown on the y axis. In this example, we see that children of low ability on the left have a small probability of getting item #8 correct while those of high ability are almost certain to get it correct. Notice that the lowest probability of getting the item right is about 0.25, not zero. This is because, for a multiple choice item of four categories a child has a one in four chance of getting the item right simply by guessing. The diagonal line represents the "slope" of the item response function. It indicates to what extent the

Exhibit 3.3: Item Response Curve for TVIP Item # 8: A Multiple-Choice Item Scored Right/Wrong

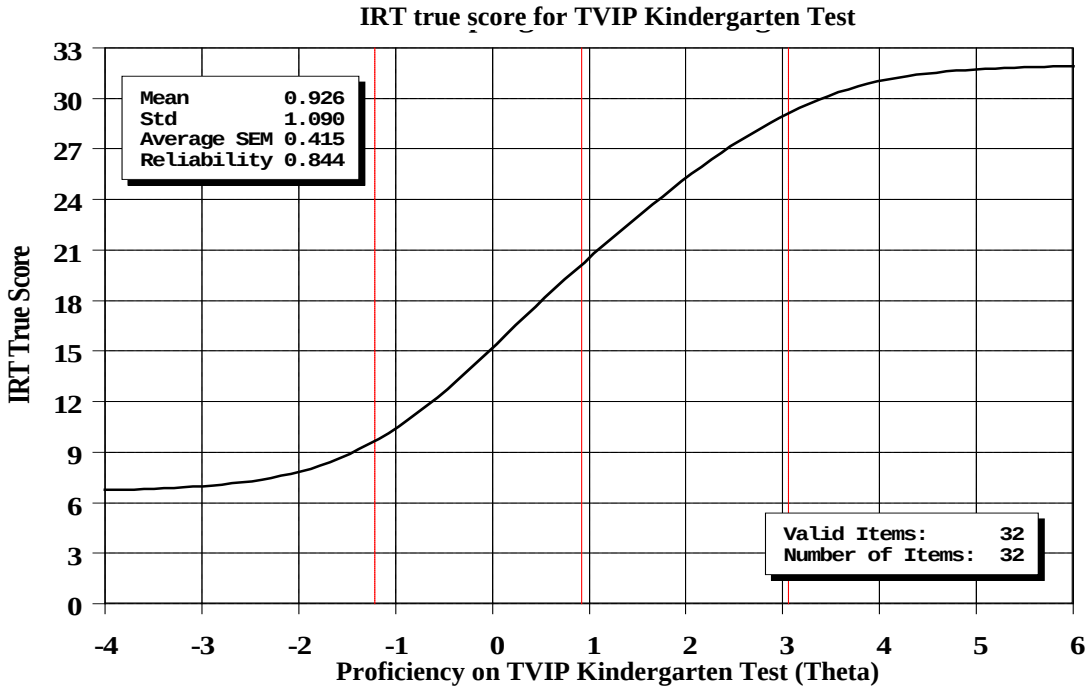


item discriminates among low and high ability children. For an item with a very steep slope, almost none of the low ability children will get it correct and almost all of the high ability children will get it correct.

A similar item response curve is estimated for each item in a test. For tests that have the same ability (theta) scale, the item response function for an item can be the same for different populations and testing times. When this measurement invariance holds it provides a way to compare test results across sample and time even when some different items are used for different tests.

Given an estimate of a person's ability, we can add up the probabilities of getting all of the items correct on a test. This is the estimated true score for that individual. If we graph each person's ability against their estimated true score we get the test response function (an example is shown in Exhibit 3.4).

Exhibit 3.4: IRT True-Score for the 32-Item TVIP Kindergarten Test



This figure shows that the test characteristic curve rises 19 raw score points over the ability range {-1.212 < Theta < 3.063} containing 95% of the expected target population. The test characteristic curve provides a way of equating different tests. If we estimate item response functions separately for two samples we will get ability estimates that are on a different metric in each sample. These metrics are not directly comparable. This is illustrated by Exhibit 3.5. To remedy this situation we equate the tests, i.e., we calculate coefficients for a linear transformation of origin (intercept) and of scale (slope) that will place the tests on a reference scale metric. In practice this means that we apply a linear transformation to each person's ability score in one sample so that the difference between the test characteristic curves is minimized. The result after equating is shown in Exhibit 3.6.

After the tests have been equated, the ability estimates for the tests are comparable making it possible to: (1) compare the scores of a short form to the full-length publisher test; (2) compare an assessment from one wave of testing to another wave of testing; (3) compare the scores from one study to those of another study; or (4) compare the scores from one study to the national norm sample assessment established by the test publisher. Often we want to compare

Exhibit 3.5: Illustration of Test Characteristic Curves of a Test Administered to Two Different Samples

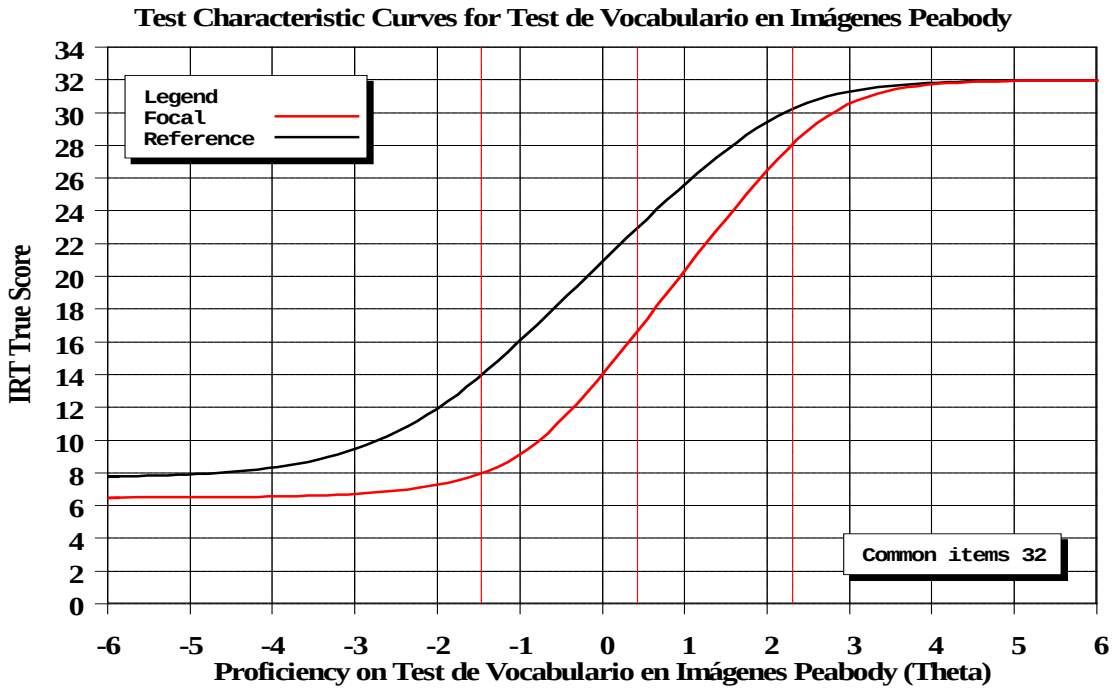
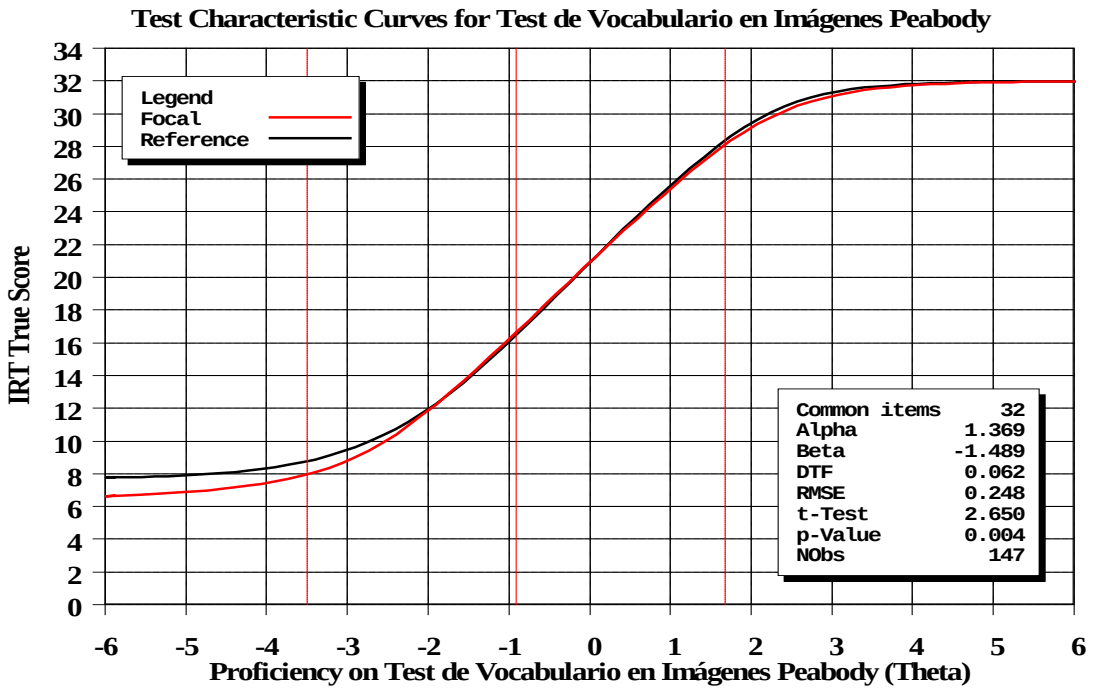


Exhibit 3.6: Test Characteristic Curves of a Test Administered to Two Different Samples, After Equating



two assessments that share only a subset of items, for example it may be the case that only a third of the items overlap between those used in a study and the national norm sample. Is such a case, the equating procedure described above would be applied using only the overlapping items. By applying item response theory (i.e., estimating item response for each item, and assuming that these functions are the same across samples), we can estimate comparable ability scores across time and samples.

The assumption that item response functions are invariant can be checked in various ways. Most commonly, Differential Item Functioning (DIF) tests are performed to determine if an item has a different item response function for different samples. In a DIF test, we compare children in two samples who have the same ability level. We then see if the tendency to get the item correct is the same for both of the samples. This comparison is done over the whole range of ability. If the probability of getting the item correct at each level of ability significantly differs between the groups, the item response is not invariant and the item cannot be used to equate tests. Note that some item are not right/wrong but have a number of levels of correctness, e.g., 0=no knowledge, 1=partial knowledge of the answer, 3= child got the item totally correct. The equating procedures and the DIF tests can be generalized to include such items.

Creating the PPVT Short Form

The shortened PPVT is an adaptive test with multiple versions used during the various data collection periods of the Head Start Impact Study. The test was developed using a 3-parameter logistic IRT model incorporating all available PPVT items used in the previous FACES studies. The added third parameter – which accounts for guessing behavior -- compensates for the possibility that a low-ability child will correctly respond to several difficult items simply by guessing.

The adaptive short form was created by selecting items of appropriate difficulty for the fall 2002 and spring 2003 Head Start Impact Study test administrations. The appropriate difficulty level was determined by examining the latent ability distribution of children of similar ages to the Head Start study sample who were included in FACES.[9]

The adapted test consists of a 20-item "core" set of test items that is used as a router test in all test versions: (a) if a child had 11 or fewer core items correct, they were administered the "basal item" set; (b) if a child had 17 or more core items correct they were administered the "ceiling item" set; and, (c) if a child had between 11 and 17 items right, no further items were administered (i.e., their score was based on the core set of items. Three versions of the adapted test were developed: in Version 1, the ceiling and basal sets each had 10 items, while in Versions 2 and 3, the basal and ceiling item sets each had nine items.

Versions 1 and 3 were comprised of odd-numbered items taken from the original PPVT, while Version 2 included only even-numbered items taken from the original PPVT. As a result Version 2 has no overlap with the other versions. There are 31 items that overlap between Versions 1 and 3 (a 77% overlap). The new items introduced in Version 3 were all in the ceiling set. Because there are no common items linking the three different PPVT adapted short forms, the earlier FACES calibrations serve as an external anchor test. That is, each of the three adapted PPVT test versions was equated to the FACES standard, placing all scores on a consistent scale metric (see previous discussion). After equating, the Head Start adapted PPVT tests were rescaled to all have a mean of 250 and a standard deviation of 50 in the base year (i.e., fall 2002). The following exhibit documents the versions used in each test administration.

Exhibit 3.7: PPVT Version Used by Cohort and Data Collection Wave

Cohort	Fall 2002	Spring 2003	Spring 2004	Spring 2005	Spring 2006
3-year-olds	Version 1 (pre-K)	Version 1 (pre-K)	Version 2 (pre-K)	Version 3 (K)	Version 3 (1st grade)
4-year-olds	Version 1 (pre-K)	Version 1 (pre-K)	Version 2 (K)	Version 3 (1st grade)	

[9] In FACES, a model-based estimate of the raw score was calculated by summing the probabilities of a correct response across all items at a given ability level. Unlike the scoring algorithms employed by the testing company, there is no assumption that easier items not presented are correct or that harder items not reached are incorrect. Such strong assumptions about how the child would have done on items they didn't take may lead to inaccurate estimates of an individual's ability.

Scoring the Adapted PPVT Test

Scoring individual child tests was done separately by age cohorts. This guarantees that the estimates of the age cohort ability distributions are unbiased estimates for each individual cohort. Within each age cohort, an individual child's ability score was based on his/her actual pattern of right, wrong, and omitted responses to the administered test items to place each child on a continuous scale defined by the IRT item difficulty, discrimination and guessing behavioral parameters. The scoring procedure used a marginal maximum likelihood approach to correctly estimate the IRT item parameters (Mislevy & Bock, 1983).

Under marginal maximum likelihood estimation, an individual's ability score is based on two types of information: (1) a "prior distribution", discussed below, for each child, i.e., an estimate of what scores would look like in the absence of any data (e.g., if one were trying to estimate tomorrow's temperature a good "prior" would be today's temperature); and (2) how an individual child performed on the administered test. That is, an individual child's ability score − referred to as the expected *a posteriori* (EAP) estimate − is based on a combination of the estimated prior and his/her actual pattern of test score responses. The greater the reliability of the test, the more an individual's actual test responses determines his/her EAP ability score; for tests that are more unreliable the more the estimated prior will determine the ability score. In effect ability scores are "shrunken" towards the prior mean in proportion to the reliability of the test.

The most common approach for creating an assumption for the prior is to assume a single prior distribution for all children. However, when subgroups of children are being compared − in this case, a comparison between the children assigned to the Head Start group and those assigned to the control group − using a separate prior distribution for each subgroup produces less biased estimates of Head Start/control group means and standard deviations and of any differences between the two groups (Mislevy, 1991). As a consequence, within each age cohort (i.e., separately for children in the 3- and 4-year-old cohorts) separate prior distributions were estimated for Head Start and control group children.

The prior distributions were estimated from the actual test responses (for each separate wave of data collection) by accumulating individual likelihood distributions (estimated for each child) to obtain marginal likelihood distributions separately for the Head Start and control

groups, in each of the two age cohorts. These estimated marginal distributions then served as the prior distributions used in the calculation of ability scores for each individual child. As a result, the "shrinkage" of the scores is toward the mean for each random assignment group rather than to the grand mean of all children.

Changes in Scoring from the Interim Report: It should be noted that the IRT approach used in the First Year Report was slightly different from that described here and used in the Final Report. The IRT approach used in the HSIS is called marginal maximum likelihood (or MML). The MML approach yields consistent estimates of item parameters. Also, in contrast to other IRT procedures, it yields plausible estimates of ability when a child gets all or none of the items correct (Mislevy & Bock, 1983). However, to use MML IRT, the analyst must posit the distribution of the child's ability prior to testing by estimating the distribution of ability for the demographic group of the child. This is called the prior distribution of the child's ability. The final estimate of a particular child's ability uses information from the prior guess of the child's ability together with the information derived from how the child responded to the current assessment. Commonly a single prior distribution is assumed for all examinees, which was what was done for the First Year Report.

For the current report, however, it was decided that a more valid estimate of Head Start and control group differences could be generated if a separate prior distribution was assumed for children in the Head Start group and for children in the control group. When children come from more than one population and comparisons are to be made between the estimated population means, an unbiased estimate of group differences can be obtained by allowing for potentially different prior distributions for the two groups being compared (Mislevy, 1991). If separate prior differences are not assumed for the two groups the IRT group means will tend to be "shrunken" towards the grand mean, resulting in Head Start-control group differences that are too small. This would have the effect of biasing downward all IRT-based impact estimates and decreasing the power to detect treatment effects when they exist. As a result of these considerations, IRT scaling was implemented for this report assuming separate prior distributions for the Head Start and control groups. This leads to results for the PPVT and TVIP in spring 2003 that will be

slightly different than those reported in the First Year Report[10]. However, we believe that these new estimates of ability lead to more valid impact estimates and more powerful tests for treatment effects.

Scoring the TVIP and CTOPPP Elision Tests

The adapted version of the TVIP was developed in the same way as the adapted PPVT using initial data and test development work conducted as part of the FACES project. A separate 3-parameter IRT model was also developed for the CTOPPP Elision test. Scoring of both tests was done using the maximum likelihood method discussed above for the scoring of the PPVT.

Scoring of Other Standardized Tests

The Compuscore and Profiles Program (Riverside Publishing, 2001) was used to score the Woodcock-Johnson III Tests of Achievement. The Compuscore program is a computer program designed to score and assist in interpreting the Woodcock-Johnson III Tests of Achievement. Based on the child's raw score, the program can produce a range of other scores including a W-Ability score, standard score, norm percentile, and both age- and grade-equivalent scores. Data used in the impact analyses were W-Ability scores, a linear IRT score obtained by the publisher using a Rasch model. It should be noted that the Rasch model does not include any consideration for guessing, as does the 3-parameter model developed by the research team for the adapted tests discussed above.

Publisher provided "look-up tables" were used to score the Batería-R Woodcock-Muñoz Pruebas de aprovechamiento-Revisada using each child's actual raw scores (i.e., number of correct items).

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[10] For the 3-year-old cohort, the First Year Report PPVT scores are 254.0 (Head Start group), 250.0 (control group) with a regression-adjusted impact of 4.23. In the Final Report, the PPVT scores are 257.50 (Head Start group), 251.43 (control group) with a regression-adjusted impact of 6.53. For the 3-year-old cohort, the First Year Report TVIP scores are 253.4 (Head Start group), 247.1 (control group) with a regression-adjusted impact of 6.31. In the Final Report, the TVIP scores are 256.83 (Head Start group), 247.05 (control group) with a regression-adjusted impact of 5.21. For the 4-year-old cohort, the First Year Report PPVT scores are 293.9 (Head Start group), 291.3 (control group) with a regression-adjusted impact of 2.59. In the Final Report, the PPVT scores are 294.35 (Head Start group), 290.25 (control group) with a regression-adjusted impact of 3.55. For the 4-year-old cohort, the First Year Report TVIP scores are 296.0 (Head Start group), 291.9 (control group) with a regression-adjusted impact of 7.95. In the Final Report, the TVIP scores are 298.54 (Head Start group), 290.77 (control group) with a regression-adjusted impact of 9.04.

The total number of correct responses was used as the score for the Leiter Sustained Attention task and the McCarthy Draw-A-Design task.

Scoring of Non-Standardized Tests

The total number of correct responses (or raw score) was used for the Letter Naming Task, Color Names and Counting, and Story and Print Concepts in the First Year Report. However, the scoring was changed for Color Names and Counting in the Final Report. The original response categories were categorical with unequal intervals. Most outcome variables in the study are continuous or binary. By changing these two outcomes to binary variables, the variables are more accurately analyzed and the findings can be easily interpreted. Two outcome variables are derived from this test—Color Score and Counting Bears. In the First Year Report, the Color Score ranged from 0-20. The child was instructed to name the color for each of 10 bears on a test plate. A child received a score of two for each color named correctly without assessor prompting. For the colors not named by the child, the assessor provided a prompt (e.g., *"Can you find the red bear?" "Point to the red bear."*). The child received a score of one for each color identified correctly with an assessor prompt(s). For the Final Report, the scoring was changed to one (correctly identified all colors without an assessor prompt) or zero (did not correctly identify all colors without an assessor prompt). For Counting Bears, the child is asked to count the 10 bears, point to each bear while counting, and then provide the total number of bears on the test plate. The task is a measure of counting and one-to-one correspondence. In the First Year Report, the Counting Bears score ranged from one (child could not count or did not try) to five (perfect counting and one-to-one correspondence). For the Final Report, the scoring was changed to one (perfectly counted the bears and demonstrated one-to-one correspondence) or zero (did not perfectly count the bears and/or did not perfectly demonstrate one-to one correspondence). The Name Writing task was scored from zero to two, based on rubrics used in the Woodcock-Johnson III Tests of Achievement Writing Samples test.

Description of Composites

The Woodcock-Johnson III cluster or composite scores are the average score for a combination of individual tests focused on a specific dimension such as reading or math. Although the individual tests are the basic administration components, the composite or cluster

interpretation minimizes the potential problem of generalizing from the score for a single, narrow ability to a broad, multifaceted domain and provides higher validity because more than one score serves as the basis for the interpretation of a child's ability (Mather and Woodcock, 2001). The child's performance on an individual test informs the broader measure of general ability in the composite or cluster score. The composite scores developed by Woodcock-Johnson III are based on developmental evidence from research on children's growth patterns. Composite scores from the Woodcock-Johnson III that are used in the study include the following:

- **Pre-Academic Cluster.** This cluster provides an early overall academic measure including pre-reading and letter and word identification skills, developing mathematics skills, and early writing and spelling skills. Tests included in the cluster include Letter-Word Identification, Spelling, and Applied Problems. The reliability is 0.97 for four- and five-year olds and 0.98 for six year olds. A similar cluster (Skills Cluster) is available for the Spanish assessment in Puerto Rico.
- **Basic Reading Skills.** This cluster provides an overall measure of basic reading skills including sight vocabulary, phonics, and structural analysis. Tests included in the cluster are Letter-Word Identification and Word Attack. The published median reliability is 0.93 in the 5-19 age range. An equivalent composite is **not** available for the Spanish assessment in Puerto Rico.
- **Math Reasoning.** This cluster provides an overall measure of mathematical knowledge and reasoning including mathematical problem solving and vocabulary and analysis. Tests included in the cluster are Applied Problems and Quantitative Concepts. The published median reliability is 0.95 in the 5-19 age range. An equivalent composite is **not** available for the Spanish assessment in Puerto Rico.
- **Academic Skills.** This cluster provides an overall score of basic achievement skills including reading, math calculation, and spelling. Tests included in the cluster are Letter-Word Identification, Calculation, and Spelling. The published median reliability is 0.95 in the 5-19 age range. An equivalent composite is **not** available for the Spanish assessment in Puerto Rico.
- **Academic Applications.** This cluster measures the application of academic skills to academic problems. Tests included in the cluster are Passage Comprehension, Applied Problems, and Writing Samples. The published median reliability is 0.94 in the 5-19 age range. An equivalent composite is **not** available for the Spanish assessment in Puerto Rico.

Exhibit 3.8 provides the list of composites and the time when the necessary scales comprising each composite were available for each cohort in the combined sample. As noted above, only the Skills Composite is available for the Spanish assessment.

Exhibit 3.8: Composite Measures by Cohort and Year for the Combined Sample

Test	Cohort	Fall 2002	Spring 2003	Spring 2004	Spring 2005	Spring 2006
Pre-Academic Skills Cluster	3	X	X	X	X	X
	4	X	X	X	X	
Basic Reading Skills	3				X	X
	4			X	X	
Math Reasoning	3				X	X
	4			X	X	
Academic Skills	3					X
	4				X	
Academic Applications	3					X
	4				X	

Percentiles

Exhibits 3.9 and 3.10 provide the percentiles by cohort and data collection period for the normed tests from the child assessment. These percentiles present a mixed picture on the cognitive performance of the study children. Some measures suggest the study children are performing below average compared to children in the general population while other measures suggest that the study children are performing at or above average relative to the general population. Hence, it is unclear whether these norms suggest that this group of children from low-income families is indeed faring as would be expected of other children their age. It should be noted that a WJ III percentile can increase by several points with one additional correct item.

The percentiles for the WJ III tests are unweighted and based on the mean raw score, mean birth date, and mean testing date for each cohort by data collection. This information was entered into the Compuscore program for generating the mean percentile for each test by cohort and year. For the PPVT, each student's standard score was generated and then on average standard score was calculated for each cohort and year. With this average standard score, a table for normals was used to generate the corresponding percentile.

Exhibit 3.9: Percentiles on the Norm-Referenced Tests for the 4-Year-Old Cohort by Year

Test	Fall 2002 Baseline		Spring 2003 Head Start		Spring 2004 Kindergarten		Spring 2005 1st Grade	
	Head Start Group	Control Group	Head Start Group	Control Group	Head Start Group	Control Group	Head Start Group	Control Group
Language and Literacy								
PPVT (Adapted)	29	28	31	27	28	26	28	25
WJ III Letter-Word Identification	34	34	46	38	62	62	63	63
WJ III Spelling	34	34	30	30	58	58	61	61
WJ III Oral Comprehension	39	38	29	29	33	40	42	42
WJ III Pre-Academic Skills	40	31	32	30	54	54	60	60
WJ III Word Attack	--	--	--	--	93	93	81	81
WJ III Basic Reading Skills	--	--	--	--	82	82	73	73
WJ III Academic Applications	--	--	--	--	--	--	42	42
WJ III Academic Skills	--	--	--	--	--	--	65	65
WJ III Passage Comprehension	--	--	--	--	--	--	38	38
WJ III Writing Samples	--	--	--	--	--	--	46	46
Math								
WJ III Applied Problems	30	30	23	23	36	36	47	47
WJ III Quantitative Concepts	--	--	--	--	43	43	38	38
WJ III Math Reasoning	--	--	--	--	36	36	42	42
WJ III Calculation	--	--	--	--	--	--	62	62

NOTE: --indicates data not collected on the outcome for the cohort during the data collection period.

Exhibit 3.10: Percentiles on the Norm-Referenced Tests for the 3-Year-Old Cohort by Year

Test	Fall 2002 Baseline		Spring 2003 Head Start Year		Spring 2004 Age 4 Year		Spring 2005 Kindergarten		Spring 2006 1st Grade	
	Head Start Group	Control Group	Head Start Group	Control Group	Head Start Group	Control Group	Head Start Group	Control Group	Head Start Group	Control Group
Language and Literacy										
PPVT (Adapted)	29	29	32	29	31	29	35	36	24	24
WJ III Letter-Word Identification	51	51	52	34	49	49	63	63	63	63
WJ III Spelling	45	45	41	28	38	38	61	53	64	64
WJ III Oral Comprehension	39	39	41	41	36	36	37	38	48	41
WJ III Pre-Academic Skills	40	40	39	24	38	38	57	55	60	60
WJ III Word Attack	--	--	--	--	--	--	89	91	77	77
WJ III Basic Reading Skills	--	--	--	--	--	--	79	80	71	71
WJ III Academic Applications	--	--	--	--	--	--	--	--	45	45
WJ III Academic Skills	--	--	--	--	--	--	--	--	64	64
WJ III Passage Comprehension	--	--	--	--	--	--	--	--	40	40
WJ III Writing Samples	--	--	--	--	--	--	--	--	57	57
Math										
WJ III Applied Problems	32	32	32	24	27	27	37	40	45	45
WJ III Quantitative Concepts	--	--	--	--	--	--	36	40	35	35
WJ III Math Reasoning	--	--	--	--	--	--	34	37	39	39
WJ III Calculation	--	--	--	--	--	--	--	--	57	57

NOTE: --indicates data not collected on the outcome for the cohort during the data collection period.

Other Cognitive Outcomes

The tests included in the direct child assessment battery are described above. Other measures of children's cognitive skills include the following:

- **Teacher report of academic skills.** Each child is rated on three academic skills (language and literacy, science and social studies, and mathematical skills) by his/her teacher. The child is rated as compared to other children at the same grade level using a five point scale ranging from one (far below average) to five (far above average).
- **Teacher report of school accomplishments.** Each child is rated by his/her teacher on a series of items that describe the child's skills, knowledge, and behaviors focusing on language and literacy and mathematics. The child is rated using a five point scale that reflects the degree to which the child has acquired the demonstrated skills, knowledge, and behaviors ranging from one (not yet) to five (proficient). More complex skills, knowledge, and behaviors are added to the first grade list.
- **Parent report of promotion.** Parents were asked the grade level of their child. This information was confirmed with the teacher-reported expected promotion of the child. Overall there was consistency between the two reports. Parent data were used because the response rate was higher for parents than teachers and it provided the actual promotion data while the teacher data provided information on whether or not the child was expected to be promoted to the next grade.
- **Parent emergent literacy scale (PELS).** PELS is a parent-report on five literacy items originally developed for use in FACES 2000: child can recognize most/all of the letters of the alphabet; child can count to 20; child pretended to write his/her name in the last month; child can write his/her first name; and child can identify the primary colors.

Social-Emotional Outcomes

- **Social skills and positive approaches to learning.** Parents are asked to rate their child's social skills and positive approaches to learning. The measure assesses social skills focused on cooperative and empathic behavior, such as, "Makes friends easily," "Comforts or helps others," and "Accepts friends' ideas in sharing and playing." The measure also assesses aspects of children's approaches to learning such as curiosity, imagination, openness to new tasks and challenges, and having a positive attitude about gaining new knowledge and skills. Examples include, "Enjoys learning," "Likes to try new things," and "Shows imagination in work and play." The scale contains seven items, with each item scored from zero (not true) to two (very true), and the scale scores can range from 0 to 14. The scale is based on an instrument used in the Head Start Family and Child Experiences Survey (FACES).[11] Mean scores on the scale obtained from parents of Head Start children in the Head Start Impact Study

[11] Administration on Children and Families (2001). Retrieved 10/15/04 from: http://www.acf.dhhs.gov/programs/core/ongoing_research/faces/faces_instruments.html

were closely comparable to mean scores obtained from parents of an independent national sample of Head Start children in FACES 2000. As in FACES, social skills and positive approaches to learning scores tended to be skewed toward the higher end of the range because parents tended to rate their children as exhibiting most of the positive attributes asked about in the rating instrument. Nonetheless, the scale has shown significant relationships with other measures of children's social development and with relevant child and family characteristics.

- **Social competencies checklist**. Parents were asked to provide information on social capabilities using a Social Competencies Checklist, also used in FACES 2000. The checklist consisted of 12 items; for each item, the parent was asked to report whether the child engaged in that behavior or exhibited that attribute "regularly" or "very rarely or not at all." Examples of the items included, "Shares newly learned ideas," "Takes care of personal belongings," "Helps with simple household tasks," and "Notices when others are happy, sad, angry." The total scale score could range from zero (all items rated "rarely or not at all") to 12 (all items rated "does regularly").

- **Problem behavior of children.** Parents were asked to rate their children on items dealing with aggressive or defiant behavior such as, "Hits and fights with others," "Has temper tantrums or hot temper," and "Is disobedient at home." Other items dealt with inattentive or hyperactive behavior, including, "Can't concentrate, can't pay attention for long," and "Is very restless and fidgets a lot." A third set of items dealt with shy, withdrawn, or depressed behavior, e.g., "Feels worthless or inferior," and "Is unhappy, sad, or depressed." For each item, the parent was asked to judge whether the behavioral description was "not true," "sometimes true," or "very true" of the child. The **Total Behavior Problem** scale derived from parent ratings contained 14 rating items and the total scale score could range from zero (all items marked "not true") to 28 (all items marked "very true"). The **Aggressive Behavior** subscale contained four items and could range from zero to eight. The **Hyperactive Behavior** subscale contained three items and scores could range from zero to six. The **Withdrawn Behavior** subscale contained three items and scores could range from zero to six. These scales were also used in FACES 2000, and their development was based on prior work by Rutter, Achenbach, Zill and Peterson, and others (see U.S. Department of Health and Human Services, 2001). The mean scores obtained in the Head Start Impact Study were very comparable to mean scores obtained from parents of an independent national sample of Head Start children FACES.

- **Child-Teacher Relationship.** This instrument developed by Robert Pianta includes three scales: closeness, conflict, and total positive relationship. Both a short form and a long form are available; the shortened version of the instrument was used for the Head Start Impact Study. The teacher is asked to rate the child on 15 items, such as, "If upset, this child will seek comfort from me" or "This child easily becomes angry at me". The teacher rates the child on each item using a five point response format ranging from one (definitely does not apply) to five (definitely applies). The closeness scale contained seven items and the scores could range from 7 to 35. The conflict scale contained eight items and the scores could range from 8 to 40. The total positive relationship scale contained 15 items and the scores could range from 15 to 75.

- **Child-Parent Relationship.** Parents were asked to rate their child's relationship with them. The same scales and scoring were used as described for the Child Teacher Relationship scale. It also should be noted that he long version of the Child Teacher Relationship was adapted for use with parents in The NICHD Study of Early Child Care.
- **Adjustment Scales for Preschool Intervention (ASPI).** The ASPI is based on the ASCA (Adjustment Scales for Children and Adolescents). The ASPI (Lutz, Fantuzzo, & McDermott, 2002) contains 24 classroom situations that provide 144 descriptors of both typical and problem classroom behavior. The teacher is asked to select all behavior descriptions that match a child's behavior to a specified classroom situation over the past two months. For example, one classroom situation is, "How does this child seek your help?" The behavior descriptions are: too lethargic to ask, asks for help when needed, seeks help when not needed, rarely needs help, not shy but never seeks help, or too timid to ask. The teacher is instructed to select any behavior description that she/he observed for this child during the past two months. The factors identified from the ASPI are aggressive, withdrawn/low energy, socially reticent, oppositional, and inattentive/hyperactive. In addition, three situational dimensions have been identified with the ASPI—structured learning, peer interaction, and teacher interaction. The raw score is based on the sum of the checked behavior descriptions for the items that loaded on each factor and the raw scores are converted to t-scores derived from the developer's original ASPI standardization sample.

Health Outcomes

- **Receipt of health care services.** Parents were asked to report on various health care services, two of which are used in this report:
  - **Whether the child has health insurance.** Parents were asked if the child was currently covered by Medicaid or a state health insurance program, or by health insurance through their job or the job of another employed adult.
  - **Whether the child has received dental care.** Parents were asked if the child had ever seen a dentist since September of that year.[12]
- **Child's health status.** Parents were asked to report on their child's health status:
  - **Child's health status (excellent or very good).** Parents were asked if, overall, the child's health was excellent, very good, good, fair, or poor. This outcome was coded "yes" for those who reported that their child's health was excellent or very good.
  - **Whether the child needs ongoing medical care.** Parents were asked if their child had an illness or condition that requires regular ongoing medical care.
  - **Whether child received medical care for an injury in the last month.** Parents were asked how many times their child, in the last month, had seen a doctor or other medical professional or visited a clinic or emergency room for an injury.

[12] At the time of the 2002 baseline, parents were asked whether the child had seen a dentist.

This outcome was coded yes if the parent reported any such occurrences in the last month.

Parenting Outcomes

- **Educational activities.** Parents were asked to report on the types of educational activities they did with their child:
  - **Reading to the child at home.** Parents reported on the item "How many times have you or someone in your family read to [CHILD] in the past week?" Possible responses range from one (not at all) to four (every day).
  - **Family cultural enrichment activities.** Parents reported on a seven-item checklist of activities the parent, or another family member, may have done with the child during the past month. The seven activities include going to a movie; play or concert; art gallery or museum; playground, park, or zoo; community, ethnic, or religious event; and talking about family or cultural heritage and going on errands. A total score was computed by summing the number of different activities the parent and child participated in together, with a possible score of zero (none) to seven (all).
- **Discipline practices.** Parents reported on the following:
  - **Use of physical discipline.** Parents reported on the item "Sometimes children mind pretty well and sometimes they don't. Have you spanked [CHILD] in the past week for not minding?"
  - **Use of time out.** Parents reported on the item "Have you used 'time out' or sent [CHILD] to his/her room in the past week for not minding?"
- **Parental safety practices.** Parents reported on a 10-item scale that assessed how often the 10 different safety precautions were used, including keeping harmful objects out of reach, using car seats, supervising the child during bath time, and having a first aid kit and working smoke detector at home. Possible responses ranged from one (never) to four (always).
- **Parenting styles.** The parents were asked to respond to selected items from the Child –Rearing Practices Report (CRPR) (Block, 1965). Parents were asked to respond to items, such as, "I teach my child that misbehavior or breaking the rules will always be punished one way or another" and "I believe physical punishment to be the best way of disciplining" using a Likert scale that ranged from one (exactly like you) to five (not at all like you). The parenting styles identified for the analysis, and described in Chapter 7 of the Final Report, are: authoritative, authoritarian, permissive, and neglectful.
- **Teacher report of parent participation in school activities.** Teachers were asked two questions: "Have one or both of the child's parents (or guardians) attended open house meetings, back-to-school nights, or class events, such as a class play or recital, this year?" and "Have one or both of this child's parents (or guardians) acted as volunteers or helped out with class activities or class trips this year?"

- **Teacher report of school contact/communication.** Teachers were asked two questions: How often have this child's parents (or guardians) initiated contact with you to find out how things were going with the child or to offer help with class activities?" and How often have you had to contact or tried to contact this child's parent(s) or guardians about behavior or schoolwork problems this child has been having?" The response categories ranged from zero (not at all) to four (about once a month or more often).

Psychometric Information

In HSIS, various data collection instruments were used to assess children's outcomes. The means, standard deviations, and reliabilities (when appropriate) are reported by cohort and year in Exhibits 3.11–3.15.

Intraclass Correlations

The intraclass correlation in a multilevel context is the correlation between any two randomly chosen individuals. Exhibits 3.16–3.20 provide the components of child-to-child outcome variance, the percent of variance for each component, and the ICCs for children within centers, and centers within programs.

Exhibit 3.11: Fall 2002 Psychometric Data for All Outcomes by Cohort

Outcome	Child Cohort					
	3-Year-Old Cohort (Baseline)			4-Year-Old Cohort (Baseline)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
PARENTING PRACTICES						
Family Cultural Enrichment Scale	3.34	1.35	NA	3.43	1.37	NA
Parent Read to Child in Last Week	0.34	0.47	NA	0.36	0.48	NA
Parental Safety Practices Scale	3.66	0.35	NA	3.66	0.35	NA
Parent Spanked Child in Last Week	0.48	0.50	NA	0.42	0.49	NA
Parent Used Time Out in Last Week	0.63	0.48	NA	0.61	0.49	NA
Parenting Style: Authoritarian						
Parenting Style: Authoritative						
Parenting Style: Neglectful						
Parenting Style: Permissive						
HEALTH						
Child Received Dental Care	0.67	0.47	NA	0.75	0.44	NA
Child's Overall Health Status Is Excellent/Good	0.79	0.41	NA	0.79	0.41	NA
Child Had Care for Injury Last Month	0.06	0.24	NA	0.06	0.23	NA
Child Has Health Insurance Coverage	0.89	0.31	NA	0.87	0.34	NA
Child Needs Ongoing Care	0.20	0.40	NA	0.16	0.37	NA
SOCIAL-EMOTIONAL						
Aggressive Behavior	3.08	1.75	0.87	3.01	1.73	0.85
Hyperactive Behavior	1.89	1.55	0.84	1.96	1.54	0.82
Social Competencies	10.71	1.52	0.94	10.81	1.44	0.94
Social Skills and Positive Approaches to Learning	12.15	1.80	0.85	12.28	1.76	0.85
Total Problem Behavior	6.15	3.64	0.96	6.27	3.72	0.95
Withdrawn Behavior	0.62	0.91	0.90	0.73	0.98	0.88
COGNITIVE						
Parent-Reported Emergent Literacy Scale (PELS)	1.99	1.25	0.54	2.76	1.38	0.61
CTOPPP Elision	232.74	40.37	0.78	271.93	39.37	0.84
PPVT (Adapted)	230.19	37.65	0.61	271.28	38.76	0.78
Counting Bears	0.16	0.37	NA	0.40	0.49	NA

Exhibit 3.11: Fall 2002 Psychometric Data for All Outcomes by Cohort (continued)

Outcome	Child Cohort					
	3-Year-Old Cohort (Baseline)			4-Year-Old Cohort (Baseline)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
Color Identification	0.28	0.45	NA	0.54	0.50	NA
McCarthy Draw-a-Design	2.71	1.09	0.65	3.82	1.70	0.82
WJ III Applied Problems	366.15	27.34	0.88	390.54	22.65	0.89
WJ III Oral Comprehension	433.52	12.74	0.76	445.13	14.13	0.82
WJ III Pre-Academic Skills	336.47	20.37	0.77	357.65	18.65	0.77
WJ III Spelling	334.41	24.40	0.70	358.28	25.17	0.80
WJ III Letter-Word Identification	294.09	22.43	0.82	308.28	25.46	0.89

NOTE: NA indicates the outcome is not an appropriate candidate for reliability (e.g., single item, index, etc.).

Exhibit 3.12: Spring 2003 Psychometric Data for All Outcomes by Cohort

Outcome	Child Cohort					
	3-Year-Old Cohort (Head Start Year)			4-Year-Old Cohort (Head Start Year)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
PARENTING PRACTICES						
Family Cultural Enrichment Scale	3.66	1.41	NA	3.95	1.43	NA
Parent Read to Child in Last Week	0.32	0.47	NA	0.34	0.47	NA
Parental Safety Practices Scale	3.71	0.33	NA	3.72	0.33	NA
Parent Spanked Child in Last Week	0.45	0.50	NA	0.38	0.48	NA
Parent Used Time Out in Last Week	0.64	0.48	NA	0.64	0.48	NA
HEALTH						
Child Received Dental Care	0.60	0.49	NA	0.65	0.48	NA
Child's Overall Health Status Is Excellent/Good	0.78	0.41	NA	0.80	0.40	NA
Child Had Care for Injury Last Month	0.09	0.28	NA	0.12	0.32	NA
Child Has Health Insurance Coverage	0.92	0.27	NA	0.88	0.32	NA
Child Needs Ongoing Care	0.13	0.34	NA	0.11	0.32	NA
SOCIAL-EMOTIONAL						
Aggressive Behavior	3.01	1.72	0.61	2.79	1.68	0.56
Hyperactive Behavior	1.85	1.55	0.62	1.74	1.48	0.59
Closeness (parent-reported)	33.53	2.50	0.71	33.45	2.65	0.73
Conflict (parent-reported)	18.08	6.70	0.77	17.58	6.55	0.76
Positive Relationships (parent-reported)	63.35	7.65	0.65	63.76	7.69	0.63
Social Competencies	10.97	1.33	0.54	11.03	1.33	0.56
Social Skills and Positive Approaches to Learning	12.39	1.73	0.62	12.47	1.72	0.63
Total Problem Behavior	6.02	3.65	0.74	5.70	3.59	0.74
Withdrawn Behavior	0.57	0.95	0.41	0.67	0.96	0.38
COGNITIVE						
Parent-Reported Emergent Literacy Scale (PELS)	2.60	1.45	0.58	3.56	1.39	0.56
CTOPPP Elision	238.30	48.04	0.82	272.66	49.29	0.87
PPVT (Adapted)	254.49	35.79	0.62	292.35	37.89	0.79
Counting Bears	0.29	0.45	NA	0.57	0.50	NA
Color Identification	0.48	0.50	NA	0.69	0.46	NA

Exhibit 3.12: Spring 2003 Psychometric Data for All Outcomes by Cohort (continued)

Outcome	Child Cohort					
	3-Year-Old Cohort (Head Start Year)			4-Year-Old Cohort (Head Start Year)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
McCarthy Draw-a-Design	3.14	1.18	0.54	4.49	2.06	0.74
Letter Naming	4.71	7.20	0.96	10.40	9.70	0.97
WJ III Applied Problems	375.43	28.39	0.89	395.98	25.53	0.90
WJ III Oral Comprehension	435.48	14.04	0.79	443.52	17.92	0.88
WJ III Pre-Academic Skills	341.56	19.74	0.67	362.83	21.63	0.76
WJ III Spelling	345.11	22.58	0.73	369.66	25.44	0.78
WJ III Letter-Word Identification	303.79	25.04	0.87	322.41	27.80	0.90

NOTE: NA indicates the outcome is not an appropriate candidate for reliability (e.g., single item, index, etc.).

Exhibit 3.13: Spring 2004 Psychometric Data for All Outcomes by Cohort

Outcome	Child Cohort					
	3-Year-Old Cohort (Age 4 Year)			4-Year-Old Cohort (Kindergarten)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
PARENTING PRACTICES						
Family Cultural Enrichment Scale	3.91	1.33	NA	4.03	1.39	NA
Parent Read to Child in Last Week	0.34	0.48	NA	0.37	0.48	NA
Parental Safety Practices Scale	3.72	0.34	NA	3.70	0.36	NA
Parent Spanked Child in Last Week	0.34	0.47	NA	0.30	0.46	NA
Parent Used Time Out in Last Week	0.62	0.48	NA	0.57	0.49	NA
Parenting Style: Authoritarian	0.06	0.23	0.74	0.08	0.27	0.73
Parenting Style: Authoritative	0.69	0.46	0.74	0.65	0.48	0.74
Parenting Style: Neglectful	0.06	0.24	0.75	0.11	0.31	0.75
Parenting Style: Permissive	0.20	0.40	0.74	0.19	0.39	0.73
School Contact and Communication	--	--	--	0.84	0.37	NA
Parent Participation	--	--	--	0.89	0.31	NA
HEALTH						
Child Received Dental Care	0.69	0.46	NA	0.67	0.47	NA
Child's Overall Health Status Is Excellent/Good	0.83	0.38	NA	0.79	0.41	NA
Child Had Care for Injury Last Month	0.10	0.31	NA	0.11	0.32	NA
Child Has Health Insurance Coverage	0.93	0.26	NA	0.88	0.33	NA
Child Needs Ongoing Care	0.13	0.34	NA	0.12	0.33	NA
SOCIAL-EMOTIONAL						
Aggressive Behavior	2.64	1.75	0.65	2.44	1.70	0.58
Hyperactive Behavior	1.68	1.48	0.58	1.46	1.49	0.62
Closeness (parent-reported)	33.44	2.69	0.75	33.26	2.82	0.74
Conflict (parent-reported)	17.89	6.72	0.78	17.65	6.62	0.78
Positive Relationships (parent-reported)	63.44	7.87	0.65	63.49	7.94	0.62
Social Competencies	11.08	1.36	0.62	11.13	1.19	0.50
Social Skills and Positive Approaches to Learning	12.53	1.66	0.61	12.64	1.58	0.57
Total Problem Behavior	5.46	3.71	0.77	5.09	3.60	0.74
Withdrawn Behavior	0.62	0.93	0.41	0.73	0.97	0.35

Exhibit 3.13: Spring 2004 Psychometric Data for All Outcomes by Cohort (continued)

Outcome	Child Cohort					
	3-Year-Old Cohort (Age 4 Year)			4-Year-Old Cohort (Kindergarten)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
ASPI-Aggressive	--	--	--	48.73	7.38	0.89
ASPI-Inattentive/Hyperactive	--	--	--	50.73	8.33	0.78
ASPI-Withdrawn/ Low Energy	--	--	--	49.15	7.06	0.78
ASPI-Oppositional	--	--	--	47.85	7.34	0.72
ASPI-Problems with Peer Interactions	--	--	--	51.43	10.73	0.81
ASPI-Shy/Socially Reticent	--	--	--	47.46	7.61	0.74
ASPI-Problems with Structured Learning	--	--	--	51.06	10.12	0.82
ASPI-Problems with Teacher Interaction	--	--	--	49.93	9.43	0.63
Closeness (teacher-reported)	--	--	--	30.22	4.41	0.80
Conflict (teacher-reported)	--	--	--	13.42	5.99	0.87
Positive Relationships (teacher-reported)	--	--	--	64.59	8.45	0.87
COGNITIVE						
Parent-Reported Emergent Literacy Scale (PELS)	3.92	1.24	0.50	NA	NA	NA
CTOPPP Elision	276.52	51.80	0.87	322.89	48.39	0.88
PPVT (Adapted)	299.65	38.65	0.70	333.03	40.17	0.84
Counting Bears	0.55	0.50	NA	--	--	--
Color Identification	0.81	0.40	NA	--	--	--
McCarthy Draw-a-Design	4.88	2.13	0.76	--	--	--
Letter Naming	13.29	9.63	0.97	22.82	6.35	0.97
WJ III Applied Problems	400.46	22.59	0.89	426.46	20.72	0.89
WJ III Oral Comprehension	446.00	16.05	0.85	456.90	18.48	0.89
WJ III Pre-Academic Skills	369.49	21.50	0.78	406.35	23.44	0.81
WJ III Quantitative Concepts	--	--	--	441.85	17.47	0.88
WJ III Spelling	376.50	26.26	0.81	414.01	28.92	0.86
WJ III Word Attack	--	--	--	432.14	34.44	0.89
WJ III Letter-Word Identification	331.62	28.35	0.91	378.12	32.58	0.94
WJ III Basic Reading Skills	--	--	--	405.09	31.87	0.89
WJ III Math Reasoning	--	--	--	434.14	17.11	0.61

Exhibit 3.13: Spring 2004 Psychometric Data for All Outcomes by Cohort (continued)

Outcome	Child Cohort					
	3-Year-Old Cohort (Age 4 Year)			4-Year-Old Cohort (Kindergarten)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
School Accomplishments	--	--	--	28.14	7.82	0.94
Language and Literacy Ability	--	--	--	0.73	0.44	NA
Math Ability	--	--	--	0.78	0.42	NA
Social Studies and Science Ability	--	--	--	0.82	0.39	NA
Promotion	--	--	--	0.93	0.26	NA

NOTE: NA indicates the outcome is not an appropriate candidate for reliability (e.g., single item, index, etc.).

-- indicates data not collected on the outcome for the cohort during the data collection period.

Exhibit 3.14: Spring 2005 Psychometric Data for All Outcomes by Cohort

Outcome	Child Cohort					
	3-Year-Old Cohort (Kindergarten)			4-Year-Old Cohort (1st Grade)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
PARENTING PRACTICES						
Family Cultural Enrichment Scale	3.93	1.40	NA	3.96	1.38	NA
Parent Read to Child in Last Week	0.34	0.47	NA	0.41	0.49	NA
Parental Safety Practices Scale	3.72	0.35	NA	NA	NA	NA
Parent Spanked Child in Last Week	0.28	0.45	NA	0.22	0.41	NA
Parent Used Time Out in Last Week	0.55	0.50	NA	0.51	0.50	NA
Parenting Style: Authoritarian	0.07	0.25	0.73	0.08	0.27	0.75
Parenting Style: Authoritative	0.67	0.47	0.73	0.66	0.48	0.75
Parenting Style: Neglectful	0.07	0.25	0.73	0.08	0.27	0.75
Parenting Style: Permissive	0.20	0.40	0.73	0.19	0.39	0.75
School Contact and Communication	0.82	0.39	NA	0.80	0.40	NA
Parent Participation	0.87	0.34	NA	0.87	0.34	NA
HEALTH						
Child Received Dental Care	0.74	0.44	NA	0.67	0.47	NA
Child's Overall Health Status Is Excellent/Good	0.82	0.38	NA	0.81	0.39	NA
Child Had Care for Injury Last Month	0.13	0.34	NA	0.14	0.35	NA
Child Has Health Insurance Coverage	0.92	0.27	NA	0.87	0.33	NA
Child Needs Ongoing Care	0.17	0.37	NA	0.13	0.34	NA
SOCIAL-EMOTIONAL						
Aggressive Behavior	2.34	1.83	0.70	2.25	1.79	0.66
Hyperactive Behavior	1.44	1.51	0.64	1.44	1.53	0.66
Closeness (parent-reported)	33.12	2.73	0.73	33.24	2.70	0.72
Conflict (parent-reported)	17.13	6.52	0.79	16.94	6.77	0.81
Positive Relationships (parent-reported)	63.85	7.64	0.68	64.22	8.10	0.65
Social Competencies	11.01	1.35	0.57	11.11	1.27	0.56
Social Skills and Positive Approaches to Learning	12.42	1.76	0.65	12.64	1.61	0.62
Total Problem Behavior	4.95	3.86	0.79	4.94	3.82	0.78
Withdrawn Behavior	0.64	0.95	0.43	0.77	1.03	0.44

Exhibit 3.14: Spring 2005 Psychometric Data for All Outcomes by Cohort (continued)

Outcome	Child Cohort					
	3-Year-Old Cohort (Kindergarten)			4-Year-Old Cohort (1st Grade)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
ASPI-Aggressive	48.84	7.45	0.91	48.83	7.66	0.91
ASPI-Inattentive/Hyperactive	50.32	8.68	0.81	50.42	8.35	0.78
ASPI-Withdrawn/ Low Energy	48.88	6.63	0.75	49.55	7.29	0.80
ASPI-Oppositional	48.34	7.85	0.78	47.83	7.41	0.74
ASPI-Problems with Peer Interactions	51.22	11.28	0.86	51.43	11.20	0.85
ASPI-Shy/Socially Reticent	47.40	7.56	0.71	47.39	7.58	0.73
ASPI-Problems with Structured Learning	50.11	10.54	0.85	50.67	10.74	0.85
ASPI-Problems with Teacher Interaction	49.36	10.00	0.70	49.49	10.28	0.71
Closeness (teacher-reported)	30.14	4.52	0.82	29.83	4.54	0.82
Conflict (teacher-reported)	14.00	6.88	0.90	14.07	6.59	0.88
Positive Relationships (teacher-reported)	63.98	9.60	0.89	63.57	9.28	0.88
COGNITIVE						
CTOPPP Elision	333.21	45.69	0.89	--	--	--
PPVT (Adapted)	340.11	28.36	0.67	360.96	32.25	0.80
Letter Naming	23.56	5.62	0.97	--	--	--
WJ III Academic Applications	--	--	--	461.51	16.81	0.81
WJ III Academic Skills	--	--	--	448.38	24.20	0.84
WJ III Applied Problems	430.84	21.36	0.88	454.66	19.56	0.85
WJ III Calculation	--	--	--	461.13	18.85	0.82
WJ III Oral Comprehension	457.63	17.51	0.88	472.91	17.48	0.85
WJ III Passage Comprehension	--	--	--	450.08	24.43	0.91
WJ III Pre-Academic Skills	411.51	22.69	0.81	446.07	24.66	0.85
WJ III Quantitative Concepts	443.35	16.86	0.87	461.54	17.74	0.87
WJ III Spelling	419.81	24.80	0.84	451.03	25.98	0.89
WJ III Word Attack	436.69	33.64	0.90	468.28	31.94	0.94
WJ III Letter-Word Identification	383.72	32.78	0.93	432.64	36.38	0.94
WJ III Writing Samples	--	--	--	479.81	13.86	0.74
WJ III Basic Reading Skills	410.21	31.49	0.89	450.44	32.74	0.91

Exhibit 3.14: Spring 2005 Psychometric Data for All Outcomes by Cohort (continued)

Outcome	Child Cohort					
	3-Year-Old Cohort (Kindergarten)			4-Year-Old Cohort (1st Grade)		
	Mean	Standard Deviation	Reliability	Mean	Standard Deviation	Reliability
WJ III Math Reasoning	437.10	17.22	0.70	458.02	17.33	0.71
School Accomplishments	27.94	7.67	0.94	43.51	10.66	0.96
Language and Literacy Ability	0.77	0.42	NA	0.70	0.46	NA
Math Ability	0.82	0.38	NA	0.79	0.41	NA
Social Studies and Science Ability	0.85	0.35	NA	0.84	0.36	NA
Promotion	0.91	0.29	NA	0.92	0.27	NA

NOTE: NA indicates the outcome is not an appropriate candidate for reliability (e.g., single item, index, etc.).

-- indicates data not collected on the outcome for the cohort during the data collection period.

Exhibit 3.15: Spring 2006 Psychometric Data for All Measures by Cohort

Outcome	Child Cohort: 3-Year-Old Cohort (1st Grade) Mean	Standard Deviation	Reliability
PARENTING PRACTICES			
Family Cultural Enrichment Scale	3.90	1.46	NA
Parent Read to Child in Last Week	0.37	0.48	NA
Parent Spanked Child in Last Week	0.22	0.42	NA
Parent Used Time Out in Last Week	0.51	0.50	NA
Parenting Style: Authoritarian	0.06	0.24	0.71
Parenting Style: Authoritative	0.70	0.46	0.71
Parenting Style: Neglectful	0.06	0.24	0.71
Parenting Style: Permissive	0.18	0.38	0.71
School Contact and Communication	0.81	0.39	NA
Parent Participation	0.84	0.36	NA
HEALTH			
Child Received Dental Care	0.73	0.44	NA
Child's Overall Health Status Is Excellent/Good	0.84	0.37	NA
Child Had Care for Injury Last Month	0.08	0.27	NA
Child Has Health Insurance Coverage	0.93	0.26	NA
Child Needs Ongoing Care	0.16	0.37	NA
SOCIAL-EMOTIONAL			
Aggressive Behavior	2.25	1.82	0.69
Hyperactive Behavior	1.44	1.53	0.68
Closeness (parent-reported)	33.20	2.66	0.73
Conflict (parent-reported)	17.02	6.66	0.81
Positive Relationships (parent-reported)	64.12	7.66	0.70
Social Competencies	11.11	1.30	0.59
Social Skills and Positive Approaches to Learning	12.54	1.66	0.62
Total Problem Behavior	4.95	3.90	0.79
Withdrawn Behavior	0.72	0.97	0.36
ASPI-Aggressive	48.99	7.57	0.90

Exhibit 3.15: Spring 2006 Psychometric Data for All Measures by Cohort (continued)

Outcome	Child Cohort 3-Year-Old Cohort (1st Grade) Mean	Standard Deviation	Reliability
ASPI-Inattentive/Hyperactive	50.54	8.44	0.78
ASPI-Withdrawn/ Low Energy	49.24	6.92	0.77
ASPI-Oppositional	48.31	7.84	0.78
ASPI-Problems with Peer Interactions	52.10	11.56	0.86
ASPI-Shy/Socially Reticent	47.23	7.34	0.69
ASPI-Problems with Structured Learning	50.68	10.62	0.85
ASPI-Problems with Teacher Interaction	50.04	10.07	0.69
Closeness (teacher-reported)	29.84	4.48	0.81
Conflict (teacher-reported)	14.14	6.93	0.89
Positive Relationships (teacher-reported)	63.45	9.71	0.89
COGNITIVE			
PPVT (Adapted)	359.16	29.38	0.78
WJ III Academic Applications	462.67	17.24	0.83
WJ III Academic Skills	450.04	22.74	0.82
WJ III Applied Problems	454.37	20.38	0.85
WJ III Calculation	461.73	17.13	0.80
WJ III Oral Comprehension	471.93	16.14	0.83
WJ III Passage Comprehension	450.64	23.89	0.91
WJ III Pre-Academic Skills	447.35	24.28	0.85
WJ III Quantitative Concepts	461.69	17.25	0.86
WJ III Spelling	454.41	24.69	0.89
WJ III Word Attack	468.98	30.72	0.93
WJ III Letter Word Identification	433.31	35.99	0.94
WJ III Writing Samples	483.04	14.64	0.75
WJ III Basic Reading Skills	451.13	31.87	0.90
WJ III Math Reasoning	457.97	17.24	0.78
School Accomplishments	42.60	10.30	0.96
Language and Literacy Ability	0.72	0.45	NA

Exhibit 3.15: Spring 2006 Psychometric Data for All Measures by Cohort (continued)

Outcome	Child Cohort		
	3-Year-Old Cohort (1st Grade)		
	Mean	Standard Deviation	Reliability
Math Ability	0.79	0.41	NA
Social Studies and Science Ability	0.84	0.36	NA
Promotion	0.92	0.26	NA

NOTE: NA indicates the outcome is not an appropriate candidate for reliability (e.g., single item, index, etc.).

Exhibit 3.16: Components of Variance and ICC's by Cohort for Fall 2002

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	PPVT (Adapted)	1137.49	105.70	135.94	0.18	0.10	82%	8%	10%
3	WJ III Pre-Academic Skills	384.66	14.65	44.84	0.13	0.10	87%	3%	10%
3	WJ III Letter-Word Identification	483.96	5.41	13.25	0.04	0.03	96%	1%	3%
3	WJ III Spelling	574.71	26.86	0	0.04	0.00	96%	4%	0%
3	WJ III Applied Problems	716.37	7.29	14.21	0.03	0.02	97%	1%	2%
3	WJ III Oral Comprehension	150.12	1.77	12.86	0.09	0.08	91%	1%	8%
3	CTOPPP Elision	1530.24	4.65	89.97	0.06	0.06	94%	0%	6%
3	Aggressive Behavior	2.83	0.03	0.11	0.04	0.04	96%	0%	4%
3	Hyperactive Behavior	2.19	0.07	0.08	0.06	0.03	94%	3%	3%
3	Withdrawn Behavior	0.78	0.04	0	0.05	0.00	95%	5%	0%
3	Total Problem Behavior	11.90	0.35	0.40	0.06	0.03	94%	3%	3%
3	Social Competencies	2.25	0	0.03	0.01	0.01	99%	0%	1%
3	Social Skills and Positive Approaches to Learning	3.35	0	0.05	0.02	0.01	98%	1%	1%
4	PPVT (Adapted)	1148.54	86.94	270.32	0.24	0.18	76%	6%	18%
4	WJ III Pre-Academic Skills	368.63	15.36	36.18	0.12	0.09	88%	3%	9%
4	WJ III Letter-Word Identification	603.22	35.59	20.74	0.09	0.03	91%	6%	3%
4	WJ III Spelling	607.21	0	18.02	0.03	0.03	97%	0%	3%
4	WJ III Applied Problems	512.48	12.95	19.65	0.06	0.04	94%	2%	4%
4	WJ III Oral Comprehension	166.06	19.47	17.21	0.18	0.08	82%	10%	8%
4	CTOPPP Elision	1685.45	72.71	38.81	0.06	0.02	94%	4%	2%
4	Aggressive Behavior	2.94	0.10	0	0.03	0.00	97%	3%	0%
4	Hyperactive Behavior	2.25	0.09	0.05	0.06	0.02	94%	4%	2%
4	Withdrawn Behavior	0.92	0.04	0	0.04	0.00	96%	4%	0%
4	Total Problem Behavior	13.07	0.71	0.10	0.06	0.01	94%	5%	1%

Exhibit 3.16: Components of Variance and ICC's by Cohort for Fall 2002 (continued)

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
4	Social Competencies	1.73	0.09	0.04	0.07	0.02	93%	5%	2%
4	Social Skills and Positive Approaches to Learning	2.86	0.17	0.02	0.06	0.01	94%	5%	1%

NOTE: Only continuous outcomes are included in the table.

ICC - Children within Centers = (V2+V3)/(V1+V2+V3)

ICC - Centers within Programs = V3/(V1+V2+V3)

Exhibit 3.17: Components of Variance and ICC's by Cohort for Spring 2003

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	PPVT (Adapted)	982.90	82.39	198.61	0.22	0.16	78%	7%	16%
3	WJ III Pre-Academic Skills	363.64	5.33	19.19	0.06	0.05	94%	1%	5%
3	WJ III Letter-Word Identification	592.53	15.20	33.68	0.08	0.05	92%	2%	5%
3	WJ III Spelling	463.55	7.40	15.51	0.05	0.03	95%	2%	3%
3	WJ III Applied Problems	684.92	28.46	75.46	0.13	0.10	87%	4%	10%
3	WJ III Oral Comprehension	142.80	17.15	44.47	0.30	0.22	70%	8%	22%
3	CTOPPP Elision	1788.10	98.49	273.86	0.17	0.13	83%	5%	13%
3	Aggressive Behavior	2.82	0.05	0.10	0.05	0.03	95%	2%	3%
3	Hyperactive Behavior	2.21	0.06	0.06	0.05	0.03	95%	2%	3%
3	Withdrawn Behavior	0.85	0.02	0	0.03	0.00	97%	3%	0%
3	Total Problem Behavior	12.22	0.31	0.45	0.06	0.03	94%	2%	3%
3	Social Competencies	1.93	0.04	0.02	0.03	0.01	97%	2%	1%
3	Social Skills and Positive Approaches to Learning	2.72	0.25	0.09	0.11	0.03	89%	8%	3%
3	Closeness (parent-reported)	5.21	0.28	0.14	0.08	0.03	92%	5%	3%
3	Conflict (parent-reported)	42.81	0.84	1.16	0.04	0.03	96%	2%	3%
3	Positive Relationships (parent-reported)	54.59	1.87	1.25	0.05	0.02	95%	3%	2%
4	PPVT (Adapted)	947.30	112.78	292.12	0.30	0.22	70%	8%	22%
4	WJ III Pre-Academic Skills	426.93	21.34	16.49	0.08	0.04	92%	5%	4%
4	WJ III Letter-Word Identification	704.32	38.60	34.74	0.09	0.04	91%	5%	4%
4	WJ III Spelling	584.23	21.13	11.18	0.05	0.02	95%	3%	2%
4	WJ III Applied Problems	572.86	38.27	54.58	0.14	0.08	86%	6%	8%
4	WJ III Oral Comprehension	194.73	24.70	87.87	0.37	0.29	63%	8%	29%
4	CTOPPP Elision	1980.17	99.23	282.54	0.16	0.12	84%	4%	12%
4	Aggressive Behavior	2.80	0.13	0.04	0.06	0.01	94%	4%	1%
4	Hyperactive Behavior	2.06	0.20	0.02	0.10	0.01	90%	9%	1%
4	Withdrawn Behavior	0.95	0	0.03	0.03	0.03	97%	0%	3%
4	Total Problem Behavior	12.42	0.94	0.22	0.09	0.02	91%	7%	2%

Exhibit 3.17: Components of Variance and ICC's by Cohort for Spring 2003 (continued)

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
4	Social Competencies	1.65	0.05	0.05	0.06	0.03	94%	3%	3%
4	Social Skills and Positive Approaches to Learning	2.35	0.27	0.12	0.14	0.04	86%	10%	4%
4	Closeness (parent-reported)	4.78	0.64	0.31	0.17	0.05	83%	11%	5%
4	Conflict (parent-reported)	43.48	0.85	0.47	0.03	0.01	97%	2%	1%
4	Positive Relationships (parent-reported)	55.09	3.39	0.70	0.07	0.01	93%	6%	1%

NOTE: Only continuous outcomes are included in the table.

ICC - Children within Centers = (V2+V3)/(V1+V2+V3)

ICC - Centers within Programs = V3/(V1+V2+V3)

Exhibit 3.18: Components of Variance and ICC's by Cohort for Spring 2004

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	PPVT (Adapted)	1038.05	121.11	351.56	0.31	0.23	69%	8%	23%
3	WJ III Pre-Academic Skills	408.23	22.38	42.08	0.14	0.09	86%	5%	9%
3	WJ III Letter-Word Identification	693.02	40.71	92.15	0.16	0.11	84%	5%	11%
3	WJ III Spelling	628.28	41.15	38.86	0.11	0.05	89%	6%	5%
3	WJ III Applied Problems	447.89	14.71	50.26	0.13	0.10	87%	3%	10%
3	WJ III Oral Comprehension	176.76	18.94	68.33	0.33	0.26	67%	7%	26%
3	CTOPPP Elision	2064.51	183.90	321.72	0.20	0.13	80%	7%	13%
3	Aggressive Behavior	2.92	0	0.13	0.04	0.04	96%	0%	4%
3	Hyperactive Behavior	2.16	0.02	0.05	0.03	0.02	97%	1%	2%
3	Withdrawn Behavior	0.78	0.02	0.03	0.05	0.03	95%	2%	3%
3	Total Problem Behavior	12.65	0.06	0.58	0.05	0.04	95%	0%	4%
3	Social Competencies	1.72	0.03	0.02	0.03	0.01	97%	2%	1%
3	Social Skills and Positive Approaches to Learning	2.62	0.06	0.07	0.05	0.02	95%	2%	2%
3	Closeness (parent-reported)	6.48	0.07	0.39	0.07	0.06	93%	1%	6%
3	Conflict (parent-reported)	41.54	0	1.33	0.03	0.03	97%	0%	3%
3	Positive Relationships (parent-reported)	57.36	0	2.06	0.03	0.03	97%	0%	3%
4	PPVT (Adapted)	1105.03	114.23	405.09	0.32	0.25	68%	7%	25%
4	WJ III Pre-Academic Skills	497.80	28.97	37.64	0.12	0.07	88%	5%	7%
4	WJ III Letter-Word Identification	951.47	69.97	83.86	0.14	0.08	86%	6%	8%
4	WJ III Spelling	713.09	39.47	53.29	0.12	0.07	88%	5%	7%
4	WJ III Applied Problems	399.89	11.15	17.42	0.07	0.04	93%	3%	4%
4	WJ III Oral Comprehension	243.08	15.85	90.86	0.31	0.26	69%	5%	26%
4	CTOPPP Elision	2102.11	81.82	243.49	0.13	0.10	87%	3%	10%
4	WJ III Basic Reading Skills	887.29	77.51	82.94	0.15	0.08	85%	7%	8%
4	WJ III Math Reasoning	278.34	10.73	10.95	0.07	0.04	93%	4%	4%
4	WJ III Word Attack	1035.11	94.51	93.70	0.15	0.08	85%	8%	8%
4	WJ III Quantitative Concepts	279.48	18.46	11.31	0.10	0.04	90%	6%	4%
4	School Accomplishments	57.48	2.16	0.24	0.04	0.00	96%	4%	0%
4	Aggressive Behavior	2.94	0.07	0.01	0.03	0.00	97%	2%	0%

Exhibit 3.18: Components of Variance and ICC's by Cohort for Spring 2004 (continued)

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
4	Hyperactive Behavior	2.10	0.12	0.03	0.07	0.01	93%	5%	1%
4	Withdrawn Behavior	0.94	0	0.02	0.02	0.02	98%	0%	2%
4	Total Problem Behavior	12.83	0.53	0.19	0.05	0.01	95%	4%	1%
4	Social Competencies	1.35	0.04	0.01	0.03	0.01	97%	3%	1%
4	Social Skills and Positive Approaches to Learning	2.20	0.11	0.06	0.07	0.03	93%	5%	3%
4	Closeness (parent-reported)	7.13	0.14	0.02	0.02	0.00	98%	2%	0%
4	Conflict (parent-reported)	41.50	2.96	0.38	0.07	0.01	93%	7%	1%
4	Positive Relationships (parent-reported)	58.27	4.06	0.34	0.07	0.01	93%	6%	1%
4	Closeness (teacher-reported)	19.31	0	0.23	0.01	0.01	99%	0%	1%
4	Conflict (teacher-reported)	36.93	1.76	0	0.05	0.00	95%	5%	0%
4	Positive Relationships (teacher-reported)	74.38	2.39	0.36	0.04	0.00	96%	3%	0%
4	ASPI-Aggressive	54.57	0.41	0.45	0.02	0.01	98%	1%	1%
4	ASPI-Withdrawn/ Low Energy	48.32	0.03	0	0.00	0.00	100%	0%	0%
4	ASPI-Shy/Socially Reticent	56.55	0	0	0.00	0.00	100%	0%	0%
4	ASPI-Oppositional	52.31	1.38	0	0.03	0.00	97%	3%	0%
4	ASPI-Inattentive/Hyperactive	68.81	1.26	0	0.02	0.00	98%	2%	0%
4	ASPI-Problems with Structured Learning	106.55	0.14	0	0.00	0.00	100%	0%	0%
4	ASPI-Problems with Peer Interaction	120.29	0.52	1.73	0.02	0.01	98%	0%	1%
4	ASPI-Problems with Teacher Interaction	92.21	0	1.32	0.01	0.01	99%	0%	1%

NOTE: Only continuous outcomes are included in the table.

ICC - Children within Centers = (V2+V3)/(V1+V2+V3)

ICC - Centers within Programs = V3/(V1+V2+V3)

Exhibit 3.19: Components of Variance and ICC's by Cohort for Spring 2005

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	PPVT (Adapted)	583.87	49.76	187.13	0.29	0.23	71%	6%	23%
3	WJ III Pre-Academic Skills	467.49	11.99	56.43	0.13	0.11	87%	2%	11%
3	WJ III Letter-Word Identification	913.13	39.51	110.23	0.14	0.10	86%	4%	10%
3	WJ III Spelling	552.33	8.41	71.72	0.13	0.11	87%	1%	11%
3	WJ III Applied Problems	461.38	8.38	26.53	0.07	0.05	93%	2%	5%
3	WJ III Oral Comprehension	226.26	15.18	60.94	0.25	0.20	75%	5%	20%
3	CTOPPP Elision	1820.94	44.14	170.14	0.11	0.08	89%	2%	8%
3	WJ III Basic Reading Skills	832.55	34.95	94.07	0.13	0.10	87%	4%	10%
3	WJ III Math Reasoning	289.11	4.66	24.87	0.09	0.08	91%	1%	8%
3	WJ III Word Attack	974.14	32.37	85.14	0.11	0.08	89%	3%	8%
3	WJ III Quantitative Concepts	261.38	6.01	27.80	0.11	0.09	89%	2%	9%
3	School Accomplishments	57.13	0.82	2.08	0.05	0.03	95%	2%	3%
3	Aggressive Behavior	2.93	0.04	0.24	0.09	0.07	91%	1%	7%
3	Hyperactive Behavior	2.20	0.02	0.05	0.03	0.02	97%	1%	2%
3	Withdrawn Behavior	0.86	0.03	0.02	0.05	0.02	95%	3%	2%
3	Total Problem Behavior	13.33	0.26	0.67	0.07	0.05	93%	2%	5%
3	Social Competencies	1.71	0.04	0.06	0.05	0.03	95%	2%	3%
3	Social Skills and Positive Approaches to Learning	2.74	0.16	0.15	0.10	0.05	90%	5%	5%
3	Closeness (parent-reported)	6.02	0.44	0.56	0.14	0.08	86%	6%	8%
3	Conflict (parent-reported)	39.06	1.04	1.75	0.07	0.04	93%	2%	4%
3	Positive Relationships (parent-reported)	54.07	1.48	2.13	0.06	0.04	94%	3%	4%
3	Closeness (teacher-reported)	19.75	0.33	0.15	0.02	0.01	98%	2%	1%
3	Conflict (teacher-reported)	45.29	1.50	0	0.03	0.00	97%	3%	0%
3	Positive Relationships (teacher-reported)	93.06	1.17	0	0.01	0.00	99%	1%	0%
3	ASPI-Aggressive	55.71	1.94	0.31	0.04	0.01	96%	3%	1%
3	ASPI-Withdrawn/ Low Energy	43.47	0	0.09	0.00	0.00	100%	0%	0%
3	ASPI-Shy/Socially Reticent	52.44	1.22	0.20	0.03	0.00	97%	2%	0%
3	ASPI-Oppositional	59.52	0.51	1.58	0.03	0.03	97%	1%	3%

Exhibit 3.19: Components of Variance and ICC's by Cohort for Spring 2005 (continued)

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	ASPI-Inattentive/Hyperactive	73.22	1.00	0.54	0.02	0.01	98%	1%	1%
3	ASPI-Problems with Structured Learning	105.56	0.56	2.00	0.02	0.02	98%	1%	2%
3	ASPI-Problems with Peer Interaction	125.32	3.67	1.16	0.04	0.01	96%	3%	1%
3	ASPI-Problems with Teacher Interaction	96.19	0	1.65	0.02	0.02	98%	0%	2%
4	PPVT (Adapted)	701.57	55.00	273.78	0.32	0.27	68%	5%	27%
4	WJ III Pre-Academic Skills	540.28	30.74	53.33	0.13	0.09	87%	5%	9%
4	WJ III Letter-Word Identification	1188.49	67.04	136.76	0.15	0.10	85%	5%	10%
4	WJ III Spelling	596.33	25.32	44.68	0.11	0.07	89%	4%	7%
4	WJ III Applied Problems	344.87	21.14	19.36	0.11	0.05	89%	5%	5%
4	WJ III Oral Comprehension	218.86	28.17	78.81	0.33	0.24	67%	9%	24%
4	WJ III Basic Reading Skills	921.85	49.21	153.48	0.18	0.14	82%	4%	14%
4	WJ III Math Reasoning	265.82	12.79	22.88	0.12	0.08	88%	4%	8%
4	WJ III Word Attack	850.71	35.56	172.50	0.20	0.16	80%	3%	16%
4	WJ III Quantitative Concepts	280.95	9.22	25.54	0.11	0.08	89%	3%	8%
4	WJ III Writing Sample	172.80	12.61	17.45	0.15	0.09	85%	6%	9%
4	WJ III Passage Comprehension	559.06	13.25	54.01	0.11	0.09	89%	2%	9%
4	WJ III Calculation	320.39	15.29	27.94	0.12	0.08	88%	4%	8%
4	WJ III Academic Skills	519.21	26.70	52.97	0.13	0.09	87%	4%	9%
4	WJ III Academic Applications	257.79	16.04	24.12	0.13	0.08	87%	5%	8%
4	School Accomplishments	110.55	3.24	0	0.03	0.00	97%	3%	0%
4	Aggressive Behavior	2.96	0	0.14	0.05	0.05	95%	0%	5%
4	Hyperactive Behavior	2.22	0.12	0.03	0.06	0.01	94%	5%	1%
4	Withdrawn Behavior	0.98	0.01	0.05	0.06	0.04	94%	1%	4%
4	Total Problem Behavior	13.71	0.47	0.47	0.06	0.03	94%	3%	3%
4	Social Competencies	1.51	0.05	0	0.03	0.00	97%	3%	0%
4	Social Skills and Positive Approaches to Learning	2.50	0.17	0.10	0.10	0.04	90%	6%	4%
4	Closeness (parent-reported)	5.87	0.57	0.18	0.11	0.03	89%	9%	3%

Exhibit 3.19: Components of Variance and ICC's by Cohort for Spring 2005 (continued)

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
4	Conflict (parent-reported)	43.55	0.59	1.87	0.05	0.04	95%	1%	4%
4	Positive Relationships (parent-reported)	58.87	1.67	2.30	0.06	0.04	94%	3%	4%
4	Closeness (teacher-reported)	21.25	0.16	0.52	0.03	0.02	97%	1%	2%
4	Conflict (teacher-reported)	40.09	1.64	0.63	0.05	0.01	95%	4%	1%
4	Positive Relationships (teacher-reported)	82.11	2.37	1.81	0.05	0.02	95%	3%	2%
4	ASPI-Aggressive	55.37	0.25	0.86	0.02	0.02	98%	0%	2%
4	ASPI-Withdrawn/ Low Energy	53.28	0.15	0	0.00	0.00	100%	0%	0%
4	ASPI-Shy/Socially Reticent	56.71	0	0	0.00	0.00	100%	0%	0%
4	ASPI-Oppositional	52.43	1.03	0.30	0.02	0.01	98%	2%	1%
4	ASPI-Inattentive/Hyperactive	69.71	0.81	0	0.01	0.00	99%	1%	0%
4	ASPI-Problems with Structured Learning	112.75	1.81	0	0.02	0.00	98%	2%	0%
4	ASPI-Problems with Peer Interaction	119.26	3.12	0.97	0.03	0.01	97%	3%	1%
4	ASPI-Problems with Teacher Interaction	104.00	0	0	0.00	0.00	100%	0%	0%

NOTE: Only continuous outcomes are included in the table.

ICC - Children within Centers = (V2+V3)/(V1+V2+V3)

ICC - Centers within Programs = V3/(V1+V2+V3)

Exhibit 3.20: Components of Variance and ICC's by Cohort for Spring 2006

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	PPVT (Adapted)	625.26	36.67	221.78	0.29	0.25	71%	4%	25%
3	WJ III Pre-Academic Skills	524.95	41.07	42.35	0.14	0.07	86%	7%	7%
3	WJ III Letter-Word Identification	1086.54	125.02	107.06	0.18	0.08	82%	9%	8%
3	WJ III Spelling	545.29	36.26	39.24	0.12	0.06	88%	6%	6%
3	WJ III Applied Problems	402.94	8.94	28.69	0.09	0.07	91%	2%	7%
3	WJ III Oral Comprehension	206.32	17.52	46.15	0.24	0.17	76%	6%	17%
3	WJ III Basic Reading Skills	838.27	98.39	81.22	0.18	0.08	82%	10%	8%
3	WJ III Math Reasoning	279.03	9.10	24.00	0.11	0.08	89%	3%	8%
3	WJ III Word Attack	785.27	78.70	77.29	0.17	0.08	83%	8%	8%
3	WJ III Quantitative Concepts	272.50	8.01	24.68	0.11	0.08	89%	3%	8%
3	WJ III Writing Sample	173.01	23.56	15.03	0.18	0.07	82%	11%	7%
3	WJ III Passage Comprehension	530.58	26.42	51.75	0.13	0.09	87%	4%	9%
3	WJ III Calculation	279.21	14.34	13.04	0.09	0.04	91%	5%	4%
3	WJ III Academic Skills	464.02	44.48	31.05	0.14	0.06	86%	8%	6%
3	WJ III Academic Applications	273.10	15.07	21.73	0.12	0.07	88%	5%	7%
3	School Accomplishments	107.44	0.66	0.20	0.01	0.00	99%	1%	0%
3	Aggressive Behavior	2.96	0	0.24	0.08	0.08	92%	0%	8%
3	Hyperactive Behavior	2.20	0	0.09	0.04	0.04	96%	0%	4%
3	Withdrawn Behavior	0.89	0	0.04	0.04	0.04	96%	0%	4%
3	Total Problem Behavior	14.04	0.01	0.81	0.06	0.05	94%	0%	5%
3	Social Competencies	1.57	0.09	0.06	0.09	0.03	91%	5%	3%
3	Social Skills and Positive Approaches to Learning	2.58	0.05	0.13	0.06	0.05	94%	2%	5%
3	Closeness (parent-reported)	6.51	0.25	0.40	0.09	0.06	91%	4%	6%
3	Conflict (parent-reported)	38.95	1.08	2.99	0.09	0.07	91%	3%	7%

Exhibit 3.20: Components of Variance and ICC's by Cohort for Spring 2006 (continued)

Age Cohort	Outcome Variable	V1 Variance (Children within Centers)	V2 Variance (Centers within Programs)	V3 Variance (Between Programs)	ICC- (Children within Centers)	ICC- (Centers within Programs)	Percent of Child Variance	Percent of Center Variance	Percent of Program Variance
3	Positive Relationships (parent-reported)	53.39	1.72	3.65	0.09	0.06	91%	3%	6%
3	Closeness (teacher-reported)	20.74	0.25	0	0.01	0.00	99%	1%	0%
3	Conflict (teacher-reported)	46.89	2.06	0.62	0.05	0.01	95%	4%	1%
3	Positive Relationships (teacher-reported)	94.05	3.42	0	0.04	0.00	96%	4%	0%
3	ASPI-Aggressive	57.38	1.25	0.94	0.04	0.02	96%	2%	2%
3	ASPI-Withdrawn/ Low Energy	46.07	0	0	0.00	0.00	100%	0%	0%
3	ASPI-Shy/Socially Reticent	52.52	0.22	0	0.00	0.00	100%	0%	0%
3	ASPI-Oppositional	58.02	1.64	1.52	0.05	0.02	95%	3%	2%
3	ASPI-Inattentive/Hyperactive	74.37	0.37	0.24	0.01	0.00	99%	0%	0%
3	ASPI-Problems with Structured Learning	107.56	2.92	0.47	0.03	0.00	97%	3%	0%
3	ASPI-Problems with Peer Interaction	129.49	2.13	3.33	0.04	0.02	96%	2%	2%
3	ASPI-Problems with Teacher Interaction	95.49	2.78	0	0.03	0.00	97%	3%	0%

NOTE: Only continuous outcomes are included in the table.

ICC- Children within Centers = (V2+V3)/(V1+V2+V3)

ICC- Centers within Programs = V3/(V1+V2+V3)

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Chapter 4: Data Collection Procedures

Introduction

Data collection for the Head Start Impact Study began in fall 2002 and continued through spring 2006, following the same children from age of entry into Head Start through the end of 1st grade. Comparable data were collected on children and families randomly assigned to both the Head Start treatment and control groups who were part of both the 3- and 4-year-old study cohorts.

Data collection focused on the full range of comprehensive services and integrated program elements for children and their families that form the cornerstone of the Head Start program and contribute to the child's readiness for and success in school. Data were collected from parents, children, Head Start program staff, other child care providers, and teachers; during the preschool years, observations of classrooms and day care homes were also conducted to provide direct assessments of the quality of both Head Start and other child care settings.

Comparable data collected for both Head Start and control group children consisted of the following:

- **Measures of children's development** that include (1) direct child assessments, (2) parent reports, and (3) teacher/care provider reports. Child outcomes were measured in the key domains of cognitive development (including assessment of skills in the areas of reading, writing, vocabulary, oral comprehension and phonological awareness, and math), social-emotional development, and health.
- **Characteristics and quality of children's home environments** were measured through (1) parental reports of beliefs and attitudes about their child's learning and parental participation in, and satisfaction with, their child's child care experience; (2) family household and demographic information, including parent-child relationships and the quality of the child's home life; (3) parent ratings of their child's behavior problems, social skills, and competencies; (4) parents' perceptions of their child's accomplishments; (5) parents' perception of their relationship with their child; and (6) child and family receipt of a variety of comprehensive services.
- **Characteristics and quality of the primary preschool and child care arrangements** were measured through (1) interviews with center-based directors, (2) surveys of teachers or interviews with care providers, and (3) observations of these settings. Characteristics and quality of school settings were measured through (1) surveys of teachers and (2) by linking schools attended by study children to

annual data collected from every public school in the U.S. by the Department of Education's National Center for Education Statistics (NCES).

Data collection activities for each wave of data collection are summarized in Exhibits 4-1 and 4-2.

Exhibit 4-1. Data Collection Schedule – 3-Year-Old Cohort

	2002-2003 Head Start Year		2003-2004 Age 4 Year		2004-2005 Kindergarten		2005-2006 1st Grade	
	Fall	Spring	Fall	Spring	Fall	Spring	Fall	Spring
Children	✓	✓		✓		✓		✓
Parent/Primary Caregiver[a]	✓	✓	✓	✓	✓	✓	✓	✓
Program Staff/Other Care Provider and Elementary School Teacher		✓		✓		✓		✓
Quality of Care Setting		✓		✓		✓		✓

[a] Primary caregiver data collection in fall '03, 04, and 05, is limited to tracking and confirming the following spring school setting.

Exhibit 4-2. Data Collection Schedule – 4-Year-Old Cohort

	2002-2003 Head Start Year		2003-2004 Kindergarten		2004-2005 1st Grade		2005-2006 2nd Grade	
	Fall	Spring	Fall	Spring	Fall	Spring	Fall	Spring
Children	✓	✓		✓		✓		
Parent/Primary Caregiver[a]	✓	✓	✓	✓	✓	✓	✓	
Program Staff/Other Care Provider and Elementary School Teacher		✓		✓		✓		
Quality of Care Setting		✓		✓		✓		

[a] Primary caregiver data collection in fall '03, 04, and 05, is limited to tracking and confirming the following spring school setting.

Data Collection Staff Structure

Highly experienced and skilled teams of professional researchers, field supervisory staff, and field interviewer/assessors, and classroom observers implemented a comprehensive data collection plan to complete all recruitment, random assignment, data collection, and monitoring/ quality control tasks. Beginning in February 2002, recruitment teams, comprised of senior staff, visited each of the sampled Head Start grantees to enlist program cooperation and participation in the study, and to gather information needed to design random assignment and monitoring procedures, and to gain insight on the state and local context within which the Head Start programs operated. In addition, during data collection, some individuals from these recruitment teams conducted quality control visits and continued responsibility in ensuring elementary school participation as the study children started their early school years.

The critical efforts of these recruitment teams were supplemented by those of local Westat field supervisors, called "site coordinators," who were assigned to a geographic area immediately after sample selection, with many continuing in this role for the duration of the study. The site coordinator's role was a pivotal one in serving as the primary local contact. Site coordinators often laid the groundwork and helped with the preparation for the recruitment team visits, and later with providing additional information and follow-up (e.g., conducting parent/primary caregiver meetings and community meetings to respond to questions or concerns after the recruitment teams' visits).

After each grantee was successfully enrolled in the study, the recruitment teams and site coordinators worked with program administrators to select a program staff person to serve as the "on-site liaison" to work closely with the site coordinator. On-site liaisons and site coordinators worked hand in hand to: implement random assignment; recruit and maintain the cooperation of study participants including parents and their children, teachers, and administrative staff; secure informed consent from parents; make all logistical arrangements for data collection; and assist with tracking study participants over time. These relationships, developed early in the course of the study, were invaluable in ensuring open communications, achieving cooperation and participation, and developing trusting, ongoing working relationships with the sites.

Following the completion of random assignment, and obtaining informed consent from parents, local measurement teams, comprised of interviewer/assessors and observers, under the

supervision of the field site coordinator, completed the various data collection tasks in each of the 25 geographic areas that comprised the study sample for each data collection wave. These data collection tasks included conducting in-person parent interviews, individual child assessments, and distributing and collecting self-administered staff questionnaires. During the preschool years, teams of specially trained observers were responsible for conducting Head Start and non-Head Start classroom and day care home observations to provide assessments of program quality, and site coordinators also conducted interviews with both Head Start and non-Head Start center directors. Throughout the successive waves of data collection, the site coordinators continued to coordinate all data collection activities in the geographic area; tracking study participants; managing field staff, and ensuring quality control.

Staff Training

Beginning in fall 2002, and then each spring thereafter through spring 2006, a single centralized in-person training was conducted in the Washington, DC area for all field interviewer/assessors and site coordinators. At each of the trainings, Westat field staff were trained thoroughly in the administration of all data collection instruments for that wave including administering the child assessment, conducting the parent interview, fielding the self-administered teacher survey and teacher's/care provider's child report form (TCR), and conducting the center director interview. Field staff also were trained in all field procedures including securing parent participation, obtaining informed consent, tracking respondents, building rapport, maintaining cooperation over time, and conducting refusal conversion. This centralized in-person approach ensured standardization of methods and procedures, fostered camaraderie and a shared sense of purpose, and enabled the Project Officer and other federal staff to easily attend.

In addition, for the fall parent updates, telephone conference trainings were conducted on the parent tracking update with a followup telephone conference question and answer session one week later. Then, site coordinators provided additional training for their interviewer/ assessors and also corrective feedback immediately after conducting an edit of the first few instruments.

Also, during each of the first two years of the study, separate centralized in-person training sessions were conducted in the Washington, DC area for field staff observers to train

them in conducting classroom observations in Head Start and other center-based programs. Observers received in-depth training in using a structured classroom observation instrument comprised of a table to record counts of children and adults, a Classroom Observation of Teacher-Directed Activities, the Early Childhood Environment Rating Scale – Revised (ECERS-R), and the Arnett Scale of Lead Teacher Behavior. Training included multiple practice observations in local classrooms, followed by small group interactive training sessions to achieve and ensure the required high degree of reliability. Separate trainings were also conducted to prepare observers to conduct observations in day care homes, substituting the Family Day Care Rating Scale (FDCRS) for the ECERS-R.

For every year of data collection, these training sessions lasted at least five days and as many as nine days, depending upon the data collection tasks for that round of data collection. Given that over time much of the field staff was experienced on the study, and some were with the study since its beginning in fall 2002, the trainings could be structured to include fewer days for experienced returning staff than for new staff. The trainings enabled new staff to benefit enormously from the experienced staff who shared their successful methods and ideas, both in large group training sessions and in the small group interactive practice sessions where we deliberately paired each new staff member with a returning staff member to foster peer learning. New staff attended a one-day preliminary session to provide an introduction to general interviewing techniques and afford an opportunity for experience and practice with the instruments prior to the main combined training conducted over several days for all field staff.

Site coordinators also participated in a separate preliminary set of training sessions led by home office staff. These sessions were geared to developing the management, leadership, and computer skills necessary to the site coordinators' assignments including supervising the interviewer/assessors, overseeing the logistical procedures, and coordinating data collection. In addition to the training they received from the operations director, field director, and other home office staff, site coordinators attended workshops structured for them to share their ideas and successful techniques with each other and with their managers. In addition, time was scheduled for the new interviewer/assessors and their site coordinators to meet and discuss project goals and expectations.

A variety of methods were employed such as interactive large group sessions including video and live demonstrations of assessment administration to provide an overview and detailed training on the various segments of the instruments including rules for administration, coding and scoring, engaging respondents, and providing "neutral praise" that provides encouragement without reacting to the particular response or level of achievement (e.g., "you're doing such a nice job of cooperating"). Small group breakout sessions also were included with round robin practice of the entire assessment and key sections as well as role plays to gain familiarity and proficiency with the instruments. One day of training was devoted to live practice of the child assessment with children with evaluation and corrective feedback provided. Additional evening training sessions also were conducted to provide further training and practice for those who wanted it and those for whom the trainers felt it was necessary.

Additional training sessions and practice with instruments in Spanish was required for the bilingual English/Spanish interviewer/assessors. Experienced bilingual project staff conducted these sessions tailored to the administration of parent interviews and child assessments in Spanish.

Training materials included an agenda of project activities, an interviewer/assessor's manual and training scripts and exercises. The materials were customized for each round of data collection and provided a framework for training, a set of valuable reference materials for staff in the field, and documentation of the procedures and content used during the administration of the instruments. Interviewer/assessors also completed a half day of home study following training to enhance learning from the in-person sessions.

Interviewer/assessors and observers were trained to strictly follow procedures, read questions verbatim, provide "neutral praise" to respondents and adhere to study protocols to ensure that the highest quality data were collected without biasing responses. Practicing the child assessment with an adult partner is quite different from conducting an assessment with an actual child. To ensure high quality, every interviewer/assessor was required to demonstrate proficiency in the child assessment by conducting the assessment with a child of the same age as the study children. We recruited children from the local Washington, DC area to serve as practice subjects at training and found this to be a highly effective tool to gauge interviewer/ assessors' proficiency in administering the child assessment. Only interviewer/assessors who

passed the child assessment live practice session were permitted to conduct child assessments in the field. Similarly, observers visited schools recruited in the local Washington, DC area as training sites for observers to observe and develop inter-rater reliability.

Informed Consent

To secure the cooperation of parents, a complete package of materials and advance publicity was designed to inform potential participants and enlist their participation as early as possible. The materials explained that the grantee was participating in a study and that applying meant that the caregiver was agreeing to random assignment procedures. Separate informed consent was obtained to participate in subsequent data collection (described below). These materials enlightened parents (prior to and/or during the application process) regarding the key elements and benefits of the study; notified them of the various incentives that would be provided for their cooperation over time, and informed them of their role in completing interviews and aiding study staff in locating and securing the participation of child care providers. Recruitment teams and site coordinators met with grantees, parents, and staff to explain critical information to potential participants; highlight reasons to participate in the study; explain the process and importance of random assignment for the study design; and describe the implications of this process for children and families assigned to the treatment and control groups.

Following random assignment in late summer and early fall 2002, parents were immediately contacted to notify them of their selection, and to secure written informed consent. At the outset, consent was obtained for the duration of the study with parents agreeing to their child's participation and assessment, as well as to allowing the study team to contact their child's care providers/teachers to gain their cooperation, and to obtain important locator and tracking information to help locate the family in the future, particularly if the family moved. Site coordinators met with the parents/primary caregivers in group settings, (e.g., in Head Start and other child care centers when feasible), and individually as necessary, to explain the study, respond to questions, read the informed consent letters as necessary, and obtain the parents'/primary caregivers' signed informed consent. Field staff used a number of aids to inform parents, encourage their participation, and gain informed consent including a script with a brief statement explaining the purpose and importance of the study and a description of the

information to be collected, assurances that participation is voluntary and information collected will be kept private, and a description of the cash incentive; a study brochure and a study information sheet with frequently asked questions and answers; as well as procedures to guide the determination of whether the person contacted is the parent/primary caregiver, and therefore, the appropriate respondent.

The informed consent letters summarized the purpose, scope, and importance of the study and described the activities as well as the incentives for participating. The letters included a statement that participation is voluntary and an assurance that respondents' participation would not result in a loss of any current benefits they might have. The letters also included an assurance of privacy. We used several approaches that enhanced our ability to secure parents' consent and gain continued participation of families. These included incentives for participation, assurances of privacy, and an emphasis on the importance of parents' sharing their experience to further improve early childhood programs. For those from whom consent was not obtained prior to the start of data collection in fall, field staff obtained informed consent in person prior to conducting the parent interviews or child assessments in fall or spring. Securing blanket informed consent at the outset of the study was a major step toward maintaining high response rates.

Field interviewers/assessors and site coordinators were thoroughly trained in strategies for gaining and maintaining cooperation. Field staff members were trained in identifying, understanding, and responding appropriately and effectively to reasons why parents may be hesitant to cooperate. They were taught strategies to reassure the parents and ensure their cooperation including efforts to find out why the respondent was reluctant and to address these concerns wherever possible. Just as one respondent differs from another, the reasons for refusals are many and varied so it was necessary to train field staff to become sensitive to how firm a "no" they are receiving and sense the reasons behind the hesitancy to develop ways to reassure the respondent and gain cooperation.

Data Collection Procedures by Respondent

Collecting Parent/Primary Caregiver Data

Beginning with baseline in fall 2002, and then each spring thereafter until the child's 1st grade year, interviewer/assessors conducted face-to-face interviews, approximately one-hour in length, with a parent/primary caregiver living with and responsible for raising the study children. Questions were included to obtain information about the characteristics and involvement of other family members as well. Interviews typically were conducted in the family's home, or alternatively, if the parent preferred, in a suitable public location such as a library.

In addition, to keep the families actively involved and to continue to track the families effectively, each fall beginning in fall 2003, site coordinators and interviewer/assessors conducted short telephone, and when necessary, in-person parent update interviews, collecting tracking information and the child's current school and/or care setting information to provide a basis for the spring data collection. We used a number of approaches to strengthen and maintain the parents' commitment to continuing in the study. These included: maintaining frequent in person, telephone and mail contact with the families coupled with providing monetary incentives.

Parent interviews were conducted in the languages of the respondents and were available in both English and Spanish versions. Bilingual interviewers were hired and for most of the languages, including Spanish, Creole, Cantonese, etc., interviewers fluent in the language conducted the interview. When this was not the case, we enlisted the aid of an interpreter to assist an English-speaking interviewer trained to conduct the interview.

Collecting Child Assessment Data

Children were assessed individually in fall 2002 and then each spring through 1st grade using a battery of child measures (see Chapter 3 in this volume). The assessments were designed to provide direct measures of how well Head Start and non-Head Start preschool programs, or other child care, are achieving the goal of assisting children to be physically, socially, and educationally ready for school. The assessment battery was composed of a short series of tasks that varied over time consistent with the child's age. Children who did not advance to the next

level of schooling with their peers, or who were advanced, continued to be assessed with the instruments for their cohort group.

During the preschool years, the 45 - 60 minute child assessment battery was typically administered one-on-one by specially trained assessors in the child's "main" care setting, i.e., where the child spends the most time Monday through Friday between the hours of 9 a.m. and 3 p.m. When the children attained school age, the assessments were typically conducted in the child's home or if the parent preferred, in a suitable public place.

To determine the appropriate language of assessment, at the time of baseline in fall 2002, the interviewer/assessor asked the main care provider a series of three questions (see Chapter 3). For children for whom Spanish was the appropriate language, a bilingual interviewer/assessor administered the assessment battery in Spanish and also administered two subtests in English, i.e., the Peabody Picture Vocabulary Test (adapted) (PPVT) and the Woodcock-Johnson III Letter-Word Identification. Subsequently, each spring, the children assessed in Spanish in fall 2002 were assessed primarily in English, along with the continued administration of two Spanish language measures: the Test de Vocabulario en Imágenes Peabody (TVIP) and the Batería Woodcock-Muñoz Identificación de Letras y Palabras. One exception is Puerto Rico where, because instruction is in Spanish, all children were assessed only with the complete Spanish battery each spring for the duration of the study.

In fall 2002, for children who could not be assessed in either English or Spanish, a bilingual interviewer/assessor or an interpreter for the child's language were used. The interviewer/assessor (or interpreter) used the English assessment booklet, translated the instructions into the child's language, and administered four subtests: McCarthy Draw-A-Design, Color Names and Counting, Leiter-R-Adapted, and Story and Print Concepts. These four tests do not require the child to be able to speak English. Those subtests that would require the child to speak English were not administered to this population. Subsequently, for the spring assessments, these children were tested in English.

Collecting Data from Program Staff, Teachers and Child Care Providers

During the preschool years, Head Start and preschool program teachers as well as other child care providers (other than parents/other primary caregivers) were surveyed each spring, and

center directors were also interviewed on an annual basis. During the elementary school years, kindergarten and 1st grade teachers were surveyed, again in the spring. Parents and primary caregivers played a key ongoing role in identifying the child's care providers and granting permission for us to contact them. Parents were offered additional incentive payments for contacting the child care providers and securing their cooperation. Providers who participated also were offered a monetary incentive.

Both preschool and elementary school teachers completed self-administered paper questionnaires including a teacher survey, as well as an individual child rating form, the teacher's/care provider's child report (TCR) for each study child in the teacher's class. Only the lead teacher in the study child's classroom was surveyed. The teacher surveys required approximately 30 minutes and were available in both English and Spanish. The Spanish version was used largely in Puerto Rico, but also, when necessary on the mainland. The teacher surveys included questions about the center or school, questions about the class, and questions about the teacher, and were designed to gather information about school settings, teachers' beliefs about how children learn, levels of education, and job satisfaction.

During the preschool years, if the child was not in a center or preschool classroom, but was cared for by another child care provider other than the parent or primary caregiver, at least five hours per week between the hours of 8 a.m. and 6 p.m., Monday through Friday, in-person interviews were conducted requiring approximately 30 minutes with that care provider, and the care provider was also asked to complete an individual child rating form, the teacher's/care provider's child report (TCR). For children in multiple arrangements that met these criteria, the following hierarchy was used to prioritize the care setting and within that, the care provider to interview: (1) day care home with non-relative; (2) day care home with relative; (3) child's home with non-relative; and finally, (4) child's home with non-parental relative. The interview included questions on the number of children in the care setting, types of child activities used, beliefs on how children should be taught and managed, options for parent and family involvement, staffing, and respondent demographic information. While not all of the child's care providers were directly interviewed, information was obtained from the parent/primary caregiver on all the settings in which the child was placed.

As noted above, teachers and care providers also were asked to complete a TCR. The TCR is an individual child rating form that asks about the child's development and behavior. The following scales are used: teacher/provider relationship with child, classroom behavior and conduct, problem solving and initiative, social relationships, creative representation, music and movement, language ability, and mathematical ability. The child rating forms required approximately five – ten minutes per form and were available in both English and Spanish versions. A separate booklet was completed by the lead teacher in the child's classroom or the care provider each spring during the preschool, kindergarten and first grade years.

During the preschool years, both Head Start and non-Head Start center directors were interviewed in person to obtain information on the operation and quality of Head Start and non-Head Start center-based programs. Issues addressed in this interview included: staffing and recruitment, teacher education initiatives and staff training, parent involvement, curriculum, classroom activities and assessment, home visits, kindergarten transition, and demographic information about the director.

Assessment of Quality of Care Settings

During the preschool years, direct observations of quality and care settings were used for children in center-based and family day care home programs, including those participating in Head Start. These tools provided direct measures of the extent to which Head Start centers, and other childcare programs, employed skilled teachers and provided developmentally appropriate environments and curricula for their pupils. Trained observers conducted observations in classrooms and centers attended by the sampled children. Observers spent enough time in each class to ensure observation of a major portion of the daily schedule and a variety of classroom and center activities. The observers used standardized observational methods and coding schemes that have been widely used in child development research and whose utility has been proven in previous large-scale studies. These include: the Early Childhood Environment Rating Scale (revised) (ECERS-R), the Classroom Observation of Teacher-Directed Activities Checklist, the Arnett Scale of Teacher/Provider Behavior, and the related Family Day Care Rating Scale (FDCRS) for observations in non-center-based settings.

Children were exposed to a wide variety of types of care settings selected by their parents in addition to, or in lieu of, Head Start. Consequently, it was necessary to arrive at a means of

carefully targeting and limiting the number of care settings to be assessed to a maximum of one per child with priority given to those programs where the child was in care at least five hours per week between the hours of 8 a.m. to 3 p.m., Monday through Friday. For the Head Start group, Head Start (regardless of full day or part day) was considered the primary setting assessed or the "focal" setting. For all others, the following hierarchy was used: (1) center-based program, (2) day care home with non-relative; (3) day care home with relative; (4) child's home with non-relative; and finally, (5) child's home with non-parental relative. While children may spend significant portions of their time in care settings other than those being assessed for quality, and these may contribute in important ways to the child's experience and growth, information on these settings was obtained solely from the parent interview.

Classroom observations were not conducted in elementary schools, but instead information on school quality was collected from the teacher questionnaire, (e.g., years of teaching experience, years of schooling, number of hours of in-service training), and through secondary data sources (e.g., percent of children eligible for free or reduced-price lunch, percent of children in the children's schools who scored at the "proficient level" on state math and reading assessments, class-size ratio, etc.).

Privacy

Protecting and maintaining the privacy of respondents and their data were primary concerns and were treated very seriously. Parent/primary caregiver respondents received assurances that "all information collected during the study will be kept private except as required by law." This assurance was included in the informed consent letters the parents/primary caregivers signed to participate in the study, and was reiterated in the letters they received to announce the beginning of each data collection period with similar statements incorporated in the parent interviews. Teachers and administrators also received assurances of confidentiality. Included in the cover letters for the teacher questionnaires was this statement: "All responses are treated with strict confidentiality and members of the study team are committed to this. To ensure confidentiality, survey results will not be reported for any individuals, agencies, centers, preschool programs, child care providers, or schools." In addition, the Head Start Impact Study "Study Information Sheet," distributed to parents and other respondents to answer some frequently asked questions included this assurance: "The confidentiality of each participant is

carefully protected under federal law. No information will be linked to a specific child, parent, or staff member. No information will be released about individuals or child care providers. All information will be held in strict confidence and will be protected."

Staff were required to sign a confidentiality pledge stating that no data would be released to unauthorized personnel. All field data collectors were required to sign Westat's Fieldworker Code of Conduct and Assurance of Confidentiality form prior to each data collection period. Protecting and maintaining confidentiality of respondent data was also a topic covered in depth in the training manual and at interviewer training to have interviewers understand and apply ethical principles and practices when conducting their work, provide historical context, and raise interviewers' awareness of the importance of informed consent and confidentiality. Any interviewers found to violate confidentiality are subject to immediate dismissal as described in the Fieldworker Employee Guide. In addition, all Westat home office professional staff were required to take Westat's online course on the protection of human subjects. This online course emphasizes the protection of respondents as human subjects and the serious obligation of protecting the confidentiality of respondent data. Quality control monitors and site coordinators reinforced and enforced confidentiality standards and instructions in the field. A special version of the online course that includes computer data security procedures was required study for all programming staff.

Access to Westat facilities is controlled at all times through the use of magnetic key cards. Access to the computer centers is also controlled by the key card entry system, with limited access privileges for designated operations and project support staff only. Special secured areas are established for sensitive data processing functions such as storing and printing of confidential data based on project requirements.

Data collected in hard-copy form are kept in a locked field room accessible only by authorized project staff. Signs restricting access are posted at the entrances to secured data processing areas. Likewise, system-generated output containing confidential data is stored in locked areas until no longer needed and is disposed of in accordance with project requirements. Access to secure computer systems is password protected with all electronic data (including records with identifying information) stored on password-protected servers and accessible only by authorized research and programming staff.

Several steps are taken to prevent the loss or corruption of data in case of equipment or facility failure. First, users are instructed to store all data files on network server directories rather than local PC hard disk drives. Second, Westat's Computer Operations staff backup all server-based storage to tape on a daily basis as well as performing a full disk backup once a week with an additional backup created every fourth week and retained for one year. All backup tapes are removed daily from Westat's premises and transported in secure containers to an off-site storage facility that specializes in transporting and storing electronic media. Tape identifiers for all backup tapes are maintained in a central tape management system for easy reference and retrieval.

Interviewers agree to keep all case record folders with names and addresses and all instruments in a secure place in their home. While in the field they are required to keep all study materials with them at all times.

Interviewer/assessors and site coordinators are also trained on proper procedures to communicate confidential information securely, including using ID numbers instead of respondent names in email messages and password protecting files that contain respondent names and addresses. Field staff returned hard copy completed instruments to the home office via Federal Express. All completed hard-copy instruments are kept in a locked field room accessible only by authorized project staff. Data that are transmitted are encrypted before being transmitted to the home office so that all data are secure and cannot be accessed even if the data were intercepted in the transmission. All electronic data (including records with identifying information) are stored on password-protected servers and accessible only by authorized research and programming staff.

Westat further protects the privacy of families by making sure that interviewer/assessors do not interview families whom they know. If interviewer/assessors encounter a family with whom they are acquainted (other than from a previous round of data collection), the case is transferred to another interviewer/assessor. In addition, Westat had a policy of not hiring Head Start staff as interviewer/assessors to minimize the likelihood of previous acquaintance with study families.

Incentives

Incentives provided a valuable and effective mechanism for encouraging parents/primary caregivers, children, teachers, and other care providers to participate initially and to maintain that participation throughout the longitudinal study. For this study, the children received small gifts including stickers, notepads, and pencil sharpeners following their completion of the child assessments. Parents/primary caregivers received $20 in cash following each in-person interview, along with an additional bonus payment during the last round of data collection of $25 if the family participated in all previous rounds of data collection. Parents received a check or money order for $20 for the short tracking telephone update. These incentives encourage cooperation and participation, emphasize the importance of the study and underscore the value of respondents' participation in it.

Teacher incentives also proved to be very effective and consisted of a graduated cash incentive for completing the teacher survey and TCRs depending on the number of child forms completed: $15 for the teacher survey and 1-3 child forms; $25 for the survey and 4-10 child forms; or $35 for the survey and 11+ child forms. Most teachers received an incentive of $15. For classroom and day care home observations, center directors and day care home providers received a check in the amount of $25. Incentives were not offered for the center director interview, as a high level of cooperation was obtained without offering an additional incentive for interviewing administrators at this level.

Tracking

Tracking is a critically important part of a longitudinal study such as the Head Start Impact Study. Once a child is selected into the study, the child remains in the study, and consequently, must be traced throughout the course of the study. The collection of data other than the parent interviews was, however, limited for cost purposes to all children who remained in their original area or who moved within a 50-mile radius of the sampled Head Start centers. When a family moved to the geographic area of another site, the case was transferred for purposes of completing data collection there. Staff continued to trace children regardless of how far away they moved, and when the family was located outside the 50-mile radius of any of our sites, the parent interview was conducted by telephone when possible. Although families may

move far away from an area, they frequently return, and therefore, despite the fact that we may not have complete data for all waves of data collection, we did endeavor to collect data from families during as many waves as possible.

Initial contact information was obtained for the parent/primary caregiver from the Head Start program applications submitted for admission to Head Start in fall 2002. Interviewers/ assessors and site coordinators updated contact information supplied by the parent/primary caregiver during each round of data collection in the spring parent interview and during the fall parent update. Maintaining contact with families twice a year to update contact information for the parent/primary caregiver and up to three contacts proved a very effective strategy both for maintaining parents' interest and participation in the study and for locating and contacting families in successive rounds. The most recent information was made available to field staff for the following round of data collection. Field staff were trained in maintaining professionalism and adhering to the required confidentiality rules while tracing.

If the family could not be reached easily by telephone and a move was suspected, field staff followed a number of leads, narrowly focused at first and then expanding as needed. Field staff utilized telephone directory assistance, called the contacts supplied by the parent/primary caregiver, conducted internet searches, visited the last known address for the family to check with primary sources in the neighborhood, and when necessary, contacted additional secondary local sources such as the post office to endeavor to locate the family. When feasible, long distance tracing efforts were coordinated between teams (i.e., when a family moved to the geographic region within or close to another study site).

Quality Control

Findings from research are dependent on the quality of the data collected. Therefore, procedures were built into every step of this national study to ensure the highest quality data possible. All measures and procedures were reviewed and tested multiple times. Project staff and field staff were selected based on their skills and experience in conducting this type of research. To provide a solid base of knowledge and skills necessary to be successful in collecting the data staff were provided detailed training materials with a consistent set of definitions and rules as well as substantial opportunities at training for practice in applying these

rules and definitions. High quality data are the result of well trained and skilled interviewers and observers.

As noted above, for every year of data collection, intensive in-person trainings were conducted for as many as nine days, depending upon the data collection tasks for that round. Additional days of training were provided for new interviewers. Interviewers and observers—both new and returning—were constantly evaluated during training for their strengths and weaknesses and extra evening sessions were required for those who needed additional practice.

Out in the field, once the data were collected, interviewer/assessors and observers were trained to take the next step in the quality control process: reviewing and editing all completed data collection forms for completeness and clarity. In addition, site coordinators carefully edit and review the booklets to identify any errors or issues as soon as possible and avoid ongoing problems. Field staff also are trained to record notes as necessary to explain unusual situations. When the data collection forms and instruments are received at the home office, they are edited again by our data processing staff. If necessary, additional coding or scoring also is done at this time, and site coordinators are contacted to resolve inconsistencies or other questions that arise during the home office review.

In addition to all of the other quality control measures used on this project, in fall 2002 and each spring thereafter, project staff trainers conducted quality control visits to the sites in the study. The purpose of these visits was to ensure that field data collectors used and maintained consistent data collection techniques, administered the instruments accurately, and scored the instruments correctly and reliably. All new interviewers/assessors and observers and some returning field staff had quality control visitors. These on site evaluations were invaluable in correcting small errors before they became ingrained. Inter-rater reliability was monitored during the training sessions and in the field by quality control visitors.

Response Rates

Response rates are presented in Chapter 2.

Chapter 5: Impact Analysis Methods

This chapter describes the procedures used to calculate and test the statistical significance of all estimates of impact on child and parent outcomes presented in the *Head Start Impact Study Final Report* (U.S. Department of Health and Human Services, January 2010), providing greater detail than what readers can find in Chapter 2 of that report. This chapter is organized into nine sections: Section 1 summarizes all of the outcome measures used in the impact analysis; Section 2 describes the baseline variables used as covariates in the analysis, including imputation for fall 2002 item and instrument nonresponse. Section 3 gives a brief description of the analysis weights (more detail can be found in Chapter 2); Sections 4 and 6 describe the procedures used to calculate annual impact estimates both for the full sample and for selected subgroups; Section 5 discusses procedures for calculating "impact on treated (IOT)" estimates, i.e., impact estimates for those children who actually participated in Head Start; Section 7 describes the methods used in the longitudinal, repeated measures, analysis to estimate Head Start impacts on children's cognitive growth. Finally, Section 8 notes how the analysis methods in this report differ from those in the previous Interim Report (U.S. Department of Health and Human Services, 2005), for a small number of situations for which technical improvements were possible.

Outcome Domains and Measures

A wide variety of data sources and measures are used in this report to assess the impact of Head Start on fostering and enhancing child development, including direct child assessments, parent/primary caregiver interviews, interviews with providers of early care services used by participating study children, observations of children's early care settings, and information provided by kindergarten and 1st grade teachers. Outcome measures span the cognitive, social-emotional, parenting, and health domains, as described in Chapter 3. The baseline measures indicated were collected in fall 2002; outcome information was collected in spring 2003, 2004, 2005, and 2006. Both study cohorts—children who entered Head Start and the research sample at age 3 and those entering at age 4—were followed through the end of 1st grade; as a result, data for the older cohort end in spring 2005 while the younger cohort is followed until spring 2006. Exhibit 5.1 provides a summary of all the outcome measures and a full description of these measures is found in Chapter 3.

Exhibit 5.1: Summary of the HSIS Measures by Domain and Data Collection Period

Measure	Preschool Year(s)	Kindergarten	1st Grade
COGNITIVE			
Language and Literacy			
Peabody Picture Vocabulary Test (PPVT) (Adapted)*	X	X	X
Woodcock-Johnson III Letter-Word Identification*	X	X	X
Woodcock-Johnson III Spelling*	X	X	X
Woodcock-Johnson III Oral Comprehension*	X	X	X
Woodcock-Johnson III Pre-Academic Skills*	X	X	X
Color Identification*	X		
Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP) Elision*	X	X	
Letter Naming	X	X	
Woodcock-Johnson III Word Attack		X	X
Woodcock-Johnson III Basic Reading Skills		X	X
Woodcock-Johnson III Academic Applications			X
Woodcock-Johnson III Academic Skills			X
Woodcock-Johnson III Passage Comprehension			X
Woodcock-Johnson III Writing Samples			X
Spanish Language and Literacy			
Test de Vocabulario en Imágenes Peabody (TVIP) (Adapted)*	X	X	X
Batería Woodcock-Muñoz Identificación de letras y palabras*	X	X	X
Pre-writing			
McCarthy Draw-a-Design*	X		
Parent-Reported Literacy			
Parent-Reported Emergent Literacy Scale (PELS)*	X		
Math Skills			
Woodcock-Johnson III Applied Problems*	X	X	X
Counting Bears*	X		
Woodcock-Johnson III Quantitative Concepts		X	X
Woodcock-Johnson III Math Reasoning		X	X
Woodcock-Johnson III Calculation			X
School Performance			
School Accomplishments		X	X
Promotion (Parent-reported)		X	X
Language and Literacy Ability		X	X
Math Ability		X	X
Social Studies and Science Ability		X	X

Exhibit 5.1: Summary of the HSIS Measures by Domain and Data Collection Period (continued)

Measure	Preschool Year(s)	Kindergarten	1st Grade
SOCIAL-EMOTIONAL			
Parent-Reported			
Aggressive Behavior*	X	X	X
Hyperactive Behavior*	X	X	X
Withdrawn Behavior*	X	X	X
Total Problem Behavior*	X	X	X
Social Competencies*	X	X	X
Social Skills and Positive Approaches to Learning*	X	X	X
Closeness	X	X	X
Conflict	X	X	X
Positive Relationships	X	X	X
Teacher-Reported			
ASPI-Aggressive		X	X
ASPI-Inattentive/Hyperactive		X	X
ASPI-Withdrawn/ Low Energy		X	X
ASPI-Oppositional		X	X
ASPI-Problems with Peer Interactions		X	X
ASPI-Shy/Socially Reticent		X	X
ASPI-Problems with Structured Learning		X	X
ASPI-Problems with Teacher Interaction		X	X
Closeness		X	X
Conflict		X	X
Positive Relationships		X	X
HEALTH			
Parent-Reported			
Child Received Dental Care*	X	X	X
Child Has Health Insurance Coverage*	X	X	X
Child's Overall Health Status Is Excellent/Good*	X	X	X
Child Needs Ongoing Care*	X	X	X
Child Had Care for Injury Last Month*	X	X	X
PARENTING PRACTICES			
Parent Spanked Child in Last Week*	X	X	X
Parent Used Time Out in Last Week*	X	X	X
Parent Read to Child in Last Week*	X	X	X
Parental Safety Practices Scale*	X	X	X
Family Cultural Enrichment Scale*	X	X	X
Parenting Style: Authoritarian+	X	X	X
Parenting Style: Authoritative+	X	X	X
Parenting Style: Neglectful+	X	X	X
Parenting Style: Permissive+	X	X	X

Exhibit 5.1: Summary of the HSIS Measures by Domain and Data Collection Period (continued)

Measure	Preschool Year(s)	Kindergarten	1st Grade
Teacher-Reported			
School Contact and Communication		X	X
Parent Participation		X	X

NOTE: * indicates baseline measures.

+ indicates that the data is only available during the age 4 year for the 3-year-old cohort children. The data is available for all children during the kindergarten and 1st grade years.

Background Measures Used in the Analysis

The background measures of child and family characteristics collected in fall 2002 play several roles in the analysis. They provide descriptive profiles of the population studied and serve as covariates in the impact analysis to help explain child-to-child variation in outcomes and thereby reduce uncertainty in (i.e., increase the statistical precision of) the impact estimates. They also provide the information needed to form subgroups of Head Start children and families of special policy interest, such as bilingual children or families in particular racial/ethnic groups (see section on subgroup analysis). Baseline versions of outcome measures (see Exhibit 5.1) provide additional explanatory power in the analysis; and time-to-assessment variables can reduce bias due to unequal maturation of treatment[13] and control group children for outcome measures collected at somewhat different times each spring for the two groups. The following discussion describes the selection and use of demographic covariates, baseline test scores, and imputation for item and instrument nonresponse for the demographic covariates and fall baseline scores.

Adding Demographic and Time Covariates

While an intact randomized sample and complete outcome data ensure that no systematic biases enter into the simple difference-in-mean estimates of Head Start's impact, more sophisticated analysis methods provide further advantages. In addition to assignment to the Head Start treatment group, other factors such as a child's background and family characteristics and the initial starting points for the key outcome measures may influence her/his outcomes in

[13] In the *Head Start Impact Study Final Report*, the treatment group is the Head Start group.

later years. If these factors can be included in models that "explain" child outcomes as the joint result of Head Start access, demographic background characteristics, and "pre-test" values of the outcomes, uncertainty about the process used to generate outcomes will decline. As a result, the variance of the Head Start impact estimates will be reduced and the chances of detecting a statistically significant Head Start impact on the outcomes of interest will increase. Correspondingly, the study will be able to detect smaller impacts with equivalent power (power is the probability of correctly rejecting the null hypothesis that there is no significant Head Start impact), known as "minimum detectable effects," as additional factors are taken into account.

The background variables used in this report for this purpose – as covariates in the impact regressions – were selected in four stages, starting with a focus on the four different outcome domains (cognitive, social-emotional, health, and parenting):

- Specification of the likely predictors of child and family outcomes for each domain, based on past research and the set of child and family background measures collected by the study in fall 2002.
- Merger of the four sets of predictors (one for each outcome domain) into a single comprehensive list.
- Removal of covariates or collapsing of covariate categories whose role in the regression equations is unstable due to small cell sizes or high correlation with another covariate.[14]
- Removal of covariates whose measured values may have been affected by the group to which a given child was randomly assigned (i.e., treatment or control) due to the timing of baseline data collection (see discussion below).

These steps resulted in a single set of covariates included in all the impact regressions (see Exhibit 5.2 with exceptions noted) that take account of child and family demographic characteristics. Each demographic variable used as a covariate is posited to relate to the outcomes in a linear fashion. For those covariates that create two-way categorizations of the children (e.g., gender), this reduces to a simple shift parameter in the average outcome level between the two groups.

[14] Unstable coefficients arise for a variety of reasons, most often because a categorical variable has zero or very few observations in one of its cells. Unstable standard error estimates can also occur when some of the replicate subsamples used to calculate variances have zero observations.

Exhibit 5.2: Demographic and Time Variables Included in the Statistical Models Estimating the Impact of Head Start

Child Covariates
Child Gender
Child Age at Spring Assessment
Child Race/Ethnicity (White/Other, Black, Hispanic; all models except for cognitive outcomes for the Spanish-English language group, and models of parenting and health outcomes)
Fall Test Language (English vs. Spanish/Other)
Number of Weeks Elapsed between Sept 1, 2002 and Fall Testing (for child assessment outcomes)
Parent Covariates
Primary Language Spoken at Home (English vs. Spanish/Other)
Primary Caregiver's Age as of Sept 1, 2002
Both Biological Parents Live with Child
Biological Mother is a Recent Immigrant
Mother's Highest Level of Educational Attainment (less than High School, High School, beyond High School)
Mother's Marital Status (Not Married; Married; Separated, Divorced, or Widowed)
Mother Gave Birth to Study Child as a Teenager (Age $\leq$ 19)
Number of Weeks Elapsed between Sept 1, 2002 and Parent Interview (for parent outcomes)

Adding Controls for Fall 2002 Outcome Measures

The "pre-test" version of the outcome variable (collected in fall 2002) was added to each impact regression to help explain child outcomes and thus increase the precision of the estimated impacts of Head Start. For example, a child's cognitive abilities measured at the beginning of her or his Head Start enrollment (e.g., his/her fall 2002 PPVT-III score) strongly predict her or his cognitive abilities at the end of a year in the program (or in the control group) and continue to have strong predictive power in later years. Controlling for pre-test levels of spring outcomes may also remove potential differences between the Head Start group and control samples due to nonresponse in the spring data collection that is not captured by the nonresponse adjustment to the analysis weights (see Chapter 2). However, the fall 2002 pretest measures were not entered directly in the model, but in a centered or "residualized" form as explained below.

Addressing Possible Early Impacts

Most of the fall 2002 data on children and families in the study were collected during a three-month period from October 2002 through December 2002 (with most completed by mid-November, see Exhibit 5.3) at a considerable lag from random assignment (which took place from May to September 2002).

Exhibit 5.3: Percent of Treatment and Control Children Assessed by Month of Assessment

Cohort and assignment group	October	November	December	January	Total
3-year-olds					
Treatment	37.0	39.1	18.2	5.7	100%
Control	23.6	42.1	24.0	10.3	100%
4-year-olds					
Treatment	36.2	38.9	21.1	3.8	100%
Control	22.0	41.9	27.7	8.4	100%

As a result, there is a possibility that Head Start had some impact on these measures. It was not feasible to conduct parent interviews and administer child assessments prior to randomization in this study due to a confluence of circumstances. Notification of acceptance into or exclusion from the Head Start program needed to occur in the spring and summer of 2002, as it does every year, to allow applying families to plan ahead and if necessary make alternative arrangements. Once a child was deemed eligible for the program, postponing random assignment long enough for parent interviews and extensive in-person assessment of children to take place first would have imposed an unacceptable hardship on families and on Head Start agencies left wondering which children they would serve. Placing data collection ahead of eligibility determination would have resulted in many costly interviews and assessments being conducted for children and families who ultimately proved ineligible for Head Start and of no relevance to the study. This meant that only those measures needed for eligibility determination and random assignment itself were collected for individual families and children prior to randomization. These include the child's gender, age, and race/ethnicity. These variables come from rosters completed by program intake staff prior to random assignment based on program application forms.

Additional demographic variables, though collected some days or weeks after random assignment, could not plausibly have been affected by Head Start in so short a time. These variables include the language used to assess the child at baseline, mother's age, mother's highest level of education, mother's marital status, whether the mother was a teenager at the time of the birth of the sampled study child, whether both biological parents live with the study child, whether the child's mother was a recent immigrant, and the primary language spoken at home.

However, the cognitive, social-emotional, health, and parenting measures used as outcomes could have been affected by Head Start very early in children's participation in the program. For this reason, we do not want to include the fall 2002 versions of these variables in the impact model in their original form. If impacts of Head Start occurred quickly in fall 2002, inclusion of the unadjusted "pre-test" variables in the impact equation would attenuate impact estimates. That is, Head Start would not receive full credit for the impacts it achieved in subsequent springs because the portion of the impact achieved prior to fall 2002 data collection would be removed by the added covariates.[15]

To avoid this problem, all fall 2002 "pre-test" measures of outcome variables used as covariates are "residualized" before inclusion in the impact regressions. The "residualization" procedure removes any systematic differences between treatment and control group levels in the fall measures, including those potentially due to Head Start's impact. For a given age cohort, it subtracts the mean level of the fall measure for the entire treatment group from each individual treatment group member's value of that measure and does the same (using the control group mean) for members of the control group.[16] The resulting mean-deviated variables are used as covariates in spring impact regressions rather than the original fall variables.[17] Since the means

[15] The measure of program impact from the regression model—the coefficient on the variable indicating membership in the Head Start group—will include only that portion of the overall spring difference between treatment and control groups *not accounted for by other variables in the model*. Fall measures that are systematically higher (or lower, for factors that Head Start participation might reduce such as parental use of physical discipline) for the treatment group than for the control group, and that predict child variation in spring outcomes, will account for some of the systematically higher (or lower) spring outcomes for the Head Start group, thereby reducing the size of the coefficient measuring program impact.

[16] In practice, this procedure was accomplished by regressing the fall measure on an intercept and a dummy variable for membership in the treatment group, using the same sample and analysis weights as the spring impact regressions. (This means a given covariate is residualized multiple times, separately for each spring impact regression model; this assures that the properties sought through residualization manifest exactly each time the covariate is used in an impact regression.) The fitted equation is then used to predict a value for each individual based on her/his treatment/control group assignment, and the individual's "residual" derived by subtracting the predicted value from the actual value. With such a simple specification for the initial regression, the model produces a predicted value equal to the treatment group mean of the variable for every individual in the treatment group, and a predicted value equal to the control group mean for every individual in the control group, resulting in a "residualized" version of the variable that is mean-deviated within each treatment/control cell.

[17] Some pre-test measures used as covariates have "artificial zeros" inserted into them for children with certain language backgrounds, as explained below. In these instances, only the observations with real data values are used in the "residualization" procedure, both to compute means and in adjustments that subtract the mean. This assures that the goals of residualization are achieved for the real data values, leaving the "artificial zero" values to be accommodated by other facets of the imputation procedure.

of the "residualized" variables are 0 for both treatment and control groups, this ensures that no part of the estimated spring impact is attenuated by their inclusion in the model.

The procedure has the drawback that the residualized fall measure will not remove *purely chance* differences between treatment and control groups on the fall measure, as would using the fall measure as a covariate directly.[18] We judge this sacrifice in statistical precision to be worth the assurance gained that the potential early *impacts* of Head Start will not interfere with unbiased impact estimation in subsequent springs. To insure the precision loss is not large, we re-ran some of the impact analyses using the original version of the fall outcome measure without residualizing it, recognizing that this potentially biases the impact estimate but focusing on the estimate's standard error. Our goal was to see if the standard error (and hence variance) of the impact estimate goes down appreciably when the fall measure retains the information needed to offset chance differences between the treatment and control group starting points on the pretest measure. These checks—which included re-running impacts on child assessment outcomes from the spring of 2003 (eleven impacts for each of two age cohorts)—showed only a trivial difference in the magnitude of the standard errors compared to the residualized case.[19] One value of including covariates in impact regressions is still realized: variation in spring outcomes associated with the pre-existing diversity of the families and children within the treatment group and within the control groups is still preserved in the "residualized" covariates and continues to reduce unexplained variability in the data, thereby improving the statistical precision of all the estimated regression coefficients, including the impact estimates.

For many child assessment outcomes, the residualization was done separately by language group within age cohort. Many of the fall tests were administered in English for both English-speaking and Spanish-speaking children at baseline. However, performance on these

[18] In addition to purely chance differences, differences due to differential nonresponse between the treatment and control groups in the collection of spring outcome measures will also not be removed by using covariates that have been "residualized" in this fashion. Fortunately, it is only the effects of differential nonresponse *not removed* by other means—i.e., by non-response adjustments to the analysis weights used in the impact regressions (described elsewhere in the report)—that remain as an influence on the spring impact estimates when "residualized" covariates are used instead of the non-residualized covariates.

[19] There was one exception among the 22 comparisons made: Woodcock-Johnson III Applied Problems, for which the residualized standard error of 2.11 was appreciably larger than the non-residualized standard error of 1.66. No other contrast was anywhere near this large.

tests depends heavily on the English-language skills of the assessed child. As a result, we expect the assessments to measure different aspects of language and literacy potential for predominantly English-speaking children and predominantly Spanish-speaking children, given the substantially different capabilities with the English language the two sets of children possessed at that time.

Exhibit 5.4 provides the particular fall 2002 outcome used with each spring outcome measure to create a "residualized" version of the fall 2002 outcome for use in the regression model. Note that children in the Other language group were not given the PPVT, Woodcock Johnson III Letter Word, or Woodcock Johnson III Applied Problems assessments in fall 2002, thus it was not possible to create a residualized fall measure for them. A value of zero was imputed for them, since zero is the mean of the residualized fall measures for the English and Spanish groups, so they could be included in the regression modeling that produced the adjusted impact estimates. Because the Other language group comprises only 1.5% of the sample and 5.7% of the combined Spanish/Other group, the effect of this imputation on the impact estimates should be minor.

Exhibit 5.4: Measures of Fall 2002 "Starting Points" Used in the Regression Models, by Child and Parent Outcomes

Outcome Measure	Fall 2002 Measure Used as a Covariate
Cognitive Domain	
Peabody Picture Vocabulary Test (PPVT) (Adapted)*	PPVT
Preschool Comprehensive Test of Phonological and Print Processing (CTOPPP) Elision Subtest *	PPVT
Letter Naming *	PPVT
Color Identification	Color Identification
Counting Bears	Counting Bears
McCarthy Scales of Children's Abilities Draw-a-Design Subtest	McCarthy Scales of Children's Abilities Draw-a-Design Subtest
Woodcock-Johnson III Letter-Word Identification *	Woodcock-Johnson III: Letter-Word Identification
Woodcock-Johnson III Spelling*	PPVT
Woodcock-Johnson III Applied Problems*	*Assessed Primarily in English in Fall 2002* Woodcock-Johnson III Applied Problems *Assessed Primarily in Spanish in Fall 2002* Woodcock-Munoz Applied Problems
Woodcock-Johnson III Oral Comprehension*	PPVT
Woodcock-Johnson III Pre-Academic Skills *	PPVT
Woodcock-Johnson III Writing Samples*	PPVT
Woodcock-Johnson III Passage Comprehension*	PPVT

Exhibit 5.4: Measures of Fall 2002 "Starting Points" Used in the Regression Models, by Child and Parent Outcomes (continued)

Outcome Measure	Fall 2002 Measure Used as a Covariate
Woodcock-Johnson III Calculation*	*Assessed Primarily in English in Fall 2002* Woodcock-Johnson III Applied Problems *Assessed Primarily in Spanish in Fall 2002* Woodcock-Munoz Applied Problems
Woodcock-Johnson III Academic Applications* Composite	PPVT
Woodcock-Johnson III Academic Skills *	PPVT
Woodcock-Johnson III Basic Reading Skills*	PPVT
Woodcock-Johnson III Math Reasoning*	*Assessed Primarily in English in Fall 2002* Woodcock-Johnson III: Applied Problems *Assessed Primarily in Spanish in Fall 2002* Woodcock-Munoz Applied Problems
Woodcock-Johnson III Word Attack*	PPVT
Woodcock-Johnson III Quantitative Concepts*	*Assessed Primarily in English in Fall 2002* Woodcock-Johnson III: Applied Problems *Assessed Primarily in Spanish in Fall 2002* Woodcock-Munoz Applied Problems
Test de Vocabulario en Imágenes Peabody (TVIP)	*Assessed Primarily in Spanish in Fall 2002* Test de Vocabulario en Imágenes Peabody (TVIP)
Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada: Identificación de letras y palabras	*Assessed Primarily in Spanish in Fall 2002* Batería Woodcock-Muñoz Pruebas de aprovechamiento-Revisada: Identificación de letras y palabras
Parent (reported) Emergent Literacy Scale (PELS)	Parent (reported) Emergent Literacy Scale (PELS)
Social-Emotional Domain	
Social Skills and Positive Approaches to Learning	Social Skills and Positive Approaches to Learning
Total Problem Behavior	Total Problem Behavior
Aggressive Behavior	Aggressive Behavior
Hyperactive Behavior	Hyperactive Behavior
Withdrawn Behavior	Withdrawn Behavior
Pianta Scale: Closeness	None
Pianta Scale: Conflict	None
Pianta Scale: Positive Relationship	None
Social Competencies Checklist	Social Competencies Checklist
Parenting Practices Domain	
Parent used time out in the last week	Parent used time out in the last week
Parent spanked child in the last week	Parent spanked child in the last week
Parental Safety Practices Scale	Parental Safety Practices Scale
Family Cultural Enrichment Scale	Family Cultural Enrichment Scale
Parenting Style is Authoritarian	Parenting Style is Authoritarian
Parenting Style is Authoritative	Parenting Style is Authoritative
Parenting Style is Neglectful	Parenting Style is Neglectful
Parenting Style is Permissive	Parenting Style is Permissive
Parent read to child in last	Parent read to child in last

Exhibit 5.4: Measures of Fall 2002 "Starting Points" Used in the Regression Models, by Child and Parent Outcomes (continued)

Outcome Measure	Fall 2002 Measure Used as a Covariate
Health Domain	
Child received dental care	Child received dental care
Child's overall health status is excellent/good	Child's overall health status is excellent/good
Child had care for injury in last month	Child had care for injury in last month
Child has health insurance coverage	Child has health insurance coverage
Child needs ongoing care	Child needs ongoing care

* Fall Measure was residualized separately by English and Spanish language groups.

Adjusting for Variation in the Timing of Outcome Measurement

Not all parent interviews and child assessments were conducted in the same week or even the same month in a particular spring's data collection. Consequently, the timing of the collection of outcome variables used in the impact analysis from these sources affects the way impacts should be estimated. That is, systematic differences between the treatment and control group samples in the timing of spring data collection could bias estimates of Head Start's impact derived from treatment-control comparisons, because children could be measured at a slightly different average ages in the two samples.[20] The importance of this factor depends on the pace of development—the cognitive and social-emotional growth of children as they age—and the degree of temporal mismatch in data collection. If data collection was completed earlier (or later) for the treatment group than the control group in any spring, some of the treatment/control difference in measured outcomes at that point could reflect different degrees of maturation rather than the desired effects of the Head Start program.

Initial data checks indicate that data for treatment group members tended to be collected somewhat earlier in the spring than for control group members, leading to a possible downward bias in estimated impacts when the artificially depressed developmental levels of the treatment group are compared to outcomes for a somewhat older control group. As discussed in the section on cross-sectional impact estimation methods, this issue was dealt with by adding a term to the impact regression equation that measures the "time of testing," i.e., the number of weeks elapsed from Sept. 1, 2002 to the day of spring data collection. If the time of testing varies

[20] Differences in the age distribution of the treatment and control samples can only arise through differential timing of spring data collection since, by virtue of random assignment, no systematic difference can occur in the distribution of dates of birth.

enough during the several months long spring data collection period to materially affect outcomes on developmental measures, the coefficient of this term will differ from 0.[21]

Imputation for Fall 2002 Item and Instrument Nonresponse

To produce as complete a data set for impact analysis as possible, it is desirable to impute missing responses for fall 2002 variables used as covariates or to form subgroups (for analysis of variation in impacts).[22] Imputation of baseline measures also helps to control for nonresponse bias caused by initial nonresponse and thus to produce a more representative file for all impact analyses. Discarding incomplete cases is inefficient, but more seriously, the complete cases may not be representative of the target population.

Hot-Deck Procedure. Fall 20002 variables missing due to item and instrument nonresponse were imputed using hot deck imputation. Hot deck imputation is a procedure where cases with missing values for specific variables have those "holes" in their records filled in with values from other similar "donor" cases. Because the imputed values are actual respondents' values, hot-deck imputation has the desirable property that imputed values are always feasible, since they come from the actual distribution of reported values for real children. The "donor," is randomly selected from a pool of similar children who are matched to the "recipient," on characteristics which are correlated with the variable being imputed. The aim is to construct pools or imputation classes that explain as much of the variance in the variable to be imputed as possible, but at the same time are of adequate size so that there is some minimum number of donors in each class so that donors are not reused too many times. The assumption is that within each imputation class, the mechanism that leads to missing data is ignorable; that is, the missing values are missing at random. This means that the probability that a value is missing can depend on the values of the imputation class variables but not on the missing values themselves. If implemented carefully, hot deck imputation can preserve the distribution of the data, so that

[21] The coefficient could also differ from 0 if time of testing is correlated with important student demographic characteristics. However, we believe that it is more likely that age will confound results than demographic characteristics that happen to be correlated with time of testing. The model does contain the child's age in weeks at the time of spring testing.

[22] Imputation was only done for missing fall 2002 covariates. Missing data from the consecutive spring follow-ups was handled through the use of non-response weights.

estimates of distributional characteristics such as percentiles, variability, and correlation will not be distorted.

The variables used to form imputation classes or cells were identified from chi-square tests of association and bivariate correlation coefficients. In some cases they were also determined by skip patterns in the parent questionnaire and other requirements of logical consistency between questionnaire items. The imputation cells were created as the Cartesian product of these variables. A donor was allowed to be used up to three times. When no more donors were available in an imputation class, adjacent cells were collapsed to expand the availability of potential donors. The order of collapsing was specified so that levels of the least correlated cell variable were collapsed first, followed by the second least correlated variable, etc. until a donor was found. Imputed values have been flagged so that an analyst has the option of not using the imputed data, such as when analyzing the effects of the imputed data on the results.

Variables That Were Imputed. Missing values for demographic variables, child health outcomes, social-emotional and scale variables were imputed for the entire sample; missing values for test score variables were imputed for all sample members of the language group that should have completed the particular assessment (see previous discussion of outcome measures). The variables that underwent imputation and their item nonresponse rates are given in Exhibit 5.5 for variables used in the analysis. The item nonresponse rate was calculated as the number of children for whom the item was missing (and hence imputed) divided by the total number of children eligible for the item. For any given spring data collection, item nonresponse rates will be lower than those in Exhibit 5.5 after conditioning on overall response status for the child assessment, since many of the item nonrespondents are also survey nonrespondents for the spring assessment. Among spring 2003 child assessment respondents, the nonresponse rates for demographic variables ranged from < 1% to 16%. Child health outcomes, scores and scale variables had missing rates ranging from 9% to 12%.

Use of Correlation and Missing Data Patterns. The multivariate relationships between items were taken into account in the imputation to maintain consistency of the data and attempt to preserve correlations among variables. Continuous correlated items such as assessment scores or social-emotional scales were usually imputed from a single donor child. The donor was randomly selected from within a donor pool of children matched by treatment/control group

Exhibit 5.5: Item Nonresponse Rates for Fall 2002 Imputed Variables Used in the Analysis

Variable Name	Imputed Count	Total Eligible	Percent Imputed
Depression Maximum Likelihood Ability Estimate	932	4667	19.97
Number Of Children Age 17 And Under In Household	652	4667	13.97
Family Cultural Enrichment Scale	924	4667	19.80
Parent Read to Child in Last Week	916	4667	19.63
Parental Safety Practices Scale	930	4667	19.93
Parent Spanked Child In Last Week	921	4667	19.73
Number of Times Parent Spanked Child	938	4667	20.10
Parent Used Time Out In Last Week	923	4667	19.78
Number of Times Parent Used Time Out	941	4667	20.16
Caregiver's Race/Ethnicity	31	178	17.42
Child Race/Ethnicity	45	4667	0.96
Child Gender	2	4667	0.04
Father's Race/Ethnicity	748	4667	16.03
Child Received Head Start Service	28	4667	0.60
Mother's Race/Ethnicity	677	4667	14.51
Caregiver’s Age As Of Sept 1, 2002	35	178	19.66
Child US Born	656	4667	14.06
Economic Difficulty	941	4667	20.16
Father's Marital Status	1067	4615	23.12
Biological Father Lives With Child	804	4667	17.23
Grandparent In Household	662	4667	14.18
Home Language	56	4667	1.20
Biological Mother Immigrant Status	681	4667	14.59
Biological Mother Recent Immigrant Status	275	1553	17.71
Biological Mother Lives With Child	661	4667	14.16
Biological Mother Total Number of Years In US	275	1553	17.71
Family Monthly Income Range	1079	4667	23.12
Age Of Mother As Of Sept 1, 2002	738	4667	15.81
Mother's Employment Status	867	4663	18.59
Biological Mother GED Status	701	4667	15.02
Biological Mother Highest Educational Achievement	701	4667	15.02
Mother's Marital Status	696	4663	14.93
Number Of Moves In The Last 12 Months	1020	4667	21.86

Exhibit 5.5: Item Nonresponse Rates for Fall 2002 Imputed Variables Used in the Analysis (continued)

Variable Name	Imputed Count	Total Eligible	Percent Imputed
Number Of Adults 18 And Over In Household	933	4667	19.99
Aggressive Behavior	918	4667	19.67
Child Received Dental Care	924	4667	19.80
Child's Overall Health Status Is Excellent/Good	921	4667	19.73
Child Had Care for Injury Last Month	929	4667	19.91
Child Has Health Insurance Coverage	924	4667	19.80
Child Needs Ongoing Care	669	4667	14.33
Hyperactive Behavior	919	4667	19.69
Fall Parent Reported Emergent Literacy Scale	914	4667	19.58
Social Competencies Check List	918	4667	19.67
Child Has Special Needs	663	4667	14.21
Social Skills And Positive Approaches To Learning Scale	918	4667	19.67
Total Child Problem Behavior	918	4667	19.67
Child Has Unmet Health Care Needs	928	4667	19.88
Withdrawn Behavior	918	4667	19.67
Respondent's Relationship To Child	663	4667	14.21
Teen Birth Status	726	4667	15.56
Number of Children Under Age 6 In Household	652	4667	13.97
Elision EAP Maximum Likelihood Ability	712	3234	22.02
PPVT EAP Maximum Likelihood Ability	1028	4375	23.50
PPVT Publisher Standardized Score	1028	4375	23.50
Spanish Elision EAP Maximum Likelihood Ability	301	1345	22.38
TVIP EAP Maximum Likelihood Ability	275	1345	20.45
TVIP Publisher Standardized Score	275	1345	20.45
Child Age In Months As Of 9/1/2002	3	4667	0.06
How Well Child Did In Counting Bears	1041	4646	22.41
Counting Bears Score	1230	4646	26.47
Color Identification Score: Total	953	4646	20.51
CTOPP Elision Total Score	712	3234	22.02
Spanish CTOPP Elision Total Score	301	1345	22.38
McCarthy Draw-a-Design Score: Total	961	4646	20.68
Woodcock Johnson III Applied Problems Standard Score	728	3234	22.51
Woodcock Johnson III Applied Problems W Score	728	3234	22.51

Exhibit 5.5: Item Nonresponse Rates for Fall 2002 Imputed Variables Used in the Analysis (continued)

Variable Name	Imputed Count	Total Eligible	Percent Imputed
Woodcock Johnson III Oral Comprehension Standard Score	745	3234	23.04
Woodcock Johnson III Oral Comprehension W Score	745	3234	23.04
Woodcock Johnson III Spelling Standard Score	690	3234	21.34
Woodcock Johnson III Spelling W Score	690	3234	21.34
Woodcock Johnson III Letter-Word Standard Score	990	4375	22.63
Woodcock Johnson III Letter-Word W Score	990	4375	22.63
Woodcock Munoz Applied Problems Standard Score	296	1345	22.01
Woodcock Munoz Applied Problems W-Score	296	1345	22.01
Woodcock Munoz Dictation Standard Score	288	1345	21.41
Woodcock Munoz Dictation/Dictation W-Score	288	1345	21.41
Woodcock Munoz Letter Word Standard Score	285	1345	21.19
Woodcock Munoz Letter Word W-Score	285	1345	21.19
Parenting Style: Authoritarian	942	4667	20.18
Parenting Style: Authoritative	962	4667	20.61
Parenting Style: Neglect	899	4667	19.26
Parenting Style: Permissive	935	4667	20.03

assignment, language spoken at home, sex, race/ethnicity, and age in months as of September 1, 2002. The test score and scale variables were imputed in groups according to similar patterns of missingness (i.e., the joint missing rates) and the degree of correlation among them. This was done so that in general only the missing test scores would be imputed on each record, and children with partially reported test scores would not have them overwritten by the donor's scores. However, for patterns of missingness represented by a small number of children, the donor's scores were allowed to overwrite the reported scores in the interests of reducing the number of computer runs. This strategy was viewed as a compromise between the desire to avoid throwing away reported scores and the goal of preserving the correlation among score variables. It should be noted that the percent of child records with partial reporting of score and scale variables is small. The social-emotional scales were either entirely missing or entirely reported for all but a trivial (<.1%) percentage of the sample. For the depression, loss of control, welfare, and crime and violence scales, 8.3 percent of the sample had partially missing data (5.6% were missing all but one scale, 2.5% were missing only one scale, and 0.2% were missing

some other combination). For the continuous test score variables, less than 5 percent of the sample had partial reporting of scores; most were either missing all scores or none.

The order in which items are imputed is also important in preserving the correlation structure in the data, because some imputed items can be used to form imputation cells in the subsequent imputation of related items. Attention to the ordering of imputation was important, for example, in the imputation of categorical assessment scores (e.g., so that the first score that was imputed could be used to create imputation cells for the next test score. It was also used throughout in the imputation of correlated demographic and household variables. Similarly, for items associated with a skip pattern in the parent questionnaire, the item that leads into the skip pattern was imputed first and the subsequent items were imputed depending on the value of the skip indicator. In addition, the demographic variables (i.e., were imputed first, and were then subsequently used to impute parenting practice, household income, child health outcomes, assessment scores and scale variables. Items with the least amount of nonresponse within a group of related categorical variables were imputed first, and then used in the imputation of items with larger amounts of missing data.

Role of Geography. In general, donors were randomly selected from within the same Head Start program within a cell when possible, collapsing with an adjacent program in the cell when necessary. Programs were sorted within a cell by primary sampling unit (PSU) within Census region, so adjacent programs tended to be from the same county or a nearby county. When there were a large number of imputation cells, the donor search often was broadened to the geographic PSU within a cell, and sometimes PSUs within a region were also collapsed. Some items such as fall scores required a closer match on demographic variables than geography or Head Start program in order to find a similar donor pool, and no attempt to stay within the PSU or program was made for these. For example, donors for fall scores were matched by home language, gender, race and age as of Sept 1, 2002 in months within the Head Start/non-Head Start groups. Geography was also ignored for certain items requiring a very close match to the donor on other questionnaire items for logical consistency.

Imputation Results. The distribution of each imputed variable was compared before and after imputation to check that the imputation procedures had not appreciably changed the distribution of the underlying variable. Correlation matrices were also examined to check that

bivariate correlations among scores and scales were not attenuated. Finally, crosstabs between categorical variables involved in skip patterns and those requiring logical consistency were checked to make sure that inconsistencies had not been introduced.

Sample Sizes, Target Populations, and Analysis Weights

The unit of analysis for all impact analyses is the child. This is true irrespective of the outcome measure or data source considered; even outcomes reported by parents and caregivers (the majority) are weighted and analyzed according to the children they describe. This makes all impact findings representative of all newly entering Head Start children in the nation in 2002.

Two separate samples were selected, one for children entering Head Start one year before their anticipated entry into kindergarten – referred to as the 4-year-old cohort – and one for children entering Head Start two years prior to their expected kindergarten entry – referred to as the 3-year-old cohort. All analyses are conducted separately for the two cohorts rather than as a pooled single analysis sample. This decision to conduct separate age-cohort analyses reflects the fact that children are at very different developmental stages at these two ages, and that the Head Start treatment differs markedly by age, not the least because of the smaller class sizes required for younger children and other programmatic factors. This division also corresponds to the structure of the original random assignment, which was done separately for the two age cohorts to run separate experiments that used two different definitions of the control group experience: no Head Start participation for the 4-year-old cohort, versus a one-year postponement of Head Start participation for the 3-year-old cohort (for which the control group was allowed to enter Head Start in the second year). Finally, the demographic composition of the group of children who enter Head Start at age three is very different from the group who enter at age four. Conducting the analyses separately allows the statistical models to make the appropriate adjustments for baseline differences in a way that best corresponds to these differences.

As described in Chapter 2, some children could not be tested in English at the time of the baseline assessments in fall 2002. These children were administered two Spanish language tests in both fall 2002 and spring 2003, i.e., the TVIP (adapted) and the Woodcock-Muñoz Letter-Word Identification Test. Separate impact estimates were developed for this group of children on these Spanish language assessments. Like all subgroups defined by characteristics independent of the intervention and not affected by random assignment, these language-based

subsamples are, but for chance,[23] well-matched in terms of children in the Head Start and control groups. Hence, they are fully suited to valid experimental examination in their own right. Thus, the separate language-of-assessment analyses provide equally unbiased measures of Head Start's impact for these subpopulations, as does the study as a whole for the full population.

All of the children in the study sample from Puerto Rico began with Spanish-language assessments and continued exclusively in that language throughout the study period (since transition to bilingualism through English acquisition does not commonly take place until a later grade). Because cognitive measures administered in different languages are not directly comparable, Puerto Rican children are analyzed separately from their "mainland" counterparts in Appendix F in the Final Report of the Head Start Impact Study.

The weighting strategy, described in more detail in Chapter 2, was chosen to maximize the data available at each analysis point by including every completed child assessment and parent interview from each wave of spring data collection. Observations were compiled separately for child assessments, parent interviews, and teacher reports and information included from one of these sources even when one or more of the other sources may have been missing. For this reason, and also due to item nonresponse for specific questions in completed questionnaires, sample sizes are not identical for all analyses, i.e., different outcome variables involve slightly differing numbers of observations. The comparability of the Head Start and control group samples established at random assignment is maintained to the greatest extent possible in each instance by adjusting the initial sampling weights to offset observable baseline differences between respondents and non-respondents.

Analysis weights were established separately for the child assessments and the parent interview each spring, for use in the annual cross-sectional analysis of Head Start impacts in each follow-up year. These weights are based on the probability of selection into the study sample, including all stages of sampling, and were adjusted each year to compensate for nonresponse by adjusting the weight for responding children with similar individual and family background characteristics on variables measured for all randomly assigned children in fall 2002. The weights also include an adjustment to the program and center weights for the exclusion of

[23] In addition to chance, the comparability of the treatment and control group samples for different language groups depends on the success of the nonresponse weight adjustments made to the overall sample to deal with possible differential nonresponse in spring data collection, discussed in Chapter 2.

grantees and centers in which the number of applicants did not exceed the number of funded federal Head Start slots. The weighted data, therefore, represent the same universe for all spring outcomes each analysis year; namely, the national population of 3- and 4-year-olds who entered Head Start for the first time in fall 2002.

A separate weight was also created for analysis of Teacher Survey/Teacher Child Rating outcomes from spring 2004 to spring 2006. The Teacher Survey and Teacher Child Rating were administered to teachers of sampled children in pre-K programs in centers (both Head Start and non Head Start programs), or who were in kindergarten or first grade. Separate weights were also created for analysis of the Director Interview data, and for analysis of classroom observations collected at children's out of home child care placements.

A separate analysis weight was also created for longitudinal analysis of children who had two or more spring data collection points between Fall 2002 and Spring 2006 (see Chapter 2). The longitudinal weight permits these children to represent the population of children who applied for their Head Start year in fall 2002. It was created by adjusting the child base weight for children, who did not have at least two data points, then poststratifying and trimming the weight in the same manner as the cross-sectional weights. The weight was used in the fitting of growth curves using multilevel modeling software by partitioning it into the center weight and within-center child weight, to correspond to the center and child levels in the three-level model (see section on repeated measures analysis). The level one child weight was then scaled using method two as described in Pfeffermann (1998), so that the scaled weights sum to the nominal sample size of children within center.

Profiles of the sampled Head Start and control group children with respect to demographic and family characteristics are provided in Chapter 2 separately by year and age cohort. Each exhibit compares the unweighted distribution of the sampled respondents at each spring data collection (spring 2003 to spring 2006) with the weighted distributions, using both the child base weight and the final child weight. The final weight includes a nonresponse adjustment for both age cohorts, and a poststratification adjustment to the race/ethnicity distribution of the Head Start National Reporting System (HSNRS) for the 4-year-old cohort in the sample (the HSNRS does not provide data for 3-year-olds), since the HSNRS is a census of 4-year-old Head Start enrollees. The poststratification adjustment has the effect of

downweighting the weights of Hispanic children and increasing the weights of Black children to more closely match the distribution of the HSNRS. The effect of the poststratification can be seen in the Child Race/Ethnicity exhibit when comparing the "Base Weights" and "Final Weights" columns. The exhibits also show that the composition of the sampled respondents has remained stable over time, whether unweighted or weighted.

Exhibit 5.6 shows the number of respondents for the Head Start and control groups by age cohort and year, separately for the child assessments, parent interview, and teacher survey/teacher child rating. Overlap among respondents for the three different data collection instruments is considerable for both age cohorts, i.e., sample sizes track closely between the three different data sources.[24]

Exhibit 5.6: Number of Respondents by Wave and Age Cohort

	Fall 2002		2003		2004		2005		2006	
Instrument	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4	Age 3	Age 4
Child Assessment										
Head Start	1310	1050	1357	1084	1322	1009	1251	1003	1281	NA
Non-Head Start	746	617	808	649	816	615	769	616	742	NA
Parent Interview										
Head Start	1387	1102	1336	1068	1320	1022	1295	1032	1274	NA
Non-Head Start	847	679	821	662	806	627	798	640	772	NA
Teacher Survey and Child Rating										
Head Start	NA	NA	NA	NA	NA	659	1032	779	1028	NA
Non-Head Start	NA	NA	NA	NA	NA	401	632	483	643	NA

NA indicates not applicable.

Annual Cross-Sectional Impact Estimation Methods–Main Impacts

The impact of Head Start is assessed using (1) simple treatment-control differences in average child and parent outcomes and (2) differences in average outcomes adjusted for the set of baseline covariates discussed above. Both methods are discussed below.

[24] There are only two ways to move closer to a single, totally uniform sample for each age cohort so that impacts on all outcomes would derive from exactly the same set of cases: impute missing outcomes (and entirely missing data collection instruments) for cases with available data for some but not all outcome measures in all years, or choose not to use data that are available by excluding from all analyses observations with less than universal data. We do neither of these: the latter would waste information while cutting sample sizes unnecessarily while the former would require assumptions too closely intertwined with the program impacts the study intends to measure.

Differences in Average Outcomes

The Head Start Impact Study, like other evaluations that use random assignment to allocate slots to program participants, provides a framework for attributing child outcomes to the effects of the program, rather than to other factors that may influence child development. Unlike pre-test/post-test analyses and other comparison group approaches, this framework makes accurate impact measurement possible without considering any individual child's starting point. If enough individuals are randomized to the Head Start and control groups, and if all randomized individuals are included in the follow-up analysis, important differences in later outcomes are more likely to result from the intervention rather than other factors. Actual measurement, and adjustment for possible chance differences in starting points, is not essential under this design (although it can be useful for certain reasons, as discussed below).

The simplicity of the basic treatment/control comparison of spring outcomes, without recourse to other data, provides a powerful motivation for evaluating program impacts in this way. The transparency of the methodology, and its lack of dependence on sometimes complex statistical methods, makes these "difference-in-means" results good candidates as initial measures of Head Start's impact. The most basic version of this analysis contrasts the average outcome level for the treatment group with the average outcome level for the control group using unweighted data. However, the unweighted estimates can be biased because they do not take into account the differential probabilities of selection of children into the sample. The child weights account for the sampling of PSUs, grantee/delegate agencies, centers, and children within centers so that the study sample can be used to represent the national Head Start population.

These weighted difference-in-means impact estimates are reported as the basic estimates in this report. Statistical tests determine which of the measured outcome differences between treatment and control group children can be considered real impacts rather than simply due to sampling error. For continuous outcome variables (e.g., PPVT III scale score), the tests are based on the linear regression model that replicates the difference-in-means calculation by expressing the spring outcome measure for child i as the sum of an intercept term and a shift in the intercept produced by a dummy variable for inclusion in the Head Start treatment group:

$$Y_i = \alpha + \beta T_i + \varepsilon_i;$$

where Y_i is the outcome measure and T_i is a 0-1 variable indicating whether the child was randomly assigned to the treatment group (T_i = 1) or the control group (T_i = 0). When derived using weighted least-squares regression, the estimated coefficients from this model, $\hat{\alpha}$ and $\hat{\beta}$ have the following equivalence to calculated measures from the difference-in-mean approach:

$$\hat{\alpha} = \bar{y}_c$$
$$\hat{\beta} = \bar{y}_t - \bar{y}_c$$

where $\bar{y}_t$ is the weighted mean of *Y* for the treatment (Head Start) sample and $\bar{y}_c$ is the weighted mean of *Y* for the control (non-Head Start) sample. By either formulation, $\hat{\beta}$ gives an unbiased estimate of the impact of access to Head Start, since no systematic differences should exist between the treatment and control samples (assuming complete follow-up data on Y), given children were randomly assigned to each group within Head Start centers. When divided by its standard error, $\hat{\beta}$ in large samples for continuous outcomes follows the Students t-distribution with 51 degrees of freedom under the null hypothesis that true impact, β is 0, where 51 is the total number of degrees of freedom associated with the jackknife estimate of the variance of $\hat{\beta}$. An unbiased standard error for $\hat{\beta}$, reflective of how the sample was drawn and weighted is obtained using replicates and weights described in Chapter 2.

Calculated p-values are also given for each estimated $\hat{\beta}$, assuming a two-tailed t-test of the null hypothesis of no Head Start impact (i.e., H_0: β =0).[25] This indicates the probability of obtaining an impact estimate of at least the magnitude observed when the true impact is 0, and allows readers to perform tests of statistical significance at different alpha levels (e.g., .10, .05 or .01) by comparing the p-value to alpha.

The 95-percent confidence interval for the true impact, $\bar{Y}_T - \bar{Y}_C$, is also reported:

$$(\bar{y}_t - \bar{y}_c) - t_{.975,df}SE(\bar{y}_t - \bar{y}_c) < \bar{Y}_t - \bar{Y}_c < (\bar{y}_t - \bar{y}_c) + t_{.975,df}SE(\bar{y}_t - \bar{y}_c),$$

The interpretation of the confidence interval is if all possible samples were drawn, then 95 of the confidence intervals constructed from these samples would contain the true impact. Note, it does not mean that the true impact has a 95 percent chance of being between the lower

[25] A two-tailed test allows for the possibility of program effects in either direction, up or down.

and upper limits for a given sample. Detailed tables providing confidence intervals will be provided on the Administration for Children and Families, Office of Planning Research and Evaluation website: http://www.acf.hhs.gov/program/opre/hs/impact_study index.html.

For binary outcome variables (e.g., use of dental care), logistic regression is used to do equivalent computations. Categorical outcome variables with more than two categories (e.g., Number of Times Read To) were collapsed into two categories. Here, the model specification is non-linear to accommodate the Bernoulli distribution of Y, which must always take on a value of 0 or 1, and to ensure that Pr(Y=1) is always between 0 and 1. The logistic model specifies the probability that Y equals 1 (conditional on T) for the i^{th} child as

$$P_i = \Pr(Y_i = 1) = \frac{exp(\alpha + \beta T_i)}{1 + exp(\alpha + \beta T_i)},$$

where T_i is the treatment group/control group indicator defined above. The coefficients α and β are estimated using the logit transformation to obtain a model which is linear in the parameters

$$ln\frac{P_i}{1 - P_i} = \alpha + \beta T_i$$

The predicted probability that Y=1 for the i^{th} child is

$$\hat{P}_i = \frac{\exp(\hat{\alpha} + \hat{\beta} T_i)}{1 + \exp(\hat{\alpha} + \hat{\beta} T_i)}.$$

The predicted marginal, $\hat{P}_t$, for the treatment group is obtained by taking the weighted average of the $\hat{P}_i$'s evaluated at $T_i = 1$ for every child in the sample, using the cross-sectional child weight. The predicted marginal represents the average predicted outcome if all children had been in the treatment group (Korn and Graubard, 1999). Similarly a predicted marginal for the control group, $\hat{P}_c$, is obtained by taking the weighted average of the $\hat{P}_i$'s evaluated at $T_i = 0$ for every child in the sample. The impact of Head Start is estimated as the difference $\hat{P}_t - \hat{P}_c$. Calculated p-values are also given, assuming a two-sided t-test of the null hypothesis of no Head Start impact (i.e., H_0: $P_t - P_c = 0$).

Differences in Adjusted Average Outcomes

To add the explanatory power of child and family background factors to the analysis, the regression models used to obtain difference-in-means estimates are expanded to express outcomes (or, in the case of logistic models, the probability of a particular outcome) as a function of assignment to the treatment group, and the set of covariates discussed in earlier in this Chapter .[26] Note that the addition of these covariates does not decrease the sample size, since missing values for the covariates and fall measures used in the model were imputed.

Letting T_i represent for child i the treatment indicator, $\boldsymbol{X}_i$ that child's vector of demographic covariates and time of testing variable, and R_i the "residualized" fall 2002 measure, the impact model becomes:

$$Y_i = \alpha + \beta T_i + \gamma' X_i + \delta R_i + e_i$$

for continuous outcome variables

$$P_i = \frac{exp(\alpha + \beta T_i + \gamma' X + \delta R_i)}{1 + exp(\alpha + \beta T_i + \gamma' X_i + \delta R_i)}$$

for dichotomous (0/1) outcome variables. The coefficients in the continuous outcome model are estimated using weighted least-squares regression, and in the binary outcome model using the logit transformation to obtain a model which is linear in the parameters:

$$ln\frac{P_i}{1-P_i} = \alpha + \beta T_i + \gamma' X_i + \delta R_i$$

For continuous outcomes, $\hat{\beta}$ is the estimate of Head Start's impact. Calculated p-values are also given for each estimated $\hat{\beta}$, assuming a two-tailed t-test of the null hypothesis of no Head Start impact (i.e., H_0: β=0).[27] This indicates the probability of obtaining an impact estimate of at least the magnitude observed when the true impact is 0, and allows readers to perform tests of statistical significance at different alpha levels (e.g., .10, 05 or .01) by comparing the p-value to alpha

[26] Outcomes derived from the teacher child rating form do not have a corresponding pretest measure so this term is dropped from these impact analyses.

[27] A two-tailed test allows for the possibility of program effects in either direction, up or down.

For binary outcomes, the impact estimate is obtained by first calculating the predicted probability that Y=1 for the i^{th} child,

$$\hat{P}_i = \frac{\exp(\hat{\alpha} + \hat{\beta} T_i + \hat{\boldsymbol{\gamma}}' \mathbf{X}_i + \hat{\delta} R_i)}{1 + \exp(\hat{\alpha} + \hat{\beta} T_i + \hat{\boldsymbol{\gamma}}' \mathbf{X}_i + \hat{\delta} R_i)}.$$

The predicted marginal, $\hat{P}_t$, for the treatment group is then obtained by taking the weighted average of the $\hat{P}_i$'s evaluated at $T_i = 1$ for every child in the sample, where the covariates and "residualized" fall score are set to the child's individual values and the child weight is used (Korn and Graubard, 1999). Similarly a predicted marginal, $\hat{P}_c$, for the control group is obtained by taking the weighted average of the $\hat{P}_i$'s evaluated at $T_i = 0$ for every child in the sample. The impact of Head Start is estimated as the difference $\hat{P}_t - \hat{P}_c$. Calculated p-values are also given—i.e., the probability of obtaining an observed impact estimate of at least the size seen when the true impact is 0—so that different significance levels (different alpha values; e.g., .10, .05 or .01) can be applied by the reader.

All estimation, model fitting and hypothesis testing was done with the SUDAAN (Research Triangle Institute, 2005) software package using the full-sample and jackknife replicate weights, to take into account the complex sample design (i.e., stratification, clustering) and weighting in the estimation of standard errors for the regression coefficients and the impact estimates.

When the "residualization" of the fall 2002 child assessment was done separately by assessment language group, the residualized fall measure R_i was entered into the model as a two-way interaction with the language group indicator, L_i (where $L_i = 1$ for children initially assessed primarily in English and $L_i = 0$ for children initially assessed primarily in Spanish or some other language). This allows the pre-test assessment score to play a distinct explanatory role in predicting spring outcomes for initially English-speaking children and initially non-English-speaking children. Specifically, three variables replace the variable R_i in the equations above, each with its own coefficient:[28]

[28] A more compact representation of the two-way interaction substitutes the expression R*L into the impact equations in place of R. All of variables listed here, each with its own coefficient, are subsumed in this notation.

- The language group indicator variable, L_i (= 1 if child i was initially assessed primarily in English in fall 2002, = 0 for all other children);
- The residualized pre-test measure (R_i) interacted with the language group indicator variable, creating $R_i L_i$; and
- The residualized pre-test measure interacted with the reverse of the language group indicator variable, $1-L_i$ (= 0 if child i was initially assessed primarily in English in fall 2002, = 1 for all other children), creating $R_i (1-L_i)$.

This specification in effect creates two distinct pre-test measures to use as independent predictors, R_iL_i for initially English-speaking children and $R_i (1-L_i)$ for children who initially speak little or no English. It assigns an artificial value of 0 to those variables for children not in the particular language group involved: a zero value for R_iL_i for children originally assessed predominantly in English, and a zero value for $R_i (1-L_i)$ for children originally assessed predominantly in Spanish and other languages. Were just these two pre-test variables added to the model with no further adjustments, the "artificial zeroes" they contain would distort estimates of the coefficients in the model, since they do not have the same meaning as "true" zeros. The addition of the language indicator variable L_i neutralizes this potential distortion and leaves all the other estimates in the model unchanged, including, crucially, the estimate of Head Start's

impact, β. At the same time, it accounts for more of the variation in outcomes within each of the language-defined subgroups.[29]

Standards of Evidence for Interpreting Multiple Impact Estimates

Standard statistical methods for determining if impacts of Head Start differ from zero, such as those described above with alpha = 0.10, limit the likelihood of a "false positive" result from any one test to 10 percent. That is, the chances of rejecting the null hypothesis of zero impact when in fact it is true is 1 in 10 with alpha = 0.10 as above, or 1 in 20 when alpha is set more conservatively to 0.05. However, the probability of incorrectly concluding that a non-reo impact has occurred goes up dramatically when many different outcomes are examined, as in the current study. For example, 10 hypothesis tests—each with a 0.05 probability of producing a false positive result if Head Start has not impact—have a 0.40 probability of generating at least one false positive if in fact the program has no impact on any outcome. In this instance, one must avoid generating a false positive with every test run—a 0.95 probability in every case when Head Start has no impact on any of the outcomes. This makes the probability of avoiding all

[29] To see how the addition of L_i protects against distortions of the regression coefficients and increases the explanatory power of the model, consider how the linear model used (linear in the log-odds ratio, for categorical outcomes) seeks to accommodate 0 values if L_i is left out. In that case, the relationship of the artificial zero values to the outcome variable Y_i would have to be reflected by the same estimated coefficient as the influence of other real values of these variables for children actually in the language group of interest, creating inaccuracies in how the model accounts for pre-test scores in both language groups. If those scores are correlated with other variables in the model—including T_i, the indicator of assignment to the treatment or control group—this opens the door to distortions in how the model represents those factors as well. It also seriously diminishes the amount of predictive information the model can extract from the pre-test measures—the very purpose for including pre-test measures in the regressions in the first place—by muting the contribution the real values can make to explaining outcomes as their mode of transmission is confounded by the artificial zeros. This threat is removed by adding the language indicator variable, L, which gives the model the ability to explain anything distinctive about the spring outcome levels of the artificial 0 cases through an intercept shift, away from the main regression line determined by real values of R_i, without disturbing anything else the model estimates. The coefficient on L_i supplies the shift amount; if the outcome variable Y_i for the English-speaking children is distinctive at all (as we would expect), its tendency to be above or below the point at which the model fit to the non-English-speaking children's data hits the vertical axis will be fully reflected in this coefficient. Note that with a constant term already in the model it is neither possible nor necessary to include a second indicator variable coded the reverse of the first (i.e., equal to 1 for the Spanish-speaking children and 0 for everyone else). The neutralizing of artificial 0s for this complementary set of individuals is accomplished by the same indicator variable, since—by defining simultaneously both the English-speaking and non-English-speaking children—its coefficient can reflect the net of the intercept shifts needed to neutralize artificial 0s for both of the pre-test variables. Also, the indicator variable L_i does not distinguish the small number of children whose language background was neither English nor Spanish at baseline from initially Spanish-speaking children; it is 0 for both groups. The cognitive assessments were not administered to non-English, non-Spanish children at baseline. To approximate what scores might have been recorded for those children had they been administered in English the mean value of R_i for Spanish-speaking children is inserted for each of these children.

false positives $(0.95)^{10} = 0.60$, resulting in a 0.40 probability of generating one or more false positives. The risk of one or more false positives is 0.65 when 10 tests are run using alpha = 0.10 (since $(.90)^{10} = 0.35$).

To limit the occurrence of false positives, the set of outcome measures used for a given age cohort in a given spring are grouped into five "families": two families comprised of cognitive outcomes taken from direct child assessments and teacher-school performance measures, plus three families consisting of all outcomes in the social-emotional, health, and parenting domains respectively. A procedure due to Benjamini and Hochberg (1995) is then used to limit the "false discovery rate" in each family of outcomes to no more than 10 percent. This procedure ensures that at most 10 percent of the impact estimates declared statistically significantly different from zero is a false positive—i.e., an instance in which Head Start in fact had no impact.

To implement this procedure, the original p-values for the individual impact estimates are ranked from 1 to m, where m is the total number of impacts estimated for the family. Each p-value is then compared to a calculated value equal to the value of its rank position in the ordering (e.g., rank position "m" for the largest p-value, rank position "m-1" for the next largest, and so on) multiplied by 0.05 and divided by m. A particular estimate is declared statistically significant in this multiple comparison test only if it is smaller than this calculated value.

In the subgroup analysis described below, five families of tests were created for each subgroup, using the same division of outcome measures described above, and the Benjamini-Hochberg procedure applied to each family and each subgroup. Additional families were created for estimated differences in impacts between subgroups; again, five families were created for each pairwise contrast between subgroups. Hence, for example, when race/ethnicity is used to form subgroups, a total of 30 families of tests are created:

	Cognitive Outcomes		Other Outcomes		
	Direct Child Assessments	School Teacher Performance	Social-Emotional	Health	Parenting
Impact on					
White children	√	√	√	√	√
Black children	√	√	√	√	√
Hispanic children	√	√	√	√	√
Difference in Impact Between					
White & Black	√	√	√	√	√
White & Hispanic	√	√	√	√	√
Black & Hispanic	√	√	√	√	√

For both main effects and subgroup impacts, the presentation of findings in the main report tables follows a consistent protocol—or standard of evidence—for which estimated impacts of Head Start are highlighted in exhibits and discussed in the text:

What we have in the chapter text is:

- **Strong Evidence of a Non -zero Impact*:*** the estimated impact for a particular outcome is statistically significant at the typical level ($p \leq 0.05$), and this result holds up under the test for multiple comparisons.
- **Moderate Evidence of a Non-zero Impact*:*** the estimated impact for a particular outcome is statistically significant at the typical level ($p \leq 0.05$), but this result *does not* hold up under the test for multiple comparisons.
- **Suggestive Evidence of a Non-zero Impact*:*** the estimated impact for a particular outcome is statistically significant under a relaxed standard ($p \leq 0.10$), and this result *may or may not* hold up under the test for multiple comparisons.

Adjusting for the Variation in the Timing of Outcome Measurement

As discussed in an earlier section of this Chapter, not all parent interviews and child assessments were conducted in the same week or even the same month in a particular spring's data collection. To deal with the possible introduction of bias in the annual cross-sectional analyses, a term is included in the vector of covariates, X_i, in the impact equation to measure the number of weeks elapsed from September 1, 2002 (the date for the calculation of every child's age) to the day of spring testing.

If this variable varies enough during a several months long spring data collection period to materially affect outcomes of 3- to 6-year-old children on age-related developmental measures, its coefficient will differ from 0. Further, if age at data collection has a different distribution for the treatment group than the control group, the variable will correlate with the treatment group dummy variable and the impact estimate $\hat{\beta}$ will shift somewhat. Any change in this coefficient will represent a neutralization of the developmental difference between treatment and control group children at the time the outcome was measured and thus constitute an improvement in the measurement of the effect Head Start participation *per se* had on treatment group members at that time.[30]

Calculating Effect Sizes

Impact estimates in their initial units are converted into effect sizes by dividing by the standard deviation of the outcome in the control group.[31] This provides a "yardstick" for gauging the quantitative importance of the estimated impact in relation to the natural variation of the child or family outcome Head Start is seeking to affect. Effect sizes tell us how much improvements produced by Head Start move children upward in the distribution of outcomes that would have prevailed had no Head Start intervention been available. The square root of the population variance of the outcome measure for the control group—the standard deviation of that measure—provides the best measure of that distribution and the conventional standard for this assessment.[32] For example, in a normal distribution a child whose outcome is at the 50th percentile of the control group distribution when s/he does not have access to Head Start moves

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[30] The coefficient could also differ from 0 because of selection patterns in the types of children whose data get collected earlier or later in the spring, since it will pick up the influence of *any* factors that correlate with the timing of spring data collection that are not otherwise included in the model. For example, spring data collection may have taken place later for more able children, leading their developmental measures at that time to exceed those of other children for *two* reasons: because they were older and because they have intrinsically greater development at any age. If there is no separate adjustment for ability when estimates are run, the coefficient on age will in this situation overstate how much aging as such affects development. We have, however, no evidence to suggest that this may have occurred.

[31] The standard deviation is calculated using the same weights on control group observations as the impact analysis itself.

[32] See, for example, *Technical Details of WWC-Conducted Computations*, September 12, 2006, pages 2-3 on "Effect size computation for continuous outcomes/ES as standardized mean difference", available from the U.S. Department of Education's What Works Clearinghouse website, http://www.whatworks.ed.gov/reviewprocess/conducted_computations.pdf.

up to the 58th percentile of the distribution if s/he experiences an effect size of 0.20; i.e., her/his impact equals one-fifth of a standard deviation of that distribution.

Two other metrics for computing effect sizes from the literature were considered but not used. The standard deviation of the combined treatment and control groups gives the same result if the two distributions are equally diffuse (i.e., if the Head Start intervention neither compresses nor expands the range of outcomes children experience but simply shifts it upward uniformly). If instead the distributions differ, the standard deviation from the combined sample makes interpretation less clear by producing an effect size that shows how much the intervention moves a child upward through some mixture of the distribution of untreated outcomes (the control group contribution to the standard deviation) and the distribution of treated outcomes (the treatment group contribution). Effect sizes derived from the standard deviation of an external reference population such as all 4-year-old children in the U.S., derived from a "norming sample," would indicate how much Head Start raises the outcomes of the children it serves through the distribution of all children. In our opinion, improvements relative to Head Start children's own untreated outcome levels say more about what the intervention accomplishes for the subjects it treats, though they say less about the extent to which that accomplishment brings Head Start children back into the American mainstream.

Estimating the Impact of Participating in Head Start

All of the impact estimates described to this point measure the effect of Head Start on the average child randomly assigned to the Head Start treatment group—that is, the impact of *granting access to Head Start services*. These estimates, based on comparisons of average outcomes between the entire treatment group and the entire control group are called "intent to treat" (ITT) impact estimates. They show the consequences of the government's intent to serve, or "treat," the first group compared to a statistically equivalent group for which there is no such intent.

However, not all of the children given access to Head Start in the study sites actually participated in federally funded Head Start services, the intended treatment. This is not an unexpected phenomenon: in the normal course of events, some children and families accepted into Head Start never participate, because their interest in what the program has to offer has

declined since application, because other center-based arrangements have been found, or because other events interrupt plans to attend (e.g., moving to another city or distant neighborhood).

The "Intention-to-Treat" and "Impact on the Treated" Research Questions

This suggests two different versions of the research questions that define the study:

- How much does Head Start help the typical child and family *admitted to* the program, on average?
- How much does Head Start help the families and children that *actually participate* in Head Start, on average?

Of course, it is more difficult to improve the average outcome of everyone accepted into Head Start than the average outcome of participants, since non-participants will presumably gain little or nothing from the program. If the non-participation rate (also known as the "no-show" rate) exceeds 5 or 10 percentage points, the difference in the two magnitudes may matter.

Answers to both questions matter for policy and program administration purposes. Head Start programs are typically funded for a fixed number of slots, regardless of whether all slots are used. In that sense, the Federal program pays for slots rather than actual participants where the two differ, so impacts per family or child admitted into those slots has some relevance to the fiscal picture.[33] Also, the Head Start program can offer opportunities to participate but it cannot compel any child to attend. Hence, the impact of admission into the program, whether taken or not, measures the typical result of what grantees do—provide access—rather than the effect of delivering services to every selected child and family. Yet the question of how much children gain from actually participating in Head Start's services remains an important one. For local programs at full attendance (not simply full enrollment, on paper) impacts per participant correspond with Federal funding per slot. When considering whether to expand or contract a fully occupied center, the value of the program slots that might be added depends on the gains provided to the children who actually occupy those slots—the participants. Moreover, if impacts per participant are large but impacts per admitted child comparatively small because of low participation, the evaluation will highlight the value of increasing participation rates as an adjunct or alternative to expanding the number of funded slots.

[33] This is particularly relevant where a slot is paid for and it goes unfilled when a child drops out of the program. However, for many Head Starts centers, slots do not necessarily stay unfilled as there are children waiting to enter the program.

In addition to no-shows, as in most social experiments some of the families of children randomized into the *control group* managed to get their children into Head Start even though they were not admitted directly. This subpopulation is known in the literature as "crossovers." The Head Start Impact Study had no way to fully ensure that the children and families randomly assigned to the control group did not participate in federally funded Head Start over the intended embargo period.[34] The grantees and delegate agencies whose applicants made up the research sample agreed not to serve those families using Federal Head Start funds during the 2002-03 program year but could not be totally monitored or compelled to abide by those agreements. Moreover, other grantees and delegate agencies in nearby communities (or, in the case of several large cities, in overlapping neighborhoods) did not enter into such agreements and, for reasons of privacy, could not be told the identities of the children and families involved in the study even had agreement been reached not to serve them.

In light of these limitations and the strong attraction of Head Start to many families, it is not surprising that a number of families from the control group in fact obtained Head Start services for their children during that year. A total of 17.6 percent of the children in the non-Head Start group are known to have had some participation in a federally funded Head Start program during the first year of the study, once analysis weights are applied. Further participation took place in the second year (see below for details). Though some of these enrollments may have been very brief, Head Start—if effective in general—likely had some impact on this subset of the control group.

The presence of no-shows and crossovers changes the meaning of the experimental comparison between the full treatment group and the full control group; it becomes the impact of *intent to treat*. At the same time, the impact of actual *receipt* of the Head Start intervention (compared to non-receipt) remains important to policy as discussed above. This leads to interest

[34] For the 4-year-old cohort, this period was the entire span of the children's potential Head Start participation, one year up to the point of kindergarten entry. Thus, the intention was that these children *never* participate in Head Start, and any participation constituted "crossing over" in violation of the random assignment intent. In contrast, the same embargo period of one year for the 3-year-olds constituted a different intent. The control group children in this age cohort were not supposed to represent outcomes in a world entirely without Head Start, but rather (as discussed below) a world where Head Start only becomes available at age 4. Thus, the only "crossing over" in violation of the random assignment design that matters for this population and causes an analysis problem is Head Start participation in the first year, the 2002-2003 school year. Future references to Head Start participation by members of the control group mean participation in the first year, when the experimental design said they were not to participate. Entry into the program in the subsequent year is not "crossing over" for the 3-year-old cohort.

in estimates of the "impact on the treated" (IOT), which show how Head Start affects the outcomes of a set of children who universally participate in Head Start compared to what would have happened to those same children had none of them participated. The challenge of creating reliable IOT measures from experimental data in the presence of no-shows and "crossovers" has been recognized in the evaluation literature for some time. For the current study, three approaches have been considered as ways to provide the government with ancillary information on IOT impacts, as a complement to the main ITT findings:

- Remove from the analysis sample those control group members who participated in Head Start in the first year, treating them like survey "non-respondents," and estimating impacts using a non-response adjustment that tries to offset their absence.[35] Then assume that Head Start had no impact on no-shows and rescale the findings to depict how it affected the remaining portion of the treatment group—i.e., participants.
- Use random assignment—0 for control, 1 for treatment—as an "instrumental variable" (IV) for Head Start participation to compute the impact of participation compared to strict nonparticipation for the subset of the experimental sample that switches between these two statuses. This approach again assumes no impacts on no-shows and in addition posits that impacts on crossovers equal those on "crossover-like" individuals in the treatment group.
- Compute lower and upper bounds on the IOT impact of Head Start by making high- and low-end assumptions about the outcomes that would have been observed for crossover children had they *not* participated in the program in the first year. As before, deal with no-shows by assuming they experienced no impacts and rescale the findings to reflect impacts on just participants.

Based on several considerations we concluded that the best way to provide information on Head Start's IOT impact is through the use of the instrumental variables (IV) approach. An explanation of this methodology is provided next, first as concerns the problem of dealing with no-shows in the treatment group and then as concerns crossovers in the control group. The justification for using the IV approach, rather than the other methods considered, follows.

Methodology for Dealing with No-Shows

Before choosing between the different ways of handling crossovers, we address one crucial group of children that must be examined no matter which strategy is adopted: no-shows,

[35] This methodology was used, largely for illustrative purposes, in the First Year Report, with the expressed intent of returning to the issue of how to construct a more robust IOT estimate for the Final Report.

the set of children in the treatment group who did not participate in federally funded Head Start in the first year of the study. Recall the intent of the study was to vary experimentally the first year of Head Start participation, which for the 4-year-old cohort was the only year during which Head Start participation might take place prior to kindergarten. But for the 3-year-old cohort, the one-year exclusion left control group children free to enter Head Start the second year if their families remained interested. The long-term goal of this part of the study—the 3-year-old cohort—was to determine whether having Head Start available at age 3 is helpful to children brought to the program at that age, or whether they would be just as well off, initially and over the longer term, if the program were not there for them until age 4. Hence, it is only failure to participate in the first year of the study, school year 2002-2003, that complicates the IOT analysis, which seeks to measure the impact of the intervention on the treatment group members who received the treatment in the first year. When the treatment group sample contains no-shows defined in this way, the initial ITT estimate does not provide this information.

One obvious way to narrow the analysis is to confine attention to just those treatment group members who participated in Head Start, eliminating no-shows from consideration. Unfortunately, this drastically undercuts the value of the control group as a randomly selected match to the treatment group that shows what their outcomes would have been absent the intervention. The set of children in the control group who correspond to the treatment group members who participate in Head Start cannot be identified in an equivalent manner—there is no information to identify which of the control group children would have participated in the program in the first year had they been granted access. Attempts to model the determinants of participation in the treatment group and mimic that selection process for the control group are bound to be incomplete and suffer the same drawbacks that affect quasi-experimental estimates from non-randomized studies: selection bias caused by uncorrected differences between participants and comparison group members that are mistaken for program impacts.

Fortunately, the best way to estimate Head Start's impact on the average first year participant does not require that one know anything about what distinguishes them from no-shows. If one can simply assume that no-shows experience zero impact from Head Start in that first year, it is possible to avoid the selection issue entirely. No-shows can be entirely different from participants in measured and unmeasured ways, but it is unnecessary to understand how they differ or to make any adjustments for their distinctive characteristics.

This seemingly magical result is achieved by reinterpreting the overall difference in mean outcomes between the entire treatment group and the entire control group that constituted the initial estimate. This estimate contains two distinct elements:

- Head Start's average impact on participants.
- Head Start's average impact on no-shows, a group that by definition did not participate in the program in the first year and that can logically be assumed to be unaffected by the program for that year (i.e., have an average impact of zero).

This assumption alone—that children and families who do not participate in Head Start or receive Head Start services remain unaffected by the program and the fact that they were assigned at random to participate in it[36]—makes it possible to interpret the entire measured effect of the program as an impact on just participants. It does not matter what the average effect on non-participants from first year participation *would have been* had they participated. Nor does it matter whether non-participants are destined to have different outcomes than participants due to pre-existing differences independently from the program.

To see this, start with the *aggregate* impact of the program on the treatment group as a whole, summed across all treatment group members. We do not actually calculate this total, but the ITT impact estimate comparing *average* outcomes between the treatment and control groups represents this aggregate impact sum divided by the number of children in the treatment group:

> *ITT impact = Average impact of access to Head Start = (Aggregate impact on all children in treatment group) / N,*

where N is the number of children in the treatment group. If instead we divide the aggregate impact by the number of children in the treatment group who actually participate in Head Start the first year of the study, P, we allocate the same total gain to just the set of participants:

> *Average impact on participants = (Aggregate impact on all children in treatment group) / P.*

This is where the assumption of zero impact on the nonparticipants—the no-shows—comes in. It allows us to infer that every bit of Head Start's total impact occurs for the P children in the participant subpopulation. This second expression is just *N/P* times the first expression. Thus, multiplying by *N/P*–or, equivalently, dividing by *P/N*, the treatment group participation rate—

[36] A comprehensive justification of this assumption in the context of the Head Start Impact Study is provided in Appendix 4.6 of the *Head Start Impact Study: First Year Findings* (June 2005).

converts the original ITT estimate of average impact of access to Head Start into an estimate of the average impact of participating.

It follows that if the ITT experimental comparison of average outcomes between all treatment group members and all control group members is not biased by systematic differences between these two randomly generated groups at baseline, this rescaling cannot be biased. This theorem, based solely on the assumption of zero impact on non-participants was first introduced into the literature by Bloom (1984) and provides a broadly accepted basis for the now almost universal practice of reporting impact estimates for participants-only alongside impact for the entire intervention-group.[37]

The Challenge of Dealing with Crossovers

Were no-shows the only departure of actual participation from the "intent to treat" of the experiment, the rescaling adjustment just described would provide an appropriate estimate of the "impact on the treated," or IOT impact. However, when some members of the control group cross over to receive federal Head Start services in the initial year of the study, an expanded approach is needed. To describe the problem this poses analytically and explain the instrumental variables (IV) approach to addressing this problem we must first broaden our mathematical notation, decomposing the ITT impact estimate into its pieces.

The ITT impact estimate (absent covariates[38]) contrasts the average outcome of the entire treatment group with that of the entire control group:

$$(1) \qquad I^{ITT} = \overline{Y}_t - \overline{Y}_c ,$$

$$I^{ITT} = \overline{Y}_t - \overline{Y}_c$$

where:

$\overline{Y}_t$ = average outcome for the entire treatment group;

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[37] The National Early Head Start Evaluation, for example, reports primarily "no-show-adjusted" estimates of impact on participants rather than highlighting more prominently the more directly obtained impact findings for the average intervention group member.

[38] The points in this section also hold when impacts are estimated using least-squares or logistic regression that includes covariates such as demographic characteristics and time of testing in the specification of the outcome measure.

$\bar{Y}_c$ = average outcome for the entire control group.

$\bar{Y}_t$ can itself be expressed as the weighted average of two pieces: (1) the average outcome for children in the treatment group who participated in Head Start; and (2) the average outcome for children in the treatment group who went through random assignment but then were "no-shows" and did not participate in Head Start. Because of random assignment we know that these same two types of children exist in the control group in the same proportions, even though we cannot explicitly identify which children they are. This allows us to restate the ITT estimate in Equation 1 in a way that separates out Head Start's impact on each of the two subpopulations. Expressed as a weighted average of the subgroup impacts, Equation 1 becomes:

$$(2) \qquad I^{ITT} = \bar{Y}_t - \bar{Y}_c = S_p [\bar{Y}_{tp} - \bar{Y}_{cp}] + S_n [\bar{Y}_{tn} - \bar{Y}_{cn}]$$

where:

S_p = share of treatment group members who participated in Head Start in the first year;

$\bar{Y}_{tp}$ = average outcome for treatment group members who participated in Head Start the first year;

$\bar{Y}_{cp}$ = average outcome for control group members who would have participated in Head Start the first year had they been put in the treatment group;

S_n = share of treatment group members who did not participate in Head Start the first year (i.e., no-shows; note that $S_n = 1 - S_p$);

$\bar{Y}_{tn}$ = average outcome for treatment group members who did not participate in Head Start the first year; and

$\bar{Y}_{cn}$ = average outcome for control group members who would not have participated in Head Start the first year had they been put in the treatment group (i.e., the "no-show-like" children).

The first term in brackets on the right-hand side of this equation, $\bar{Y}_{tp} - \bar{Y}_{cp}$, concerns the subpopulation for which the IOT analysis seeks to estimate impact: children who participate in Head Start when given access. This subpopulation is easy to identify in the treatment group, in which participation given access is directly observed. Unfortunately, the data cannot identify the same population in the control group, since those children are not given access. Still, we know that both subsets *exist* in the control group, and that they are statistically identical to the children

in corresponding portions of the treatment group except for the effects of Head Start participation.

Equation 2 can be further decomposed to make visible the children who were assigned to the control group but who managed to enter federal Head Start in the first year nonetheless—the crossovers. Due to random assignment, we know there must be children in the treatment group who correspond to these children and who would have done the same thing (crossed over) had they been assigned to the control group instead. We assume that all such individuals in fact did participate in Head Start the first year when made members of the treatment group,[39] making it possible to further subdivide Head Start's impact on participants, Y_{tp}-Y_{cp}, into two smaller pieces—one corresponding to those who would have "crossed over" to participate in Head Start had they been assigned to the control group and one corresponding to those who would not have crossed over:

$$(3) \quad \bar{Y}_{tp} - \bar{Y}_{cp} = S_{pr}(\bar{Y}_{tr} - \bar{Y}_{cr}) + S_{pk}(\bar{Y}_{tk} - \bar{Y}_{ck})$$

where:

S_{pr} = share of treatment group participants who would have crossed over [subscript "r" for crossover] if put in the control group;

$\bar{Y}_{tr}$ = average outcome for treatment group participants who would have crossed over into Head Start if put in the control group (i.e., "crossover-like" children);

$\bar{Y}_{cr}$ = average outcome for control group members who crossed over into Head Start;

S_{pk} = share of treatment group participants who would have kept out of Head Start [subscript "k" for kept out] the first year if put in the control group (note that $S_{pk} = 1 - S_{pr}$);

$\bar{Y}_{tk}$ = average outcome for treatment group participants who would have kept out of Head Start the first year if put in the control group; and

$\bar{Y}_{ck}$ = average outcome for control group members who kept out of Head Start the first year but who would have participated if in the treatment group.

[39] The assumption that the participant portion of the treatment group includes everyone who would have crossed over into Head Start if assigned to the control group is broadly viewed as almost certainly correct. For it to be wrong, children must exist whose caregivers behave in a very unusual manner, having the child participate in Head Start the first year when random assignment says s/he should *not* participate (i.e., when assigned to the control group) but keeping the child from participating when random assignment says s/he *should* participate (i.e., when assigned to the treatment group).

Substituting Equation 3 into Equation 2, we get:

$$(4) \qquad I^{ITT} = \bar{Y}_t - \bar{Y}_c = S_p[\, S_{pr}(\bar{Y}_{tr} - \bar{Y}_{cr}) + S_{pk}(\bar{Y}_{tk} - \bar{Y}_{ck})\,] + S_n[\,\bar{Y}_{tn} - \bar{Y}_{cn}].$$

The $\bar{Y}_{tk} - \bar{Y}_{ck}$ piece of the right-hand side of this equation compares universally treated children to universally non-treated children, i.e., it is an IOT estimate. This piece of the overall ITT estimate, often referred to as the "average complier effect,"[40] describes Head Start's impact on children who (a) participate in Head Start when put in the treatment group and (b) do not participate in Head Start when put in the control group, a group called "compliers" with the randomized design. The $\bar{Y}_{tk} - \bar{Y}_{ck}$ term in Equation 4 is the one IOT impact estimate that follows directly from random assignment—i.e., it compares a set of children who universally participated in Head Start ($\bar{Y}_{tk}$) with an equivalent set of children who universally did not participate ($\bar{Y}_{ck}$). But it is a portion of the ITT estimate that cannot ordinarily be identified in the data, since the $\bar{Y}_{tk}$ subset of the treatment group cannot be distinguished from the $\bar{Y}_{tr}$ subset (since both participate in Head Start the first year), and the $\bar{Y}_{ck}$ subset of the control group cannot be distinguished from the $\bar{Y}_{cn}$ subset (since neither participates in Head Start the first year).

The Instrumental Variable Approach to Addressing Crossovers

While it cannot be found directly in the data, the logical equivalent to $\bar{Y}_{tk} - \bar{Y}_{ck}$ can be inferred under certain circumstances. The conventional way of doing this is to apply an "instrumental variable" (IV) methodology that focuses on extracting impact information from the portion of the sample for which participation in Head Start varies depending on randomization status. Angrist, Imbens, and Rubin (2004) have shown in the statistical/econometric literature that an IV methodology produces an unbiased estimate of the causal effect of the intervention on compliers. Subsequently, evaluators have noted the equivalence of this method to a more intuitive approach in which the ITT estimate is rescaled to reflect what one can infer is the

[40] See, for example, Barnard, J.; Frangakis, C.; Hill, J.; & Rubin, D. (2003). Principal stratification approach to broken randomization experiments: A case study of school choice vouchers in New York City. *Journal of the American Statistical Association*, 98, 299-323.

intervention's average impact on compliers through an exercise that conceptually removes the non-compliers.[41] We will take this more transparent approach here.

Looking again at Equation 4, suppose it were true that

(5) $\bar{Y}_{tr} - \bar{Y}_{cr} = 0$,

meaning that outcomes are the same for crossovers and their non-identifiable equivalents in the treatment group. Suppose also that

(6) $\bar{Y}_{tn} - \bar{Y}_{cn} = 0$,

meaning that outcomes are the same for no-shows and their non-identifiable equivalents in the control group. Within the context of random assignment to treatment, these two equations can be true only if Head Start has the same impact on crossovers as it does on their equivalents in the treatment group, and the same impact—presumably, no impact at all—on no-shows and their equivalents in the control group, since outcomes cannot differ systematically between matched segments of the treatment and control groups except due to the impact of the intervention. Substituting Equations 5 and 6 into Equation 4 yields the following:

(7) $I^{ITT}\ \bar{Y}_t - \bar{Y}_c = S_p [\ S_{pk} (\bar{Y}_{tk} - \bar{Y}_{ck})\ ]$.

Here we see that under the conditions in Equations 5 and 6, the "intention to treat" impact estimate equals the difference in mean outcomes between compliers in the treatment group and compliers in the control group, factored downward to reflect that compliers are only a share (S_{pk}) of the total number of children who participate in Head Start the first year if assigned to the treatment group and that the full set of the children who participate the first year if assigned to the treatment group are only a share (S_p) of all children assigned to the treatment group. Using the relationships between subgroup shares given in the definitions of the different *S* terms above, Equation 7 can be rewritten as

(8) $I^{ITT} = (1 - S_n)\ (1 - S_{pr})\ (\bar{Y}_{tk} - \bar{Y}_{ck})$

[41] See for example Gennetian, L.A., Morris, P.A., Bos, J.M., and Bloom, H.S. Constructing instrumental variables from experimental data to explore how treatments produce effects, p. 86, in *Learning More from Social Experiments (2005)*, edited by H.S. Bloom. New York: Russell Sage Foundation.

This expression makes clear that the ITT estimate is "watered down" by inclusion of children who pass through random assignment but who, based on Equations 5 and 6, experience no net impacts from the intervention. In particular, the crossovers in Equation 5 diminish the average ITT impact by a factor $1–S_{pr} < 1$ determined by their prevalence in the control group, S_{pr}, while the no-shows in Equation 6 further diminish the already diminished impact by a factor $1–S_n < 1$ determined by their prevalence in the treatment group, S_n.

Returning to Equation 7, replace S_{pk} with $1 - S_{pr}$ based on the definitions given at Equation 3 above:

(9) $$I^{ITT} = S_p [(1 - S_{pr}) (\overline{Y}_{tk} - \overline{Y}_{ck})].$$

A further substitution can be made based on the definition of S_{pr}. If S_r is the share of control group that participated in Head Start in the first year (i.e., the share that are crossovers), S_{pr} can be rewritten as:

(10) $$S_{pr} = S_r / S_p.$$

It follows that

(11) $$1 – S_{pr} = (S_p – S_r) / S_p.$$

Making this substitution in Equation 9,

(12) $$I^{ITT} = S_p [(S_p – S_r) / S_p] (\overline{Y}_{tk} - \overline{Y}_{ck}) = (S_p – S_r) (\overline{Y}_{tk} - \overline{Y}_{ck}).$$

Dividing Equation 12 by $S_p – S_r$ and reversing the order of the terms gives an IOT estimate of $\overline{Y}_{tk} - \overline{Y}_{ck}$:

(13) $$\overline{Y}_{tk} - \overline{Y}_{ck} = I^{ITT} / (S_p – S_r).$$

This simply rescales the original ITT estimate by dividing by the difference in Head Start participation rates between the treatment and control groups, i.e., by the share of the total population that are "compliers." This is computationally equivalent to the Angrist-Imbens-Rubin IV estimator and, under the assumptions in Equations 5 and 6 (which parallel the Angríst-Imbens-Rubin assumptions), unbiased as a measure of the impact of participation on the "compliers" population.

For reasons discussed below, we use this methodology to estimate the impacts of Head Start on participants presented in the Final Report. As noted there, any statistically significant estimate of impact of access to Head Start is statistically significant for the impact of participation, and any non-significant impact is non-significant. This follows for several reasons, which collectively imply that the same p-values apply to the IOT estimates of participation effects as the ITT estimates of access effects. (Recall that p-values indicate the probability of obtaining an estimate of impact equal to or greater than the observed impact estimate when the null hypothesis of no impact is true.) Tables of results in Chapters 4 through 7 (for ITT estimates) and Appendix E (for IOT estimates) of the Final Report reflect this. Hence, they provide the same evidence for accepting or rejecting the respective null hypotheses of the two different analyses:

H_0^{ITT}: Average impact of access to Head Start = 0

H_0^{IOT}: Average impact of participating in Head Start = 0.

The basis for equating hypothesis test results between these two analyses starts with the logic of the null hypotheses themselves and the assumptions made in obtaining IOT impact estimates through IV methods. Specifically, equations 5 and 6, on which the IV findings rest, say that no part of the outcome difference between the entire treatment group and the entire control group—i.e., no part of the impact of access to Head Start—occurs among the "always takers" in equation 5 and no part of it occurs among the "never takers" in equation 6. Any impact of access that does occur must occur among "compliers", the remaining members of the study population who participate in Head Start if assigned to the treatment group and who do not participate if assigned to the control group. Thus, within the framework needed to do IV analysis, H_0^{ITT} is true according to whether or not compliers are affected by Head Start participation. However, within the same framework H_0^{IOT} is also true according to whether or not compliers are affected by Head Start participation. Why? Because impacts on all participants have to be assumed the same as impacts on compliers to use the IV analysis framework (see discussion of equation 14 below). Hence, the two null hypotheses are logically equivalent. It follows that whatever the data can tell us about the veracity of one in a hypothesis testing and p-value mode must be what it can tell us about the other.

At a more technical statistical level, the estimates used to test H_0^{ITT} and to H_0^{IOT}, given in equations 1 and equation 13 differ only through division of the latter by $S_p - S_r$. If this quantity is taken as a constant, with no sampling variability of its own, the standard errors of the two estimates also differ in exactly the same way, meaning that the t-statistics for testing the two null hypotheses—the ratio of the estimate to its standard error—are identical (as are the p-values). If instead one looks at $S_p - S_r$ as a random variable in its own right, adopting the position that p-values and hypothesis test conclusions are the same for the ITT and IOT analyses amounts to asserting that the sampling error in the observed compliance rate, $S_p - S_r$, is not of an important magnitude. Heckman et al. (1998) have found this to be the case when large samples are involved. Moreover, one can show that even with $S_p - S_r$, considered a random variable, the standard error of the IOT impact estimate $I^{ITT} / (S_p - S_r)$ in equation 13 asymptotically approaches the standard error of the I^{ITT} impact estimate in equation 1 divided by the observed $S_p - S_r$ value in the sample as sample size goes infinite. This again makes t-statistics, p-values, and hypothesis test inferences the same between the two types of analyses.

Other Methodologies and Their Limitations

As noted earlier, two other methodologies were considered for dealing with crossovers, beyond the IV approach:

- Remove crossovers from the analysis sample, treating them like survey "non-respondents," and estimating impacts using a non-response adjustment that tries to offset their absence.
- Compute lower and upper bounds on the IOT impact by making high- and low-end assumptions about the outcomes of crossovers would have been had they *not* participated in Head Start the first year.

Treating the crossovers as non-respondents would raise the overall "non-response" rate for the control group from 20 percent to 34 percent in spring 2003 for 3-year-olds and from 23 percent to 37 percent in spring 2003 for 4-year-olds, and to a similar extent in later years (i.e., Spring 2004, 2005, and 2006). This would make the control group non-response rate in each round of impact analysis much higher than the treatment group non-response rate that year, which is 11 percent in spring 2003 for 3-year-olds and 13 percent in spring 2003 for 4-year-olds, and similar in later years. With a high nonresponse rate, the responding subset of the control group is no longer a valid comparison for the treatment group. Nonresponse weights cannot be

relied upon to adequately compensate for the bias in the responding subset, leading us to reject this methodology. From the beginning, the National Head Start Impact Study was to be conducted as a randomized control trial, in order to guarantee that the sample used to represent Head Start participants absent the program be statistically equivalent to the group used to represent Head Start participants with the program. This has been achieved for the entire control group and the entire treatment group and should not be sacrificed now by "retro-fitting" the latter. Pre-existing differences between the children removed and those retained—and hence between the retained control group members and the full treatment group to which former would be compared—on factors not equilibrated through the non-response weighting adjustments (such as parental commitment to their children's pre-kindergarten intellectual development) would almost certainly lead to biased IOT impact estimates. Or at least raise questions about the reliability of the IOT estimates and whether they could be considered "experimental" in nature in any sense (and hence free of selection bias following non-response adjustment).

To do the bounding approach, assumptions have to be made about how high or low crossover outcomes could have been had those children not participated in Head Start. While it is possible to identify plausible "best case" and "worse case" extremes for these unmeasured values, this approach has several limitations:

- The IOT "lower bound" and "upper bound" impact estimates produced are complex to explain and difficult to compute accurately;
- The upper bound portion of the strategy can only be applied to cognitive outcomes, limiting the IOT analysis to just that domain and omitting information on the impact of participating for social-emotional, health, and parenting outcomes;[42]
- Reporting that Head Start's impact on participants is some unknown point in *a* range of numbers (i.e., in the interval between the lower and upper bounds) may be unsatisfying to policy makers.

Given these weaknesses, the greatest strength of the bounding approach—that the approximate nature of its findings conveys the reality that the randomized experiment cannot provide totally conclusive measures of the impact of participation—did not justify its use in our judgment.

[42] This is because the upper bound methodology relies on the assumption that the outcome involved has a natural upward trajectory, so that one can assume that outcomes of crossovers in Spring 2003—had those children not participated in Head Start—would have been at least as good as the last observed outcomes for those children without Head Start in Fall 2002. For cognitive outcomes, expected to grow with time on average, this seems a justifiable assumption. But not for outcomes in the other three domains, where an upward developmental trajectory over time cannot be assumed as the norm.

Validity of and Sensitivity to the IV Assumptions

We turn now to the question of whether the assumptions needed to construct the IOT estimate in Equation 13 are likely to be met. The earlier discussion of no-shows addressed the assumption in Equation 6 that outcomes are the same for no-shows and their control group counterparts, neither of which participated (for even a day) in Head Start in the year following random assignment. Based on the initial equivalence of these two sets of children and their universal lack of exposure to Head Start services, this is considered a very sound assumption.

Not so for the assumption in Equation 5, that outcomes of crossovers, $\overline{Y}_{cr}$, and outcomes of their counterparts in the treatment group, $\overline{Y}_{tr}$, are the same. One can only count on this assumption being met if crossovers receive the same Head Start intervention they would have received had they been randomized into the treatment group. Given the potential that crossover children entered the program through a more indirect or surreptitious route, and possibly with a time lag, it is essential to scrutinize this assumption carefully. One question is whether crossover children were served by different Head Start grantees than the one to which their families initially applied, since at that center they were supposed to be excluded from the program following the random assignment lottery. If so, the assumption that these children benefited from Head Start to the same extent as the corresponding children in the treatment group (who almost all participated at their centers of random assignment, if they participated in Head Start at all during the first year) becomes more fragile. For 63 percent of the crossovers in the 3-year-old sample and 59 percent of the crossovers in the 4-year-old sample the center of Head Start participation matched the center of random assignment, greatly alleviating this concern.

Another concern pertains to the timing of Head Start participation following randomization. Since, by the evidence in the previous paragraph, most crossover children participated at a center that had agreed to exclude them, it is possible that they were served later than they would have been if put in the treatment group—i.e., that the decision to break the rules of the study design and enroll them may have taken some time to take place, leading crossovers to make a delayed entry into the program compared to their treatment group counterparts The data again provide some reassurance on this point: as shown in Exhibit 5.7, the distribution of start dates for crossovers is virtually the same as for other participants (i.e., for children who participated in Head Start after being assigned to the treatment group):

Exhibit 5.7: Distribution of Start Dates for Crossovers and Treatment Group Participants

Month of First Day of Head Start Participation	Percent of Crossovers[a] (in Control Group)	Percent of Participants[a] (in Treatment Group)
July 2003 or earlier	9%	6%
August 2003	33%	34%
September 2003	40%	55%
October 2003	5%	4%
November 2003 or later	3%	1%
TOTAL	100%	100%

[a]3-year-old and 4-year-old cohorts combined, unweighted data.

While this does not tell us how the timing of entry for crossovers compares to that of their direct counterparts in the treatment group (who comprise just one-fifth of all treatment group participants), it suggests that a substantial delay in participation in relation to the equivalent subpopulation of the treatment group is unlikely.

On the basis of these data, we assume that outcomes of crossovers, $\overline{Y}_{cr}$, and outcomes of their counterparts in the treatment group, $\overline{Y}_{tr}$, are the same (i.e., we adopt the assumption in Equation 5), making the IOT impact estimate in Equation 13 unbiased for "compliers." Even so, it only applies to Head Start participants from the treatment group who would not have participated if assigned to the control group—i.e., the "complier" subpopulation. It leaves out children who participate under both assignments. It is the combined group of participants in Head Start that defines the national population of children and families served by the program and drives Congressional interest in conducting the current study. Hence, we need to think about how to extend the findings in Equation 13 to encompass all participants, including those who cross-over if put in the control group. To address this challenge, we could take one of two approaches. The first is to assume that the average impact of the program on crossovers—which we cannot directly observe—equals the estimated average impact on "compliers" in Equation 13. That is, assume that:

$$(14) \qquad \overline{Y}_{cr} - \overline{Y}_{cr}^{*} = \overline{Y}_{tk} - \overline{Y}_{ck},$$

where Y_{cr}^{*} is the average outcome that would have occurred for crossovers had they not participated in Head Start, and hence $Y_{cr} - Y_{cr}^{*}$ is the average impact of Head Start participation

on crossovers compared to no participation. The same concept of Head Start's average impact on crossovers can also be carried over to the average effect of Head Start on all treatment group participants in Equation 3:

$$(15) \quad (\bar{Y}_{tp} - \bar{Y}_{cp})^* = S_{pr}(\bar{Y}_{tr} - \bar{Y}_{cr}^*) + S_{pk}(\bar{Y}_{tk} - \bar{Y}_{ck}),$$

where $(\bar{Y}_{tp} - \bar{Y}_{cp})^*$ is the average effect of Head Start on all treatment group participants compared to outcomes for the same children had none of them participated in the program. Substituting terms based on Equation 14, this becomes

$$(16) \quad (\bar{Y}_{tp} - \bar{Y}_{cp})^* = S_{pr}(\bar{Y}_{tk} - \bar{Y}_{ck}) + S_{pk}(\bar{Y}_{tk} - \bar{Y}_{ck}) = \bar{Y}_{tk} - \bar{Y}_{ck},$$

the last step from the fact that (as noted in the definition of S_{pk} at Equation 3 above) $S_{pk} = 1 - S_{pr}$. Thus, when Equation 13 gives us an unbiased estimate of $\bar{Y}_{tk} - \bar{Y}_{ck}$, the average impact on "compliers," it simultaneously gives us an unbiased estimate of the average impact of Head Start on all participants, $(\bar{Y}_{tp} - \bar{Y}_{cp})^*$ under the assumptions in Equations 5 and 14.

However, we see no basis for assuming impacts on crossovers and "compliers" are necessarily equal. Crossovers may be a strongly self-selected and/or program-selected subset of all would-be participants in the control group, the ones who manage to participate even in the face of a study design and random assignment lottery outcomes that says they should not. It is quite possible that they differ from the remaining would-be participants (i.e., the "compliers", to which $\bar{Y}_{tk} - \bar{Y}_{ck}$ directly applies) on factors that would lead to a greater (or smaller) ability to benefit from Head Start's services.

Alternatively, one could examine how large the desired impact estimate for all treatment group participants, $(\bar{Y}_{tp} - \bar{Y}_{cp})^*$, would be for different true values of Y_{tr} - Y_{cr}*. Equation 15, restated in equation 17 with the substitution for S_{pk} just noted, serves as a good basis for doing this:

$$(17) \quad (\bar{Y}_{tp} - \bar{Y}_{cp})^* = S_{pr}(\bar{Y}_{tr} - \bar{Y}_{cr}^*) + (1 - S_{pr})(\bar{Y}_{tk} - \bar{Y}_{ck}).$$

Based on the formula for Y_{tk} - Y_{ck} in Equation 13 and using Equation 10, this can be rewritten as

$$(18) \quad (\bar{Y}_{tp} - \bar{Y}_{cp})^* = S_{pr}(\bar{Y}_{tr} - \bar{Y}_{cr}^*) + (1 - S_{pr})[I^{ITT} / (S_p - S_r)]$$

$$= (S_r / S_p)(\bar{Y}_{tr} - \bar{Y}_{cr}^*) + I^{ITT} / (1 - S_n)$$

$$= [S_r(\bar{Y}_{tr} - \bar{Y}_{cr}^*) + I^{ITT}] / S_p.$$

S_r and S_p in this formula are the share of the treatment group members who participated in Head Start in the first year and the share of control group members who participated in Head Start in the first year, both observed in the data. I^{ITT} is the main impact finding from Chapters 4 through 7 of the *Head Start Impact Study Final Report* (U.S. Department of Health and Human Services, January 2010). This leaves $\bar{Y}_{tr} - \bar{Y}_{cr}^*$, the impact of Head Start on crossovers compared to their outcome levels had they not participated in Head Start, as the only unmeasured piece of the IOT impact estimate. To address this, we do a sensitivity analysis that assumes a variety of different values for $\bar{Y}_{tr} - \bar{Y}_{cr}^*$, each determined as a fraction of the measured impact of Head Start on "compliers", $\bar{Y}_{tk} - \bar{Y}_{ck}$:

Scenario A: $\bar{Y}_{tr} - \bar{Y}_{cr}^* = 0\,(\bar{Y}_{tk} - \bar{Y}_{ck}) = 0$

Scenario B: $\bar{Y}_{tr} - \bar{Y}_{cr}^* = 0.5\,(\bar{Y}_{tk} - \bar{Y}_{ck})$

Scenario C: $\bar{Y}_{tr} - \bar{Y}_{cr}^* = 1.0\,(\bar{Y}_{tk} - \bar{Y}_{ck}) = \bar{Y}_{tk} - \bar{Y}_{ck}$

Scenario D: $\bar{Y}_{tr} - \bar{Y}_{cr}^* = 1.5\,(\bar{Y}_{tk} - \bar{Y}_{ck})$

Scenario E: $\bar{Y}_{tr} - \bar{Y}_{cr}^* = 2.0\,(\bar{Y}_{tk} - \bar{Y}_{ck})$

$(\bar{Y}_{tk} - \bar{Y}_{ck})$ for this purpose is derived from observable quantities using Equation 13: $\bar{Y}_{tk} - \bar{Y}_{ck} = I^{ITT} / (S_p - S_r)$. In all, knowing S_p, S_r, and I^{ITT} is sufficient for conducting this sensitivity analysis.

Applying this approach separately to the 3-year-old cohort and the 4-year-old cohort in the first year of the impact analysis (Spring 2003) illustrates the sensitivity of the IOT findings to different unknown values of $\bar{Y}_{tr} - \bar{Y}_{cr}^*$. We do this for two of the most central cognitive outcomes in the study, PPVT and WJ-III Letter Word Identification, as shown in Exhibits 5.8 and 5.9.

Exhibit 5.8: IOT Sensitivity Analysis for the 3-Year-Old Cohort (S_p = .882; S_r = .185)

Impact of participating in Head Start (IOT) on…	Assumed Value of Y_{tr} - Y_{cr}* as a Multiple of Y_{tk} - Y_{ck}					Impact of Access to Start (ITT)
	0	**0.5**	**1.0**	**1.5**	**2.0**	
PPVT	7.40	8.38	9.37	10.35	11.33	6.53
Effect size	*.21*	*.24*	*.27*	*.30*	*.33*	*.19*
WJ-III Letter Word	6.96	7.89	8.81	9.74	10.66	6.14
Effect size	*.27*	*.31*	*.34*	*.38*	*.41*	*.24*

Exhibit 5.9: IOT Sensitivity Analysis for the 4-Year-Old Cohort (S_p = .834; S_r = .165)

Impact of participating in Head Start (IOT) on…	Assumed Value of Y_{tr} - Y_{cr}* as a Multiple of Y_{tk} - Y_{ck}					Impact of Access to Start (ITT)
	0	**0.5**	**1.0**	**1.5**	**2.0**	
PPVT	4.26	4.79	5.31	5.84	6.36	3.55
Effect size	*.12*	*.13*	*.15*	*.16*	*.18*	*.10*
WJ-III Letter Word	7.17	8.06	8.94	9.83	10.71	5.98
Effect size	*.25*	*.28*	*.31*	*.34*	*.37*	*.21*

As can be seen, most of the IOT estimates have effect sizes noticeably larger than the corresponding ITT effect size, no matter what assumptions go into the IOT analysis, and fall within a fairly tight range between .25 and .35, except for PPVT for 4-year-olds where the range is even tighter (.12 to .18). Based on this lack of sensitivity, we present in the main volume IOT estimates for the middle-ground Scenario C, which makes the assumption that impacts on crossovers are equal to impacts on "compliers" as a first approximation.

To sum up, IOT findings on Head Start's average impact on participants are based on three assumptions (from Equations 5, 6, and 14 above):

$\bar{Y}_{tr} - \bar{Y}_{cr} = 0$ "Outcomes are on average the same for crossovers as for their non-identifiable counterparts in the treatment group"

$\bar{Y}_{tn} - \bar{Y}_{cn} = 0$ "Outcomes are on average the same for no-shows as for their non-identifiable counterparts in the control group"

$\bar{Y}_{tr} - \bar{Y}_{cr} * = \bar{Y}_{tk} - \bar{Y}_{ck}$ "Average impact on crossovers is the same as average impact on 'compliers'".

The first of these assumptions is supported by the data, the second is unobjectionable on principle, and the third does not materially affect conclusions when altered to a considerable extent in the sensitivity analysis.

Annual Cross-Sectional Impact Estimation Methods – Subgroups

Research Questions

All of the above procedures address Head Start's impact on the full set of children and family types that are provided access to Head Start, looking at *average* impacts for the diverse set of children and families. However, impacts are likely to vary across different subsets of the children and families served. For example, Head Start may benefit children whose primary language is not English more than other children (or the reverse), or it may benefit families at the high end of the household risk index more than other families (or the reverse). In addition to an interest in the overall national impact of Head Start, Congress mandated an examination of how impacts vary for different types of children and families. The intent is to understand "what drives the overall impacts" when the program is having an effect of important magnitude for the participant population as a whole. In addition, there is interest in determining the extent to which the benefits of Head Start may be widespread – i.e., whether the benefits reach many types of children and families to produce the overall average effect, rather than benefiting some but having little or no effect on others.

Identifying subgroups of children (or families) that benefit more or less from Head Start may have important policy and program implications. It can suggest areas where the program needs to be strengthened or enhanced to ensure that all participants advance in their development. For example, Head Start programs are required to serve children with special needs so it is important to understand the extent to which these children benefit from their participation over and above an interest in determining if Head Start improves the lives of the average participant. In addition, prior early childhood research has indicated that some groups of children follow different developmental paths and may, as a consequence, be assisted by Head

Start in distinctive ways, such as children from racial and ethnic minority families and non-English speaking children.

This interest in "who benefits?" motivates two types of analyses. The first considers the impact of Head Start on individual subgroups of program participants, asking for example: Does Head Start help children from Hispanic families? Children who start in the lowest quartile of cognitive development scores for Head Start participants? Special needs children? A full set of results defined on these and other dimensions, when considered as a group, will also indicate whether (a) certain subgroups "drive" the overall average impact findings or (b) widespread benefits accrue to many different subgroups.

The second set of analyses involving subgroups considers whether impacts differ in magnitude between distinct types of children and families. For example, Head Start may have smaller effects on children who initially speak little English or larger effects when primary caregivers exhibit depressive symptoms at baseline. Interest in the comparative magnitude of impacts stems from several sources:

- Researchers want to know what factors "moderate" the influence of early childhood services (such as those provided by Head Start) on child development and family functioning. In this case, the term "moderate" means alter the size of the impact of those services when they are provided to one type of child (or family) versus another. For example, the extent to which a child's primary caregiver reports symptoms of depression may moderate how much Head Start is able to help him/her develop good social skills, or a child's primary language may moderate the program's ability to expand reading readiness.
- As noted above, Congress required that the study identify the types of children and families that benefit most from Head start participation, a question that implicitly relates impacts for one type of child/family to impacts for another. For example, do younger children benefit more than older children? Or do families where parents face multiple risk factors benefit more than other families?
- Head Start program operators might seek to enhance services in ways that would particularly benefit subgroups found to be experiencing smaller impacts than other subgroups, such as children with special needs or families with diverse ethnic or linguistic backgrounds

With sufficient data, all subgroup impacts would become apparent when the difference in outcomes between treatment and control group families are compared across subgroups. But because data are limited, the study cannot decisively answer all questions about Head Start's impact on different subpopulations. Still, where evidence is strong that an impact on a particular

subgroup or a difference in impacts between subgroups has occurred, the subgroup analysis will produce a finding that is not difficult to interpret: real impacts in the measured direction have taken place with high probability.

In contrast, a non-significant finding is more ambiguous and could indicate either: (1) that there is in fact no impact, or difference in impact, for some subgroup(s); or (2) that impacts exist but are too small in magnitude to reach the threshold of what the sample is able to detect.

Controlling for Multiple Comparisons in the Subgroup Analyses

The subgroup analyses examined impacts on all of the child and parent outcomes for each of the subgroups (listed below in Exhibit 5.10), for each age cohort, and for each study year. As discussed above, when conducting such "multiple comparisons" there is a modest probability that a finding of a statistically significant difference will emerge by random chance—an event that is known in the statistical field as a "false discovery." To guard against the drawing of firm conclusions generated by false discoveries, the evaluation team statistically adjusted the p-values using the Benjamini-Hochberg (1995) procedure described in the section on the main impact analysis. The results of these adjustments are reported in the report chapters along with the "basic" statistical tests.

Subgroup Definitions

Exhibit 5.10 lists the subgroups defined for the analysis to be presented in the Final Report. All were identified in advance of any data analysis, and chosen on the basis of their program and policy importance to the Office of Policy, Research and Evaluation/Administration for Children and Families (OPRE/ACF), on past Head Start and child development research, and recommendations from the Advisory Committee on Head Start Research and Evaluation. As a precautionary measure, the distributions of the treatment and control groups were compared at baseline with respect to the subgroup variables to ensure that they did not differ significantly before using them for subgroup analysis. Such differences could lead to confounding of the estimated Head Start impacts with the subgroup distributions.[43] In the subgroup analysis, each

[43] One of the subgroup variables, Parental Risk Index, did show a significant difference between treatment and control groups for the age 3 cohort: 8 percent of the treatment group vs. 5 percent of the control group are in the high risk category ($p = .017$).

spring outcome used in the overall impact analysis is examined for each subgroup, separately by age cohort.

Exhibit 5.10: Variables Used To Define Subgroups, Measured At Baseline

Child's Pre-Academic Skills (Lowest Quartile/Not Lowest Quartile)
Child's Home Language (English Speaking/Dual Language Learner)
Special Needs (Special Needs/Not Special Needs)
Biological Mother/Caregiver Race/Ethnicity (White, Black, Hispanic)
Parent-Caregiver Report Depressive Symptoms (No/Mild/Moderate/Severe
Household Risk Index (Low/No, Moderate, High)
Urbanicity (Urban/Not Urban)

A description of the subgroups used in the analyses is provided below.

- Child's Pre-Academic Skills—based on whether the child scored in the lowest quartile of the study population on the baseline assessment of the Woodcock-Johnson III Pre-Academic Skills (comprising of three tests: Letter-Word Identification, Spelling, and Applied Problems). Two subgroups were created using this test score: the child was in the lowest quartile subgroup, or the child was not in the lowest quartile subgroup.
- Child's home language—based on the language in which the child was assessed for the baseline assessment in fall 2002. Two subgroups were created: the child was English speaking, or the child was a Dual Language Learner (See Chapter 3 in this volume and Chapter 2 in the Final Report for how the language for the baseline assessment was determined.) The agreement between the child's testing language and home language is very high (see Exhibit 5.11).
- Special needs—based on the parent's response to the following question on the baseline interview, *"Did a doctor or other health or education professional ever tell you that [CHILD] has any special needs or disabilities—for example, physical, emotional, language, hearing, learning difficulty, or other special needs?"* Two subgroups were created: the child was reported to have special needs, or the child was not reported to have special needs.
- Biological mother/caregiver race/ethnicity—based on the race of the person identified as being most responsible for the care of the child at the time of the baseline parent interview.[44] Three categories were created: White or other,[45] Black, and Hispanic. There was a very high correlation between the child and the biological mother's or primary caregiver's race/ethnicity (see Exhibit 5.11).

[44] The primary caregiver is the child's biological mother for 96 percent of the study children.

[45] Other race (N=94 for the 3-year-old cohort and N=85 for the 4-year-old cohort) was combined with White because the number of other race respondents was too small to study independently.

- Parent/caregiver-reported depressive symptoms—determined from responses to the baseline parent/caregiver interview using the shortened version (12 items) of the Center for Epidemiologic Studies-Depression scale (CES-D) (Seligman, 1993[46]). Four subgroups were created from the scale: (1) no depressive symptoms (score of 0-4), (2) mild depressive symptoms (score of 5-9), (3) moderate depressive symptoms (score of 10-14), and (4) severe depressive symptoms (score of 15-36).
- Household risk index—determined by the number of the following characteristics reported in the baseline parent interview: (1) receipt of TANF or Food Stamps, (2) neither parent in household has high school diploma or a GED, (3) neither parent in household is employed or in school, (4) the child's biological mother/caregiver is a single parent, and (5) the child's biological mother was age 19 or younger when child was born. A child's family score could range from 0 to 5 points. Three categories were created: low/no risk (0-2 risk factors), moderate risk (3 risk factors), and high risk (4-5 risk factors).
- Urbanicity—based on the location of the Head Start center at which the family applied for admission. If the center was located in a Census-defined urbanized area, the family was considered to live in an urban area; if not, the family was considered not to live in an urban area. Thus, two subgroups were defined.

Exhibit 5.11: Agreement between Race of Child and Biological Mother/Caregiver, and between Child Testing Language and Home Language

Agreement between child and biological mother/caregiver race/ethnicity (e.g., of those children who are white, what % have mothers who are white?)		Agreement between child testing language and home language (e.g., of those children tested in English, for what percent of their homes was the primary home language English?).	
White/other	97%	English	95%
African American	98%	Spanish	97%
Hispanic/Latino	94%	Other	87%

Lowest Quartile Determination

We elected to define the set of children who represent the "low ability" group as those who score in the lowest quartile on one of the standardized tests that were administered by the study team. After much discussion and review of the available baseline measures, it was determined that the Woodcock-Johnson III Pre-Academic Standard Cluster (which includes the following tests: Letter-Word Identification, Spelling, and Applied Problems) provides the best definition of lowest quartile for the Fall 2002 English-English group. The cluster is designed to measure broad achievement by assessing pre-reading and letter-word identification skills,

[46] The four depressive symptoms categories are reported on page 101 in the above reference for the 20 item CES-D. The cut points were proportionately adjusted for the shortened version of the CES-D for use in ECLS-B, FACES, and HSIS.

developing mathematical skills, and skill in written production. Exhibit 5.12 provides the distribution for the Fall 2002 Pre-Academic Standard Cluster scores. It should be noted that the 25th percentile cutoff score is for the Head Start Impact Study sample. It is not the 25th percentile score for the publisher's normed population of 3-year-olds or 4-year-olds.

Exhibit 5.12: Distribution of the Pre-Academic Standard Cluster W-ability Scores for the English-English Group, Fall 2002

Cohort	Range	25th Percentile Score
3-year-olds	286-396	319
4-year-olds	286-403	341

Thus all 3-year-olds with a W-ability score of 319 or lower are included in the lowest quartile group. Likewise, all 4-year-olds with a W ability score of 341 or lower are included in the lowest quartile group.

For the fall 2002 Spanish-English[47] group we use the parallel Bateria Woodcock-Munoz-R Skills Cluster (which includes the following tests: Letter-Word Identification, Dictation and Applied Problems) to determine the lowest quartile. This cluster is similar to the Woodcock-Johnson III Pre-Academic Standard Cluster described above. Exhibit 5.13 provides the distribution for the Fall 2002 Skills cluster scores.

Exhibit 5.13: Distribution of the Pre-Academic Standard Cluster W-ability Scores for the Spanish-English Group, Fall 2002

Cohort	Range	25th Percentile Score
3-year-olds	277.7-400.3	347.6
4-year-olds	293.7-424.7	363.7

Thus all 3-year-olds with a W-ability score of 347.6 or lower are included in the lowest quartile group. Likewise, all 4-year-olds with a W ability score of 363.7 or lower are included in the lowest quartile group.

[47] In Fall 2002, a Language Decision Form was used to determine the best language for administering the child assessment. For children residing on the US mainland, if Spanish was determined to be their language of assessment, the children were administered a Spanish assessment with two English subtests (PPVT and Woodcock-Johnson Letter-Word Identification). In subsequent data collections, the Spanish-English group children were administered an English assessment with two Spanish subtests (TVIP and Bateria Woodcock-Munoz Letter-Word Identification). This procedure allows us to measure the child's growth in Spanish and English across all data collection points.

Children who spoke a language other than English or Spanish are not included in the lowest quartile analysis. The other language group children (n = 57) were administered only non-standardized tests (e.g., Color Identification, Counting Bears, etc.) in fall 2002.

Impact Estimation Formulas for Subgroups

A computationally efficient and statistically powerful way to compare subgroups utilizes the impact regression equations discussed earlier. For a given subgroup, a single regression can provide information on how large an impact Head Start has on each of the subgroup categories, and how impacts vary across subgroup categories. This analysis interacts the indicator variable for assignment to the Head Start group with each subgroup-defining covariate in turn (or with each set of subgroup-defining covariates when a given dimension defines more than two subgroups such as mother's race/ethnicity), allowing impact to vary with that factor.

To formalize this, let *Z* represent the subgroup-defining variable (e.g., a 0/1 indicator for children with special needs) or a set of subgroup-defining variables (e.g., two out of three subgroup indicator variables that divide the sample by mother's race/ethnicity). As before *T* is the treatment indicator, ***X*** the vector of demographic covariates and time of assessment variable (if included), and *R* the "residualized" fall measure. As in the main impact analysis, if the residualized fall measure is entered separately by language group, the *R* term in the model is replaced by *L* (the language subgroup indicator, *L*=1 for English, *L*=0 for Spanish/Other) and its two-way interaction with *R*. For outcomes taken from the teacher survey and teacher child rating form, the model omits the *R* and *L* terms entirely since there is no pre-test measure for these outcomes. (Note that *Z* may or may not have been among the covariates previously included in the regressions as part of the vector of background variables, ***X***; if not, it is added now to the regression for the estimation of subgroup impacts.) For continuous outcomes, the impact regression model for the i^{th} child becomes

$$Y_i = \alpha + \beta\ T_i + \boldsymbol{\gamma}'\mathbf{X}_i + \delta\ R_i + \eta\ Z_i + \zeta(Z_i T_i) + e_i$$

For binary outcomes the impact model is:

$$\ln\frac{P_i}{1-P_i} = \alpha + \beta\ T_i + \boldsymbol{\gamma}'\mathbf{X}_i + \delta\ R_i + \eta\ Z_i + \zeta(Z_i T_i)$$

where,

$$P_i = Pr(Y_i = 1) = \frac{exp(\alpha + \mathbf{X}\beta T_i + \mathbf{R}_i + \mathbf{Z}_i + (\eta Z_i T_i)\zeta_{i\,i}}{1 + exp(\alpha + \beta T_i \mathbf{R}_i + \mathbf{Z}_i \delta_i (Z_i T_i)) + \zeta_{i\,i}}.$$

Continuous Outcome Measures

For continuous outcomes, all estimation, model fitting and hypothesis testing for subgroup analysis (as was true of the main analysis) was done with the SUDAAN software package, using the full-sample and jackknife replicate weights to take into account the complex sample design (i.e., stratification, clustering and weighting) in the estimation of standard errors for the regression coefficients and the impact estimates. To see how this methodology produces impact estimates for subgroups and tests of differences in the size of impact between subgroups, consider an example with *Y* as a continuous outcome variable (such as a child's PPVT score) and *Z* a two-way indicator variable distinguishing special needs children (*Z*=1) from non-special needs children (*Z*=0):

$$Y_i = \alpha + \beta\ T_i + \boldsymbol{\gamma}^{'}\mathbf{X}_i + \delta\ R_i + \eta\ Z_i + \zeta(Z_i T_i) + e_i$$

The coefficients from this model used in the analysis are:

- β, the coefficient on the indicator variable for assignment to the treatment group, which estimates the impact of Head Start on non-special needs children, the subgroup of children *not* flagged by the subgroup indicator variable *Z* (*Z*=0). For these children, the regression equation reduces to $Y_i = \alpha + \beta T_i + \gamma' X_i + \delta R_i + e_i$, paralleling the equation used previously to determine impacts on all children.
- ζ, the coefficient on the interaction of the treatment group indicator variable and the subgroup indicator variable, which estimates the difference between the impact of the intervention on special needs children (*Z*=1) and the impact on non-special needs children (*Z*=0).
- $\beta + \zeta$, the estimate of the impact of Head Start on special needs children, the subgroup of children flagged by the subgroup indicator variable *Z* (*Z*=1). For these children, the regression model becomes $Y_i = (\alpha + \eta) + (\beta + \zeta)T_i + \gamma^{'} X_i + \delta R_i + e_i$, again paralleling the equation previously used to determine impacts on all children but with different coefficients on T_i and $\boldsymbol{X}_i$. Statistical significance tests on this linear combination of coefficients tell us whether this impact differs significantly from 0.

A further variation of the subgroup analysis occurs by replacing *Z* with a *collection* of two or more categorical variables. This occurs when looking at subgroups defined by mother's race/ethnicity and the parental risk index. In both cases, we use two *Z* variables, call them Z_1 and

Z_2, which flag—for example—children with Hispanic and non-Hispanic black mothers respectively. The continuous version of the regression equation in this instance becomes

$$Y_i = \alpha + \beta T_i + \gamma' X_i + \delta R_i + \eta_1 Z_{1i} + \eta_2 Z_{2i} + \zeta_1 (Z_{1i} T_i) + \zeta_2 (Z_{2i} T_i) + e_i$$

Several coefficients from this model are used in the analysis:

- β estimates the impact of Head Start on children with non-Hispanic white mothers, the subgroup of children *not* flagged by either subgroup indicator (the $Z_{1i} = 0$, $Z_{2i} = 0$ subgroup). For these children the regression equation reduces to $Y_i = (\alpha + \beta T_i + \gamma' X_i + \delta R_i + e_i$, paralleling the equation previously used to determine impacts on all children.
- $\beta + \zeta_1$ estimates the impact of Head Start on children with Hispanic mothers (the $Z_{1i} = 1$, $Z_{2i} = 0$ subgroup), for whom the regression model becomes $Y_i = (\acute{\alpha} + \eta_1) + (\beta + \zeta_1) T_i + \gamma' X_i + \delta R_i + e_i$, paralleling the equation previously used to determine impacts on all children but with different coefficients on T and X.
- $\beta + \zeta_2$ estimates the impact of Head Start on children with non-Hispanic black mothers (the $Z_{1i} = 0$, $Z_{2i} = 1$ subgroup), for whom the regression model becomes $Y_i = (\acute{\alpha} + \eta_2) + (\beta + \zeta_2) T_i + \gamma' X_i + \delta R_i + e_i$, paralleling the equation previously used to determine impacts on all children but with different coefficients on T and X.
- Differences in impact between various pairs of subgroups can be calculated and tested using ζ_1 (Hispanic versus non-Hispanic white), ζ_2 (non-Hispanic black versus non-Hispanic white), and $\zeta_1 - \zeta_2$ (Hispanic versus non-Hispanic black).

Binary Outcome Measures

As in the main analysis, to calculate impacts on binary outcomes for subgroups the equation for ln (P / 1-P) is specified just as was the equation in the continuous case (except for the log-odds conversion of the left hand-side variable). Once estimated using logistic regression, we again solve for P = Pr(Y =1) as an exponential translation of the log-odds ratio. From there, the predicted marginal for the treatment group is calculated for each subgroup category by first evaluating the Pr(Y =1) equation for each child, with the treatment indicator set to 1 for the entire sample of respondents and the remaining variables set to their actual values for the child, to obtain the predicted probability of the outcome for each child. The weighted average of the predicted probabilities across children belonging to the subgroup is the treatment predicted marginal for the subgroup. The predicted marginal for the control group is calculated similarly for each subgroup category, with the treatment indicator set to 0 for the entire sample. The formula for the predicted marginal is given in Graubard & Korn (1999) as:

$$\hat{P}(r) = \frac{\sum_{i=1}^{n} w_i \delta_i \hat{P}_i(r)}{\sum_{i=1}^{n} w_i \delta_i}$$

where r is the level of the causal effect variable (r = treatment or control), $\hat{P}_i$ is the predicted probability that the binary outcome $Y_i = 1$ for the i-th child, with the r-th level indicator set to 1 for every record for the treatment predicted marginal, and set to 0 for every record for the control predicted marginal,

n is the number of sampled children,

w_i is the full-sample weight for the i-th child, and

δ_i is a dummy for the subgroup of interest (ex. 1=urban, 0=rural).

The impact estimate for a particular subgroup category is calculated as the difference between the treatment (r = treatment) predicted marginal and the control (r = control) predicted marginals for that subgroup category. Different subgroup categories are compared by calculating the pairwise differences in impact estimates between the subgroup categories, i.e., the "difference of differences". For example, to test whether the impact of Head Start on the likelihood of receiving dental care in the last 12 months is different for children in rural and non-rural areas, the predicted marginals in both the treatment condition and the control condition are calculated for the rural and non-rural and all four combinations used: treatment/rural, treatment/non-rural, control/rural, and control/non-rural. The impact estimate for each rural/non-rural subgroup is calculated as the difference in the respective predicted marginals:

$$I_{rural} = \hat{P}_{rural,T} - \hat{P}_{rural,C}$$
$$I_{nonrural} = \hat{P}_{nonrural,T} - \hat{P}_{nonrural,C}$$

The difference in impact estimates for rural and non-rural is calculated as:

$$I_{rural} - I_{nonrural} = (\hat{P}_{rural,T} - \hat{P}_{rural,C}) - (\hat{P}_{nonrural,T} - \hat{P}_{nonrural,C}).$$

The hypotheses H_0: $I_{rural} = 0$, H_0: $I_{nonrural} = 0$, and H_0: $I_{rural} - I_{nonrural} = 0$ are tested using a t-test, with the standard error of the difference or the difference of differences calculated in SAS using the jackknife replicate weights to reflect the complex sample design. For binary outcomes,

the calculation of predicted marginals, impact estimates, and standard errors for subgroups and the hypothesis testing of subgroup impacts and differences in impacts were programmed in SAS using the jackknife replicate weights, as the SUDAAN software (v.9) does not subset the sample by subgroup in calculating predicted marginals for subgroups as the Graubard & Korn formula requires. Instead, it averages over the entire sample with the subgroup category indicator set to 1 for every record—i.e., it omits the δ_i 0/1 indicator for the subgroup of interest from the predicted marginal equation. Since our goal is to compare the effect of Head Start for different subgroups, the SUDAAN calculations are not appropriate for our analysis.

The approach described above extends to analyses of three related subgroups, such as mother's race/ethnicity. Here, the model becomes

$$\ln\frac{p}{1-p} = \alpha + \beta T + \boldsymbol{\gamma}'\mathbf{X} + \delta R + \eta_1 BLACK + \eta_2 HISPANIC + \zeta_{11}(BLACK * T) + \zeta_{12}(HISPANIC * T)$$

where *BLACK*, *HISPANIC* are 0,1 indicators for the mother's race/ethnicity, and White/Other is the omitted reference group. The impact estimates for the race/ethnicity groups are

$$I_{white} = \hat{P}_{white,T} - \hat{P}_{white,C}$$
$$I_{black} = \hat{P}_{black,T} - \hat{P}_{black,C}$$
$$I_{hispanic} = \hat{P}_{hispanic,T} - \hat{P}_{hispanic,C}$$

and the differences in impacts among the race/ethnicity groups are:

$$I_{white} - I_{hispanic} = (\hat{P}_{white,T} - \hat{P}_{white,C}) - (\hat{P}_{hispanic,T} - \hat{P}_{hispanic,C})$$
$$I_{white} - I_{black} = (\hat{P}_{white,T} - \hat{P}_{white,C}) - (\hat{P}_{black,T} - \hat{P}_{black,C})$$
$$I_{hispanic} - I_{black} = (\hat{P}_{hispanic,T} - \hat{P}_{hispanic,C}) - (\hat{P}_{black,T} - \hat{P}_{black,C})$$

where $\hat{P}_{hispanic,T}, \hat{P}_{hispanic,C}, \hat{P}_{black,T}, \hat{P}_{black,C}, \hat{P}_{white,T}, \hat{P}_{white,C}$ are the predicted marginals in the treatment and control conditions for each race/ethnicity category.

Two final restrictions are placed on the subgroup analyses. First, to avoid findings that may exaggerate contrasts between subgroups due to the vagaries of small-sample analysis, subgroups with fewer than 50 observations in either the treatment or control group for any age cohort are not examined. Second, certain observations could not be included in particular subgroup analyses for certain moderators. In particular, children who could not be assessed in either English or Spanish in fall 2002 due to lack of familiarity with these languages are dropped

from the analysis sample when examining subgroups based on children's pre-academic composite score at baseline.

Repeated-Measures Impact Analysis Methods

This section describes the derivation of the longitudinal analysis findings on cognitive impacts presented in Chapter 4 of the Final Report using hierarchical linear modeling (HLM) methods to estimate the effect of access to Head Start on the average annual growth rate of five cognitive outcomes, for the three to four year follow-up period covered by the study (depending on children's age at entry into the program). HLM is an approach to analyzing data in nested hierarchies that takes this clustering into account when calculating the significance tests of estimated effects (Raudenbush and Bryk, 2002). The method is particularly well suited to framing questions about changes over time—e.g., does cognitive development over time grow along a steeper linear trajectory when children are given access to Head Start—because it models at its most irreducible level the growth trajectory of individual children. This results in more accurate estimates of changes over time in Head Start's impact than cross-sectional impact estimates, subject to the assumption of a linear growth rate.[48]

While the cross-sectional analysis of Head Start impact in any single year, using SUDAAN software, models the clustering of children within centers and of centers within grantees/delegate agencies in a way that yields correct standard errors for the impact estimates, it cannot model effects that vary randomly over groups—for example, when the amount of growth per year varies randomly from child to child. By looking at only a single year at a time, it also cannot model the growth over time of individual children. The HLM approach on the other hand can estimate effects that vary from center to center and it also estimates individual growth trajectories. However, a limitation of this software is that it can incorporate only part of the multistage stratified sample design used in the Head Start Impact Study when estimating the standard errors of effects. The current HLM software accommodates the nesting of time points within children and the nesting of children within centers but—unlike SUDAAN for cross-

[48] If (i) all the cross-sections had identical samples (or perfect longitudinal weights were used), (ii) all the same covariates were used at each time point, and (iii) all covariates measured perfectly, the estimate of the difference in growth rates between the treatment and control groups in the cross-sectional analysis would have the same expected value for any given outcome as the HLM estimate. Since these conditions are not met by the data, HLM gives the better measure of linear growth rates and Head Start's impact thereon.

sectional analysis—not the nesting of centers within grantees/delegate agencies. However, a simulation study has shown that for the types of models used here HLM yields standard errors that are correct for treatment effect estimators (Jenkins, Lee, Cheach, & Leytush, 2006).

HLM Model Specification for the Head Start Impact Study

The application of HLM used in the Head Start Impact Study is a three-level model which describes observations of time points within students, outcomes for students within centers, and the distribution of center means. Suppose that Y_{jit} is the outcome for child i in center j measured at time point t. At level 1 of the model, this outcome is related to W_{it}, the wave of data collection for child i at time t. At level 2, the initial achievement level and rate of growth in achievement are related to a child's background characteristics, $X_{1ji} \ldots X_{pji}$ (e.g. child's gender, primary language, family composition). In addition, the rate of growth at this level is potentially influenced by whether the student was assigned to the Head Start group or to the non-Head Start control group. A third level of the model accounts for the clustering of students in within centers without specifying any additional relationships among variables.

Level 1 – Time Within Students, describes the different time points of data collection for a given child. For the age three cohort there are five waves: baseline (fall, 2002), 1st followup (spring 2003), second followup (spring 2004), 3rd followup (Spring 2005), and final wave (spring, 2006). For the age four cohort only the first four waves are included.

Level 1 – Time Points Within Child:

$$(1) \qquad Y_{jit} = \pi_{0ji} + \pi_{1ji} W_{jit} + e_{jit} \ ,$$

where Y_{jit} = The outcome for child i in center j when measured at time point (i.e., data collection wave) t.

W_{jit} = Data collection wave in which Y_{jit} is observed, coded 0, 1, 2, 3, 4 for the 3-year-old cohort and 0, 1, 2, 3 for the 4-year-old cohort.

π_{0ji} = The estimated initial level of achievement for child i in center j at time point 0 (i.e., in fall 2002).

π_{1ji} = The linear growth parameter for child I in center j indicating how much that child grew in achievement between successive observation points.

e_{jit} = A random error term for child i in center j at time t.

It is assumed that the random error, e_{jit} has variance σ^2 that is fixed across all time points, children, and centers[49] and is independent of errors at other levels of the model.

Level 2 – Children Within Center, describes the way individual child characteristics relate to the level and growth rate of child outcomes within a given Head Start center. These are expressed as regression coefficients predicting the child-specific parameters in Level 1 of the model, π_{0ji} and π_{1ji}:

$$(2) \qquad \begin{aligned} \pi_{0ji} &= \beta_{00j} + \beta_{01}X_{1ji} + \cdots + \beta_{0p}X_{pji} + r_{0ji} \\ \pi_{1ji} &= \beta_{10} + \beta_{11}T_{ji} + \beta_{12}X_{1ji} + \cdots + \beta_{1(p+1)}X_{pji} + r_{1ji} \end{aligned}$$

Where X_{1ji} to X_{pji} are child characteristics that predict level and growth of achievement,[50]

T_{ji} is an indicator of treatment condition to which child i was assigned (0=control, 1=Head Start), which affects the growth rate of child outcomes if access to Head Start has an impact on children's development

β_{00j} is the average baseline achievement level of children in center j,

$\beta_{01} \cdots \beta_{0p}$ are the fixed effects of child background characteristics on baseline achievement levels, assumed to be the same for all children and all centers,

β_{10} is the average annual growth of child achievement across all children, assumed to be the same in every year

β_{11} is the impact of being assigned to the treatment group on the average annual growth of child achievement across all children, assumed to be the same in every year. This is the key estimate needed from the model, measuring Head Start's impact on the annual growth rate of child achievement.

$\beta_{12} \cdots \beta_{0(p+1)}$ are the fixed effects of student covariates on child achievement growth, assumed to be the same for all children at all centers,

r_{0ji} is the random effect associated with child i's baseline achievement level at center j,

r_{1ji} is the random effect associated with child i's annual growth rate in achievement at center j.

[49] The HLM software requires this uniform residual variance assumption at the lowest level of the model.

[50] This assumes that same child background characteristics influence initial achievement levels and achievement growth. Note that for estimation purposes all covariates are grand-mean deviated.

Level 3: Between Centers

(3) $\beta_{00j} = \gamma_{000} + u_{00j}$, where

γ_{000} is the average baseline achievement level of all children in all centers,

u_{00j} is the random effect associated with center j's average baseline achievement level.

Interpreting the Multilevel Model

The level 1 model (Equation 1) depicts an individual growth curve, that is, it describes how an individual's cognitive proficiency grows over time. This is defined as linear growth which is characterized by an initial starting point, π_{0ji} , and a yearly increment of growth, $\pi_{1ji.}$ for each child i in each center j. The level 2 model (Equation 2) describes how the intercepts and slopes of the growth line (π_{0ji}, π_{1ji} again) vary from one child to another within a center. A child's intercept term, π_{0ji} , is a function of a center mean baseline achievement level, β_{00j} , the effect of various child covariates, $\beta_{01} \cdots \beta_{0p}$, and a random error term for each child in center j, r_{0ji} . Similarly, the child's rate of growth over time, $_{,}\pi_{1ji,}$ is a function of the average growth rate at all center, β_{10} , the increment to growth due to access to Head Start following random assignment to treatment, β_{11} , the effect of child background characteristics, $\beta_{12} \cdots \beta_{0(p+1)}$ (assumed the same in all centers), and a random error term for each child in center j, r_{1ji} .

The level 3 model depicts center means, β_{00j} , as a function of a grand mean, γ_{000} , and a random center effect, u_{00j} . As mentioned before, this part of the model is included simply to account for the clustering of students within centers.

The main focus of the HLM analysis is the effect of treatment, T_{ji} , on students' growth, β_{11}. A positive value for β_{11} would imply that those given access to Head Start (the treatment group) grow in achievement more than those not given access (the control group). For example, since time is years, if β_{11} has a value of three this means that for each year of follow-up, the treatment group grows three more points than the control group on the outcome.

In other words, β_{12} is the estimate of the impact of treatment assignment on growth. In addition, this impact estimate is controlled for the values of the child covariates X_{1ji} to X_{pji}. Including covariates in the model increases the statistical precision of the impact estimate.

Estimating the Multilevel Model

Longitudinal analysis is done using the above model to obtain measures of Head Start's "intent to treat" (ITT) impact on children's annual growth rates for five cognitive outcomes: PPVT and four assessments from the Woodcock Johnson III, Letter Word Identification, Spelling, Applied Problems, and Pre-Academic Skills Composite. These measures were chosen as the only outcomes that were collected in every year of the study that can be vertically equated and for which steady growth is expected based on child development theory and prior research.

Like the main findings from cross-sectional analysis, each of the age cohorts is considered separately. For each age cohort and outcome measure, the analysis sample consists of all children for whom the outcome was observed at least two time points, which could include the baseline "pretest" observation in fall 2002 or any of the outcome measurement points in spring 2003, spring 2004, spring 2005, and—for the 3-year-old cohort—spring 2006. Thus, any sample member could have from two to five observations of Y_{jit} in the estimation sample, for t = 0, 1, 2, 3, and/or 4. This length of follow-up carries all children potentially through the end of 1st grade, although for some the final outcome observation at that age may be missing (just as any previous time points, so long as at least two total time points are observed).

Special longitudinal weights were constructed for this sample and used in forming all of the estimates. These are described in Chapter 2 of this report.

Based on these samples and weights, the HLM software produced the figures presented in Exhibits 5.4 and 4.7 of the main report for each outcome. These include the following estimates:

-- the average annual linear growth rate for children in the control group sample,

-- the average annual linear growth rate for children in the treatment group sample, $\hat{\beta}_{10} + \hat{\beta}_{11}$,

-- the estimated effect of Head Start on the average annual linear growth rate, $\hat{\beta}_{11}$,

-- the p-value for $\hat{\beta}_{11}$, from which one can decide if the regression coefficient differences significantly from zero at the alpha = .05 level (or any other level of

significance) and hence if there is strong evidence of impact of Head Start access on children's linear growth trajectories.

Methodological Refinements Since Previous Interim Report

In a small number of instances, the analysis methods used here differ from those used the study's previous impact report (U.S. Department of Health and Human Services, Administration for Children and Families, 2005), which looked only at impacts in the first year following random assignment. These refinements, made in situations where the study team identified ways to improve the analytic techniques applied, account for the minor differences in findings reported here for that year (the spring 2003 results) and in the earlier report. These refinements include the following:

- Scoring the PPVT and CTOPPP cognitive tests using separate prior distributions for the treatment and control groups and updating those priors each year from the previous year's data.
- Collapsing continuous measures of color score and number of times parents read to their children into two-way categorizations to reflect the fact that each variable took on only a few discrete values and did not have the bell-shaped (i.e., normal) curve for its distribution among sample members that estimation of impacts on continuous measures assumes. By converting each measure to a binary indicator coded one for all children above a certain threshold zero otherwise, we are able to estimate impacts using logistical methods (applied as well to other binary measures) that do not make the normality assumption.
- Three refinements were made in adjusting the impact estimates to take account of background characteristics of the children and families analyzed:
  - Demographic covariates measured in fall 2002 but at some interval after random assignment and (for the treatment group) Head Start program entry were limited to only those measures that could not have been affected by the program in only a few weeks. Covariates dropped because of the possibility of an early impact of Head Start are caregiver depression scale, caregiver's self-reported health status, grandparent living in the home, number of residential moves in the last 12 months, household monthly income range, and household receipt of TANF. The indicator of a child having special needs was also dropped because of the possibility that participation in Head Start may quickly have lead to increased identification of this characteristic.
  - Pre-test values of key outcome measures, when used as covariates, were "residualized" on the treatment/control group indicator variable before use—again, to protect against the possibility of the measures being systematically higher or lower for the treatment group than the control group due to an early impact of Head Start.

- A covariate was added to measure the date of testing in the spring of each year, to assure that differences in timing over the several-week data collection interval did not lead to higher outcome levels for one or the other experimental group because of later testing and, hence, greater maturation independent of the intervention due simply to aging.

- Finally, estimation methods and statistical testing procedures were modified in two ways:
  - For the multiple comparisons made as part of the subgroup impacts, the Benjamini-Hochberg procedure was used to reduce the likelihood of false discoveries of statistical significance.
  - Average impacts were calculated using the predicted marginal methodology, rather than the direct observation of the estimated coefficient on the treatment group indictor (for continuous outcomes) and the conditional marginal methodology (for binary outcomes) used in the previous report.

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