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ABSTRACT

West Virginia shares with the rest of the nation a common sense brand of human capital theory that sees improved education as the corrective for unemployment and economic decline. Early and continuing intervention for less advantaged students is acquiring the status of a favored educational and long-term economic development tool. This study suggests that context determines educational outcomes, not the other way around. In December 1991, the Administration for Children, Youth, and Families (ACYF) instituted a pilot program aimed at maintaining through grade 3 the gains that accrue from Head Start participation. Post-Head Start Transition now has demonstration projects in 31 states. At one site, located in two county school districts in rural western West Virginia, 211 students were followed from kindergarten through grade 3. The students attended seven control schools and five schools providing transition services: staff development on developmentally appropriate education, health care for students, encouragement for parent participation, and assistance in securing needed social services. Quantitative program evaluation consisted of analysis of annual results of five achievement tests mandated by ACYF. Findings indicate that neither Head Start participation in preschool nor transition participation in the primary grades affected achievement scores. Contains 53 references, 8 data tables, and detailed discussion of statistical analyses. (SV)

FOSTERING ACHIEVEMENT THROUGH POST-HEAD START TRANSITION: THE CASE OF RURAL WEST VIRGINIA

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FOSTERING ACHIEVEMENT THROUGH POST-HEAD START TRANSITION: THE CASE OF RURAL WEST VIRGINIA

In 1992, a private organization of wealthy West Virginia entrepreneurs approached the Marshall University Center for Economic Progress with a request for applied research. They wanted to know the education and training needs in the eight poor, rural counties which consititute southern and eastern West Virginia. "Need," in this context, meant education and training needed to enable recipients to escape unemployment, underemployment, and poverty.

The report which the request prompted went on for eighty-six pages. Using conventional concepts and methods, it explained that there were no education and training needs in southern and eastern West Virginia, not so long as "need" was construed as above. There was, however, an enormous need for jobs, the kind that were stable and paid a living wage (Marshall University Center for Economic Progress, 1992).

Rural West Virginians, the report judged, rise to the occasion. They have a long history of making good use of whatever opportunities are available. Their economic needs should not be construed obliquely in innocuous educational terms.

Evidently, the report was not taken seriously.

If it were, policymakers would not construe increases and declines in average achievement test scores as portending economic success, economic failure, or much of anything else economic for rural West Virginia.

It might occur to them, however, that shifts in measured achievement may follow economic development and decline, rather than precede it. That, it seems reasonable to surmise, in one of the lessons of the following.

RURAL WEST VIRGINIA

West Virginia has come to epitomize Appalachian underdevelopment and rural insularity. More than sixty-four percent of the state's population lives in rural areas, the second highest percentage in the U.S. (Seal and Harmon, 1995). West Virginia's per capita income remains the second lowest among the fifty states (Marshall University Center for International Programs, 1997). The dropout rate among students who have not yet reached the ninth grade is roughly 40 percent (Lange and Bickel, 1997). Since overall dropout rates are usually computed using the ninth grade as the base year, recent reports that the state's high school completion rate now exceeds eighty-five percent are likely to be badly misleading (Bickel and Lange, 1995).

In 1993 only 32 percent of West Virginia's high school graduates enrolled in a college or university (Lowe and Bickel, 1993). The State Department of Education has not reported college enrollment rates since that year. The percentage of West Virginia's population made up of college graduates is the lowest among the fifty states (West Virginia Kids Count Leadership Collective, 1996).

In spite of hardships, however, rural West Virginian's, traditionally, have left their home state only under economic duress. Given the economic wherewithal to return, moreover, they readily do so (Bickel, Arthur, and Spatig, 1994). Thus, one reason the state's population is no longer declining is a small in-migration surplus attributable to returning natives (Nyden, 1994).

The much-maligned rural Appalachian culture is, as a matter of concrete, everyday living, a culture of community (Howley, 1997). Out-migration continues only because born-and-bred West Virginia's are forced to foresake extended family and other local ties, leaving the state in search of employment (Moses, 1998).

COMMONSENSE HUMAN CAPITAL THEORY

State policymakers have been nearly unanimous in

expressing painful awareness of the lack of econonomic opportunity in West Virginia. They give voice to an understanding of at least some of socially deracinating, culturally destructive consequences of West Virginian's lack of economic control of their lives (Charleston, [WV] Daily Mail, 1993). As with others in similar positions, the most frequently invoked corrective for unemployment, underemployment, and loss of traditional industries in this non-agricultural but thoroughly rural state is improved education (see, for example, LaBelle, 1998).

Such judgments are intrinsic to a taken-for-granted, commonsense brand of human capital theory which West Virginia shares with the rest of the country. The pervasiveness and plausibility of this perspective is evident when comparing documents as diverse as now-classic theoretical statements by Nobel prize-winning economists Gary Becker (1962) and Theodore Schultz (1962) with more recent observations limited to West Virginia (see, for example, Joint Commission on College Attendance, 1988; West Virginia Board of Regents, 1988; Partnership for Progress, 1990). School-to-work legislation based on uncritical valorization of the assumptions on which this perspective is based has transformed the curriculum in West Virginia secondary schools (Howley, 1996).

EARLY INTERVENTION

Recently, state legislators, the governor, the chief state school officer, and other prominent members of the executive branch have expressed a new and intensifying interest in early childhood and elementary education (Smith, 1998). As with those reporting from large urban settings, officials in this rural state are now convinced that systematic educational intervention for the economically less fortunate should begin before kindergarten starts, and continue long after it ends.

In this view, programs such as the venerable Project Head Start were and are a good idea, indeed. Now they need to be expanded and made more educaton-intensive. The payoffs in terms of achievement in the elementary grades will be substantial, will provide a solid foundation for subsequent educational achievement, and, later still, will foster occupational and income gains (Zigler and Styfco, 1994; Currie and Thomas, 1995).

Early and continuing intervention for less advantaged students is acquiring the status of a favored educational and, for the long term, economic development tool (see, especially, Farkas, 1996). This applies whether the setting is rural or urban.

Whether or not this view has merit, its compelling pervasiveness prompted the Administration for Children, Youth, and Families (ACYF) to institute in December of 1991 a pilot program aimed at maintaining the gains that ostensibly accrue from prekindergarten intervention. In this instance, the prekindergarten endeavor is Head Start. The pilot program intended to maintain Head Start gains through the first three grades of elementary school is called Post-Head Start Transition, or simply Transition. At an annual cost of thirty-five million dollars, there are now Transition Demonstration Projects in thirty-one states.

One of the sites is located in rural West Virginia, including two school districts on the state's western border with Ohio and Kentucky. Each district has an established Head Start program, both of which operate under the administrative auspices of the same social services agency.

Participation in Head Start is determined by income, with those least advantaged enrolled first. Participation in Transition was determined by random selection of six elementary schools in each of the two counties, and then

random assignment of treatment and control status to each of the schools.

To provide the Transition (treatment) group and the control group with roughly the same number of students, five schools became Transition schools. All children in these schools, whether they had been in Head Start or not, whether they were economically disadvantaged or not, received Transition services. The remaining seven schools constituted the control group.

Two cohorts of students were identified, one made up of children beginning kindergarten in 1992, and the other group beginning kindergarten in 1993. All students were tested and parents interviewed at the beginning and end of kindergarten, and then again at the end of first, second, and third grades.

With the two cohorts intact, there were three hunderd fifty-four students at the outset, divided almost equally between Transition and control groups. Sixty-two percent of the students had attended a local Head Start. The analysis reported herein is done with the two hundred eleven students remaining at the end of the third grade. Clearly, attrition was a problem, as discussed below.

As an intervention program, Transition is conventional. It consists of four components: staff development for teachers provided by an early childhood education specialist who authored the local grant proposal; health care for students provided or obtained by a full-time registered nurse; encouragement for parental involvement through establishing parent rooms in schools and organizing activities involving parents, their children, and school personnel; and assistance in securing needed social services, provided by a full-time social worker at each school.

Implementation of Transition was monitored by program specialists from ACYF as well as by contract evaluators. The expectation was that healthy children from comfortable homes with educationally engaged parents would be better able to achieve. Similarly, teachers who understood the meaning of developmentally appropriate education would be better able to promote achievement (Spatig, Bickel, and Parrot, In press).

EVALUATING TRANSITION IN RURAL WEST VIRGINIA

Transition in West Virginia has quantitative and qualitative evaluation components. Results of the quantitative evaluation are reported below.

Independent variables are described in Table 1, and dependent variables are described in Table 2.

TABLE 1 ABOUT HERE

TABLE 2 ABOUT HERE

Descriptive statistics for Transition participants and non-participants are reported in Table 3. Comparison of means for the two groups reveals no perfectly consistent pattern of substantively interpretible advantage for either group. Nevertheless, it is the case that differences between average scores on two of the three pretests is statistically significant, working to the advantage of the non-participant group.

TABLE 3 ABOUT HERE

The statistical controls intrinsic to multiple regression analysis are, of course, intended to compensate for confounding among independent variables (Gujurati, 1995: 194-197).

Furthermore, Tables 4, 5, and 6 report logistic regression analyses in which we use the Transition participation/nonparticipation dichotomy as the dependent variable. The full complement of independent variables, including first the Woodcock-Johnson 22 (Table 4), then the Woodcock-Johnson 25 (Table 5), and then the Peabody Picture Vocabulary Test (Table 6) is used in each equation predicting Transition participation/nonparticipation.

The results indicate no systematic advantage for either the Transition participation group or the Transition nonparticipation group. Accordingly, when all controls are in place, confounding of Transition with other explanatory factors will neither mask nor exaggearte program effects.

TABLE 4 ABOUT HERE

TABLE 5 ABOUT HERE

TABLE 6 ABOUT HERE

ATTRITION

Furthermore, as is often the case with longitudinal studies, attrition, as noted above, is a serious issue in this research. Beginning with three hundred fifty-four kindergarten students, at the end of third grade only two-hundred eleven provided complete information, representing forty percent attrition. What are the consequences of this for our evaluation of Transition?

In Table 7 we report results of a logistic regression analysis with attrition, yes/no, as the dichotomous dependent variable. All independent variables used in our evaluation of Transition, including the three pretest scores, are included in this equation, as is the Transition dummy variable.

Since none of the logistic regression coefficients is statistically significant, we conclude that attrition has made it neither more nor less likely that we will find Transition effects.

TABLE 7 ABOUT HERE

A METHODOLOGICAL COMPLICATION: REGRESSION TOWARD THE MEAN

In analyses not reported here, gain scores were used as dependent variables, with regression equations otherwise specified as in this paper. As anticipated, the most powerful predictors of gain score dependent variables were pretest scores. However, the partial regression coefficients corresponding to the pretest independent variables were negative. The same was true of the bivariate correlations between pretest scores and gain scores. Negative relationships between pretest scores and gain scores are manifestations of regression toward the mean (Willett, Ayoub, and Robinson, 1991).

Recent treatments of regression toward the mean, while consistent with the pioneering work of Campbell and Stanley (1963), have usefully emphasized that regression toward the mean is a manifestation of measurement error in the pretest (Pedhazur and Schmelkin, 1991: 284-286; Samsa, 1992). Adjustments for regression toward the mean, thus, are based on the magnitude of pretest measurement error (see, for example, Reynolds, 1984).

While procedures for adjusting for this statistical artifact are available (see, for example, Edwards, Yarvis, Mueller, Zingale, and Wagman, 1978; Furlong and Feldman, 1992), they typically are used to assure that measured group differences and individual predictions are not contaminated. Little attention has been given to improving estimates of partial regression coefficients corresponding to pretest scores. The prevailing judgment seems to be that if gain scores are not used, regression toward the mean is not a concern (Hendrickson and Jones, 1987: 86-107).

This sanguine assumption, however, is unfounded. Once we interpret regression toward the mean as a consequence of measurement error in the pretest, it becomes clear that failure to make adjustments biases the estimate of the partial regression coefficient corresponding to this independent variable (Kmenta, 1997: 346-366). Similarly, failure to correct for regression toward the mean also introduces bias into estimates of coefficients corresponding to independent variables with which the pretest is associated (Chatterjee and Price, 1991).

Adjusting for Regression Toward the Mean

A simple procedure to adjust for regression toward the mean can be readily devised from existing sources. First, use the Edwards-Nunnally procedure to correct pretest scores (Speer, 1992). This is done by multiplying the pretest-posttest correlation times each score's deviation from the mean, then adding the mean back in.

Second, the corrected scores are then used to estimate the unstandardized partial regression coefficient corresponding to the pretest. Though the unstandardized coefficient has now been corrected, the corresponding standardized regression coefficient, summary statistics, and the equation's predicted values still will be erroneous.

To correct these statistics, fix the value of the pretest's unstandardized coefficient to its adjusted value, and run the entire regression again, using the original (uncorrected) values of the pretest scores. Even for statistical software packages which do not have built-in procedures for fixing coefficient values, this third step is not difficult. Create a new dependent variable by subtracting the product of the corrected coefficient and the uncorrected pretest scores from the original dependent

variable. Then regress this new dependent variable on the remaining independent variables (Kennedy, 1979: 55). This provides corrected unstandardized and standardized partial regression coefficients for the remainder of the complement of independent variables.

The standardized partial regression coefficient for the pretest can be computed by multiplying the unstandardized coefficient by the ratio of the standard deviations of the dependent variable and the pretest (Knoke and Bohrnstedt, 1994: 273). (Given that the unstandardized coefficient has already been corrected, the standard deviation of the original pretest scores is used as the denominator.)

Finally, to find R-squared, each standardized coefficient is multiplied by its corresponding bivariate correlation coefficient, and the products are summed (Duncan, 1975: 63-66). The formula for adjusted R-squared is widely reported (Knoke and Bohrnstedt, 1994: 293).

REGRESSION RESULTS: TRANSITION IN WEST VIRGINIA

Results of our evaluation of Post-Head Start Transition in two contiguous West Virginia counties are not encouraging. The regression analyses intended to account for differences in measured third grade achievement among two hundred eleven students who entered kindergarten in

approximately equal numbers in 1992 and 1993 are reported in Table 8. Our findings with regard to achievement gains attributable to Transition participation are of primary interest.

TABLE 8 ABOUT HERE

PRETESTS AND OUTCOME MEASURES

As with all other measures, the five achievement tests used in this evaluation were mandated by ACYF. Two of the five, the Woodcock-Johnson 23 and Woodcock-Johnson 24, had dramatic floor effects when used with kindergarten and first grade students. As a result, suitable pretest scores are not available, and regression results for these measures are not reported.

The three remaining tests proved suitable for use with kindergarten students, yielding approximately normal distributions of test scores, with neither floor effects nor ceiling effects. The three usable tests are well known, widely used, and have published reliabilities (see Table 2). Two of the tests, the Peabody Picture Vocabulary Test (PPVT) (Dunn and Dunn, 1981) and the Woodcock-Johnson 22 Letter-Word Identification Test (Wooddcock and Maher, 1990) are used as measures of verbal achievement. The

third, the Woodcock-Johnson 25 Applied Problem Solving Test (Woodcock and Maher, 1990) is a measure of basic mathematics problem solving skills.

In spite of the fact that we are using the PPVT as a measure of verbal achievement, it is often construed as a measure of verbal ability. The developers of the test off-handedly claim that it can be used to measure either construct, depending on circumstances. The circumstances which occasion one interpretation rather than another are not specified. Testers are admonished, however, that use of the PPVT as a "quick" measure of verbal ability should be restricted to "subjects who have grown up in a Standard English-speaking environment" (Dunn and Dunn, 1981: 2).

We acknowledge that uncertainty as to the status of the PPVT renders its use problematic for evaluating Transition. However, since both the Woodcock-Johnson 22 and Woodcock-Johnson 25 are widely acknowledged as useful achievement tests, readers who have reservations about the PPVT as a measure of achievement may ignore analyses which use this test. Furthermore, programs such as Head Start, and now Post-Head Start Transition, have been identified by some as means of ehancing not only achievement, but ability or aptitude, as well (Ramey, Yeates, and MacPhee, 1984; Ramey and Ramey, 1994). In any case, as will be evident below, assessments of the efficacy of Transition in

promoting achievement lead to the same conclusions for all three tests.

Pretests were administered at the beginning of kindergarten. Posttests were administered at the end of the third grade.

The specific question we are asking is this: does Transition participation maintain Head Start gains, enabling students, on the average, to do better from kindergarten through the third grade than otherwise would be the case? Does Transition participation provide an advantage over non-participants who are otherwise similar?

NO HEAD START GAINS TO MAINTAIN

The partial regression coefficients reported in Table 8 make clear that evaluation of the effect of Transition is not as straightforward as we might have expected. Again, the purpose of Transition is to maintain Head Start gains. However, for each of our three outcome measures, the partial regression coefficient corresponding to Head Start is statistically non-significant. Head Start participation provides no advantage to maintain.

This is sharply at odds with the what has become taken-for-granted, unassailable conventional wisdom (see, for example, Halsey, 1989; Zigler and Styco, 1994; Zigler, 1995), shared not only by academicians and policymakers, but by the public at large (Hood, 1993). It is sharply at odds, moreover, with the rationale for developing and implementing Transition (Ramey, Ramey, and Phillips, 1996).

The absence of Head Start effects may be programatically disappointing, and it certainly raises issues unanticipated in planning the evaluation of Transition. Nevertheless, the no-effect finding is difficult to dismiss as an aberration. After all, the sample size is two hundred eleven cases. With no variance inflation factor larger than 1.12, this is certainly large enough so that statistical power is not an issue (Kraemer and Thiemann, 1987: 62-65; Fox, 1997: 338-339.)

In addition, three different outcome measures were used. This, in effect, gives the Head Start variable three chances to show statistically and substantively significant positive effects, effects to be maintained by Transition.

Furthermore, a reasonably full complement of controls is in place, substantially diminishing the likelihood that program effects are masked due to confounding of the Head Start variable with uncontrolled contaminating factors. Historically, proper specification of the regression model

has been an especially troublesome issue in Head Start research and evaluation (Currie and Thomas, 1995).

The typical absence of controls for prior achievement has been of a serious limitation of much of this earlier work (Lee, Brooks-Gunn, Schnur, and Liaw, 1990). This deficiency, commonplace in early school effectiveness research, minimizes the importance of school (or program) effects, and exaggerates the influence of out-of-school factors (Levin, 1976). The unfortunate schools-don't-make-a-difference slogan, current throughout the 1960'S and 1970's, had its genesis in research deficient in this way, a deficiency which our analysis does not share,

In addition, and again in contrast to most other research and evaluation dealing with Head Start, the Transition data set does not limit us to comparing Head Start students with a heterogeneous aggregate of other same-age students. Instead, those who attended no preschool can be systematically distinguished from those who attended public and private preschool programs other than Head Start (see Lee, Brooks-Gunn, and Schnur, 1988). As a result, we are not limited to determining if Head Start is measurably better than no preschool. We can also determine if Head Start is measurably better than other kinds of preschool.

As already noted, moreover, all two hundred eleven students in our data set were selected from twelve elementary schools, six in each of two contiguous West Virginia school districts. In analyses done with the Transition data set but not reported here, the twelve schools were used to construct eleven dummy variables. When the eleven school dummies were used as independent variables in a regression equation with our dependent variables as outcomes, none were statistically significant. There is no reason to suspect, therefore, that Head Start students, or Transition students, for that matter, are in less effective schools than others, thereby masking real program effects (see Lee, Brooks-Gunn, Schnur, and Liaw, 1990).

As best we can determine, if the purpose of Head Start is to contribute to leveling the educational playing field by giving preschool assistance to less advantaged students, it is not working in Cabell and Wayne Counties in West Virginia. And the same is true of Transition.

Examining the partial regression coefficients corresponding to the Transition participation/nonparticipation variable is also disappointing. Even though there are no Head Start gains to maintain, one might hope that Transition itself would have a favorable impact on measured achievement. However, none of the coefficients corresponding to Transition is

statistically significant. There is no evidence that participation in Transition provides achievement advantages.

It remains true that Transition was designed to maintain gains rather than produce them. Nevertheless, in the absence of Head Start gains to maintain, the finding that Transition itself provides no achievement advantages becomes pertinent.

CAUTIONARY REMARKS

Whatever the merits of the foregoing, Donald Campbell and his associates have argued against overall analytical strategies such as we have employed here (Cook and Campbell, 298-301; also see Magidson, 1977). In their judgment, the regression-based approach which we have adopted, with progam participation/nonparticipation represented by a dummy variable, invites misinterpretation through exaggeration of programmatic impact by the statistically naive and the substantively tendentious.

It is important to recognize, however, that Campbell's critique applies not just to application of dummy variables in program evaluation. Instead, at least implicitly, he has issued an indictment of usual applications of ordinary least squares multiple regression analysis for all purposes

other than prediction. This applies whether using dummy variables or interval/ratio level variables.

Campbell is especially troubled by exaggeration of the importance of small and substantively inconsequential, though statistically significant, standardized regression coefficients. It is abundantly evident by now, however, that exaggeration of programmatic impact is not a characteristic of the analyses presented here. (A useful antidote to Campbell and associates' long-standing, idiosyncratic scepticism regarding ordinary least squares estimates of regession coefficients for dummy variables is provided by Hardy, 1993.)

Serious questions as to generalizability, however, do characterize our analyses. After all, two contiguous counties in western West Virginia are not the whole world, nor the rural world, nor even the rural U.S.

Nevertheless, if Transition does not work here, where will it work? After all, West Virginia, as exemplified by this two-county rural area, is poor and committed to educational solutions to economic problems. The recent and intense interest in early childhood and elementary education makes the state seem even more hospitable to a program such as Transition.

DISCUSSION AND CONCLUSIONS

What are we to make of our findings? What we have found with certainty is what everyone knows: prior achievement makes a substantial difference. Beyond that, we have found that Transition makes no difference at all. Since there are no Head Start effects to maintain, Transition can hardly accomplish what was intended. In and of itself, moreover, it contributes nothing to promoting measured achievement among this group of rural West Virginia elementary school students.

The inability to say much more may be a gauge of our lack of understanding of schooling generally, of rural schooling specifically, of ways to make schooling of any sort more effective, and of strategies for doing summative evaluations (Howley, Bickel, and McDonough, 1997). Consistent with this view, some have argued that the flexibility and exploratory potential of ethnography may offer insights which are otherwise unavailable (see, for example, Spatig, Bickel, and Parrot, In press).

Champions of Head Start have often questioned the suitability of standardized achievement test scores in evaluating early intervention programs. To these observers, narrowly focused quantification seems premature (see, for example, Hatch, 1995). If standardized tests are

used at all, perhaps they are best used in conjunction with ethnography, or so some have argued (Spatig, Bickel, and Parrot, 1994).

Whether or not these claims are accurate, Transition extends well beyond the years that are commonly subsumed by the concept "early childhood education." Given the nature of elementary education in the contemporary U.S., routinized quantification is a fact of life. This holds without regard to how much or how little we know about schooling.

It is certainly pertinent, that the (still unrealized) plans for the federally mandated national evaluation of all thirty-one Transition sites is unambiguously quantitative in nature (Head Start Bureau, 1996). The people who designed the program fully expect it to make measurable differences, benefitting less-advantaged students, particulary those who once participated in Head Start. In our analysis, these expectations have not been met.

U.S. policymakers and the public at large stubbornly persist in assuming that there is an education-intensive fix for everything. However, the evaluation of Transition reported here is taken from two counties in a rural state which, for the past eleven years, has spent more than two-thirds of its budget for public education. The figures for

1996 and 1997 were seventy-two percent. The state remains, however, one of the poorest states in the nation.

Whatever Transition does or does not do, in the Appalachian state of West Virginia it is not an effective agency for promoting school achievement, nor is it an effective agency of progressive economic development.

At the outset, we raised the possibility that measured educational achievement may rise and fall in response to economic development and decline. Context determines educational outcomes, not the other way around. Perhaps that explains at least some of the trouble with Transition in rural West Virginia.

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TABLE 1

INDEPENDENT VARIABLES

Pre-School/In-School Experience

HEADSTART	Head Start Participation, Scored 1 if Yes, 0 Otherwise.
PRESCHOOL	Other Pre-School Participation, Scored 1 if Yes, 0 Otherwise.
TRANSITION	Transition Participation, Scored 1 if Yes, and 0 Otherwise.

Student Characteristics

PRETEST	Achievement Test Score at Beginning of Kindergarten.
SOCIAL SKILLS	Social Skills Scale Score at Beginning of Kindergarten. Eight Likert Items with Three Responses to Each. (Cronbach's Alpha=.86)
GENDER	Child's Gender, Scored 1 if Male, 0 Otherwise.
ETHNIC!	Child's Ethnicity, Scored 1 if White, 0 Otherwise.
CHILD HEALTH	Parent Respondent's Assessment of Child's Health, in Five Levels.

Family and Household Characteristics

FAMILY INCOME	Family Income, in Twelve Levels.
PARENT'S EDUCATION	Parent Respondent's Education Level, in Ten Levels.
PARENTING SKILLS	Parenting Skills Score at Beginning of Kindergbarten. Twenty-six Likert Items with Five Responses to Each. (Cronbach's Alpha=.71)
PARENT'S HEALTH	Parent Respondent's Assessment of His/Her Health, in Five Levels.

Contextual Factors

PERCENT RURAL	Percent of Students in the School District Classified as Having Rural Residence.
COHORT	Scored 1 for Kindergarten in 1992, 0 for Kindergarten in 1993.

! Only 23 students in the sample are non-white.

TABLE 2

Definitions of Dependent Variables

PEABODY	Peabody Picture Vocabulary Score, End of Kindergarten. (Split-Half Reliability=.80*)
WOOD22	Spring Woodcock-Johnson 22 Score, End of Kindergarten. (Split-Half Reliability=.91*)
WOOD25	Spring Woodcock-Johnson 25 Score, End of Kindergarten. (Split-Half Reliability=.84*)

*Published reliabilties. Reliabilities for all other instruments were computed from the sample data used in the analyses reported here.

TABLE 3

TRANSITION:

	PARTICIPANTS	NONPARTICIPANTS
	MEANS (STANDARD DEVIATIONS)	MEANS (STANDARD DEVIATIONS)
HEADSTART	0.59 (0.49)	0.65 (0.48)
*PRESCHOOL	0.36 (0.48)	0.11 (0.32)
SOCIAL SKILLS	1.29 (0.24)	1.39 (0.26)
GENDER	0.55 (0.50)	0.61 (0.49)
*ETHNICITY	0.84 (0.37)	0.99 (0.09)
CHILD HEALTH	4.24 (0.93)	4.24 (0.90)
FAMILY INCOME	4.90 (2.59)	5.30 (2.27)
PARENT'S EDUCATION	4.95 (1.74)	4.75 (1.50)
PARENTING SKILLS	4.37 (0.52)	4.53 (0.49)
*PARENT'S HEALTH	4.24 (0.93)	3.56 (1.04)
*PERCENT RURAL	0.81 (0.39)	0.51 (0.50)
COHORT	0.53 (0.50)	0.56 (0.50)
*PRETEST (PEABODY)	57.19 (8.88)	59.67 (7.57)
*PRETEST (WOOD22)	9.70 (2.21)	10.96 (2.83)
PRETEST (WOOD25)	16.50 (1.28)	16.97 (1.04)
*PEABODY	98.82 (15.51)	102.43 (11.32)
*WOOD22	36.11 (7.85)	38.64 (6.39)
WOOD25	33.01 (4.38)	33.07 (3.92)

* Statistically significant difference between groups means, $p<.05$

N=111 N=110

TABLE 4

Logistic Regression Results
Cohorts 1 and 2

SELECTION BIAS

BEGINNING OF KINDERGARTEN

	B	S.E.
HEADSTART	-0.28	0.38
PRESCHOOL	2.05***	0.57
SOCIAL SKILLS	-1.61	0.89
GENDER	-0.33	0.38
ETHNIC	-2.28*	1.11
CHILD HEALTH	0.14	0.21
FAMILY INCOME	-0.08	0.09
PARENT'S EDUCATION	0.07	0.13
PARENTING SKILLS	-0.72	0.41
PARENT'S HEALTH	-0.03	0.21
PERCENT RURAL	0.75	0.42
COHORT	0.17	0.39
WOOD22 (Pretest)	-0.16	0.09

N=211

*P<.05
**P<.01
***P<.001

TABLE 5

Logistic Regression Results
Cohorts 1 and 2

SELECTION BIAS

BEGINNING OF KINDERGARTEN

	B	S.E.
HEADSTART	-0.41	0.44
PRESCHOOL	2.13***	0.58
SOCIAL SKILLS	-1.87*	0.87
GENDER	-0.33	0.37
ETHNIC	-2.33*	1.12
CHILD HEALTH	0.14	0.21
FAMILY INCOME	-0.12	0.09
PARENT'S EDUCATION	0.03	0.13
PARENTING SKILLS	-0.63	0.40
PARENT'S HEALTH	-0.02	0.21
PERCENT RURAL	0.71	0.44
COHORT	-0.02	0.38
WOOD25 (Pretest)	-0.12	0.17

N=211

*P<.05
**P<.01
***P<.001

TABLE 6

Logistic Regression Results
Cohorts 1 and 2

SELECTION BIAS

BEGINNING OF KINDERGARTEN

	B	S.E.
HEADSTART	-0.40	0.45
PRESCHOOL	2.10***	0.57
SOCIAL SKILLS	-1.98*	0.86
GENDER	-0.37	0.37
ETHNIC	-2.34*	1.14
CHILD HEALTH	0.14	0.21
FAMILY INCOME	-0.13	0.09
PARENT'S EDUCATION	0.01	0.13
PARENTING SKILLS	-0.64	0.40
PARENT'S HEALTH	-0.02	0.20
PERCENT RURAL	0.82	0.42
COHORT	-0.02	0.37
PEABODY (Pretest)	0.01	0.02

N=211

*P<.05
**P<.01
***P<.001

TABLE 7

Logistic Regression Results
Cohorts 1 and 2

ATTRITION FROM BEGINNING OF KINDEREGARTEN

	B	S.E.
HEADSTART	0.01	.43
PRESCHOOL	-0.10	.47
SOCIAL SKILLS	-0.30	.76
GENDER	-0.11	.34
ETHNIC	0.44	.69
CHILD HEALTH	0.08	.20
FAMILY INCOME	-0.04	.08
PARENT'S EDUCATION	0.05	.13
PARENTING SKILLS	-0.01	.39
PARENT'S HEALTH	0.01	.18
PERCENT RURAL	-0.03	.42
COHORT	-0.08	.34
PEABODY (Pretest)	0.01	.01
WOOD22 (Pretest)	-0.09	.09
WOOD25 (Pretest)	0.06	.20
TRANSITION	-0.11	.38

N=289!

! Sixty-five of the three hundred fifty-four cases were lost due to use of listwise deletion with incomplete responses.

TABLE 8

Regression Results
Cohorts 1 and 2

Unstandardized and (Standardized) Coefficients

	PEABODY	WOOD22	WOOD25
	END OF THIRD GRADE		
HEADSTART	2.15	-1.09	-0.63
	(.07)	(-.08)	(-.04)
PRESCHOOL	-3.81	-0.38	-0.42
	(-.10)	(-.03)	(-.03)
TRANSITION	-1.00	1.28	0.36
	(-.04)	(.10)	(.02)
PRETEST	0.90***	1.64***	1.91***
	(.54)	(.57)	(.52)
SOCIAL SKILLS	-0.67	0.96	2.67
	(-.01)	(.04)	(.09)
GENDER	2.17	2.31*	2.33*
	(.08)	(.18)	(.15)
ETHNICITY	-4.66	3.38*	2.62
	(-.10)	(.16)	(.11)
CHILD HEALTH	1.55	0.49	0.51
	(.10)	(.07)	(.06)
FAMILY INCOME	-0.08	0.19	0.08
	(-.01)	(.07)	(.03)
PARENT'S EDUCATION	-0.81	-0.06	0.30
	(-.08)	(-.02)	(.06)
PARENTING SKILLS	1.06	-0.48	-0.92
	(.04)	(-.04)	(-.06)
PARENT'S HEALTH	-1.73	0.31	0.32
	(-.12)	(.05)	(.04)
PERCENT RURAL	4.88*	1.96*	1.31
	(.16)	(.15)	(.08)
COHORT	-4.60*	-0.46	0.33
	(-.16)	(-.04)	(.02)
R-Squared	49.7%	43.8%	40.6%
Adjusted R-Squared	45.3%	38.8%	35.4%
	N=211	N=211	N=211

*P<.05
**P<.01
***P<.001

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	(-.10)	(-.03)	(-.03)
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	(-.01)	(.07)	(.03)
PARENT'S EDUCATION	-0.81	-0.06	0.30
	(-.08)	(-.02)	(.06)
PARENTING SKILLS	1.06	-0.48	-0.92
	(.04)	(-.04)	(-.06)
PARENT'S HEALTH	-1.73	0.31	0.32
	(-.12)	(.05)	(.04)
PERCENT RURAL	4.88*	1.96*	1.31
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**P<.01
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