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ABSTRACT

Arguing that summaries of 562 studies from 1985 through 1994 reveal that almost no attention is paid to the cognitive effects of Head Start programs, this report criticizes Head Start as being steadily and more generously funded without evidence that it helps children's scholastic achievements. The report criticizes available studies as practicing poor data analysis and omitting pertinent information. The report also argues that the only substantiated positive effects of Head Start include reduction of the number of children being sent to special education and reduced grade retention due to socialization factors alone. The first section of the report offers criticism of the data analysis and omission of information of an article by Schweinhart (1994) on the lasting benefits of preschool programs. The second section addresses the need to study more critically the data presented in research reports. The third section provides background information on early childhood education for disadvantaged youth in the United States. The final section describes reactions to Head Start weaknesses in Follow Through programs and the government support they received. The report concludes by noting that there are some lasting effects of Head Start, mainly of a non-significant variety, and that significance-producing programs should be looked at carefully and negative programs should be addressed for counter-productivity. (SD)

Cognitive Effects of Early Childhood Programs

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Cognitive Effects of Early Childhood Programs

Two revealing insights into the status of the cognitive effects of Head Start can be derived from studying a fairly recent (1994) article by Schweinhart (ERIC DIGEST EDO-PS-94-2). The main data cited therein are those of the "sleeper effect" year -about a decade earlier.

The fact that the data cited are so old means at least two things. First, since the High/Scope research group can be presumed to be intimately aware of all Head Start studies, there must be virtually nothing else in the literature up through 1994 relating to cognitive effects. Otherwise it would not have been necessary to refer to data a decade old. Schweinhart acknowledges that "clear evidence of the gradual disappearance of effects has been found only for gains in children's scores on tests of their intellectual performance, and not for other positive effects of programs." Second, those other effects can be reliably judged from the data cited.

In the article's section on Effects on School Performance, the first paragraph states that there were significantly better intellectual performances for a year or two after completion of the program. Paragraph 2 mentions that the gains fade away over time. Paragraph 3 mentions that several studies including that from the High/Scope program found fewer program children were placed in special education classes. Paragraph 4 mentions that this holds also for being retained in grade. Paragraph 5 mentions that program children are more likely to graduate from high school.

Thus, when their first wave of children has reached age 27 years, the High/Scope group still has published no data indicating lasting significantly improved scholastic achievement scores by the program children. Accordingly, as admitted in the quotation above, the effects mentioned in paragraphs 3-5 are to be classified as being due to socialization.

We will later (below) examine the actual data for the socialization effects that were claimed.

The Need to Study Data

As an example of how one can be misled by accepting stories and claims without demanding and examining data, consider the already mentioned so-called "sleeper effect".

One of the more striking claims of the High/Scope group publicized what was called a "sleeper effect" which noted that, after a gap of at least five years of no intervention, in the eighth grade, the scholastic performances of the experimental children were a full year ahead of those of the control group. The claim was mathematically correct, but concealed what, in my opinion, was both a clear lack of success and a radically different interpretation of their data. What happened was that the eighth grade control group tested at a 4th grade level while the experimental group tested at a 5th grade level. This is indeed a 1 year difference, but how can one trumpet as a success any program that left the children 3 years behind the norms in the eighth grade?

The total school achievement scores in 8th grade are on page 68 (Table 13) of a recent publication of the High/Scope group (Schweinhart et al, 1993). The values are 122.2 for the program children versus 94.5 for the non-program children; the difference is significant at the $p = 0.001$ level.

This looks great until one examines the absolute numbers. The next <u>preceding</u> data are for grade 3 whose scores are 252.4 and 242.4, respectively. The drop-off by eighth grade may indicate a severe decline in both sets of achievements, with a greater decline for the non-program children. As already pointed out, these numbers mean that, in grade 8, the program children tested at 5th grade level while the non-program children tested at 4th grade level, and I remarked that it is hard to accept as a success a program that leaves children 3 grades behind in eighth grade.

Moreover, their test data showed that both groups of 14-year-old children had mental ages of 11 (3 years below normal). Thus, the experimental group performed at its own mental age level while the control group performed one grade below. What then has to be explained is <u>not</u> the performance of the experimental group but

that of the control group. The project that produced this result had begun utilizing the concept of assessing progress in terms of the proportion of children retained in grade or sent to special education classes for children not progressing normally. Both these measures relate mainly to the socialization of the children rather than to their academic performances. It is not difficult to believe that the extra attention during early schooling helped regularize the classroom behavior of children to the point that teachers found them less troubling in class and, therefore, placed fewer of them in those two categories of sub-par performance. The control children were less well socialized and therefore ended up performing below their mental levels. It is also possible that some of this difference is another instance of what is called the teacher-expectation effect.

It is no wonder that little mention of the scholastic achievements can found in the publicity for Head Start. From the point of view of data analysis, the omission of the information I have added indicates a need for at least more care, if not more candor, on the parts of even those diligent High/Scope personnel.

We now turn to the question of why and when there occurred a shift of Head Start goals from cognitive effects to socialization. When the data were assembled (Weikart et al, 1970) after several years of the Head Start project now called the High/Scope group in Ypsilanti, Michigan, on page 2 of that report it was noted that experimental children were somewhat less likely to be retained in grade or to be sent to special education classes, <u>though not significantly so in either measure,</u> and not significantly enhanced in most cognitive measures. Almost at once, the designers of High/Scope began asserting that these socialization measures were always the main goal of the work. Yet, as late as 1979, on page 4 of High/Scope's publication (Monograph Number Six: The Ypsilanti-Carnegie Infant Education Project -Longitudinal Follow-Up by A.S. Epstein and D.P. Weikart) is found: "The primary objective for infants was full development of their emerging cognitive skills".

This is not to deny the importance of the evident socialization and health gains of the experimental children. It is important that, as described in the April 20, 1993 N.Y. Times, follow-up of the initial experimental High/Scope group showed that, nearly 25 years later, they earn more money, have fewer drug problems, have a greater percentage of home-owners, etc. All these aspects would be quite expectable from more socialized or better behaved persons. Yet, we should still study the actual data to grasp their pragmatic significance. These showed that, for example, "some 59 percent of the (experimental) students required help from some social services, like welfare, (while) 80 percent of those in the control group required social services." This is indeed an improvement, but there should be only restricted acclaim for a program that left 59% of its participants needing social services. In numbers, the program helped about 1/4 of the participants to improve this aspect of their functioning. This is much more than zero, but the problems of 3/4 of the disadvantaged children remained unsolved!

It is evident that Head Start needs significant improvement if we are to approach the scholastic goal that generated Head Start. But, both the national government and the education establishment ignore or don't read the data so they see no reason to cope with demands to help the children scholastically.

Critiques of other intervention programs have appeared (e.g. Spitz, 1993), showing that none is free of significant problems in execution and reveal a lack of cognitive success when the children reach the regular school grades.

Background

Early childhood education in the U.S.A. received great attention starting in the 1960's when an attempt was begun to prevent the school failure of many children from disadvantaged groups. The idea was that many parents from such socially, culturally, and economically disadvantaged situations could not give their children the culturally-needed collections of

experiences and attitudes to enable their children to profit from typical schools as then constituted. So, the reasoning went, we will try to give these children a "head start" by working with them during the years just before they enter first grade. These Head Start programs were created in many parts of the U.S.A. by many different groups of psychologists. It is significant that few, if any, such programs emerged from the school systems themselves; educators seem to have been too close to the problem to have been impelled to take novel actions to stop the problem from appearing. Instead, schools tried handling the matter in regular classes with regular teachers.

The programs created by the various Head Start experimenters were used on a variety of pre-school children in many states. The strategies adopted were of many different kinds, running from heavily academic to an emphasis on language mastery to psychologically-supportive to play therapy. They usually were begun during the fall of the year in which children reached age 3 1/2 to 4 years. In this way the children would have 2 full years of the upgrading programs before they entered the regular schools in kindergarten or first grade.

The results of trying a variety of strategies quickly showed that short sessions, such as summer programs, were insufficient to increase the likelihood of later school success as judged mainly by the fact that the children's IQ's remained low: in the 70-80 range, generally.

Longer programs, generally lasting during both pre-first grade school years, were found to have significant effects on IQ, raising them to virtually normal levels by the time of arrival in first grade. What was unexpected was the subsequent sharp fall in IQ so that, generally by 3rd grade, the IQ's of the experimental children were down at the level of the control children who had received no special inputs. In addition to the cognitive aspects, health questions were included in some Head Start Programs, though the precipitating problem was the need for upgrading the scholastic abilities of many such children. The detailed data from the High

Scope version of Head Start will be given later along with their evaluation.

Although by this early point it had become clear that there were no shining cognitive successes among the pre-school programs, some of the researchers continued to claim success and apply for and get large sums of money to support the maintaining and extending and even exporting of their Head Start programs!

Head Start has become so much like motherhood that there have been essentially no attempts to determine its lasting cognitive effects for many years even though 562 studies are summarized in a 1994 report to the Department of Health and Human Services entitled "Head Start Research from 1985 to 1994". Those summaries reveal almost no attention to cognitive effects, let alone containing relevant data. So, Head Start is being steadily and more generously funded without evidence that it helps children's scholastic achievements. It seems to be true that Head Start helps reduce the number being sent to special education or retained in grade, but there is no evidence that these two gains result from other than socialization deriving from greater interpersonal contact during the program. Some would call this simple custodial care. In addition, health aspects have had some signification gains in that, for example, most Head Start children have had all their recommended immunization shots.

Follow Through

Reactions in the 1970's to the revealed scholastic achievement weaknesses of Head Start were varied and expectable. Some experimenters suggested that the regular schooling situation was so bad that it undid the gains of the pre-school programs. Others indicated that some serious gains had been made and would show up later. For members of the first group, two possible changes could be made. One could beef up the quality (whatever that means) of teachers and teaching in the first few regular school grades so that the children didn't lose the gains of the pre-school programs. This idea seems never to have been taken

seriously. Instead, another version of change was proposed: extend the period of special schooling for the disadvantaged children another few years so as to solidify the gains. In this way we would have the chance to "Follow Through" with some special programs to continue to enhance the children's new abilities.

This was an excellent proposal and Follow Through programs received significant government support over the next few years (Brown et al, 1978).

In 1977-1979, Abt Associates of Boston published its commissioned evaluation of the Follow Through programs. Their criterion of results was to number the instances in which experimental children in a school did better, as a class, than the control children and then subtract the number of instances in which classes of control children performed better than the experimental children. If there was no effect of the Follow Through program, then the result would be zero. If there was a positive effect, the result would be positive, and if the control children outperformed the experimental children, the criterion would be negative.

What was totally unexpected was that, when the children were evaluated after grades 3 and 5, 11 of the 13 programs produced negative results! That means that 11 expert research groups changed the schooling of children in grades 1, 2, and 3 and made it worse! One group produced essentially a zero result, meaning that the program had no detectable effect, and one group in Oregon produced a definitely positive result after grade 3. There is still some controversy about that one positive result since, entirely by accident, (since no one knew what the evaluation materials would be) that program chanced to use in their instruction some materials chosen for the evaluation process. The controversy became moot because that program's own data showed that, by grade 5, any gain had vanished.

Because of the extensive presentation of the work of the High/Scope group, it should be of interest that this group was one of the 13 attempting Follow Through programs. The Abt Associates ranked the programs in order of overall effectiveness, though by

their ranking, as already mentioned, only two programs produced non-negative results. Number 13 was High/Scope.

The upshot of the discussion to this point is that the intervention programs have little, if any, short or long term scholastic effects, certainly none of any practical significance. Further, the attempts to extend the program to a total of 5 years (2 Head Start plus 3 Follow Through) produced such a clear-cut failure to help the children educationally that it is likely that Head Start by itself could have helped very little.

The net result of the Head Start kind of intervention programs we have mentioned seems to be that the teaching profession doesn't yet understand enough about the teaching/learning processes to help children develop more effectively. The money spent acquiring this information was well spent, provided the data and logical conclusions are accepted. In such a situation, it would seem advisable to take the next step by trying entirely different and novel kinds of educational approaches on the grounds that the traditional Head Start ones don't work well enough.

Why, then, was Head Start not phased out before now? There are two main reasons. First, some investigators claimed, and continue to claim, that their programs produced significant positive results. Second, although admitting the insignificant educational results, other supporters of Head Start changed the game by claiming that, after all, the main goals of such programs were social and health-related and that these latter goals were being amply achieved, so they should not be terminated.

The latter claims have recently been evaluated (Zigler et al, Public Health Journal, 1994). It is to be hoped, for the sakes of the children, that the claims are valid. Multi-hour per week interactions with children could hardly fail to help on socialization goals, but it would be desirable for someone with expertise in the fields of those two goals to attempt a serious evaluation of those claims <u>in competition with alternative ways of approaching such goals.</u>

It is remarkable also that school systems could not even

evaluate programs accurately. The available data show that the Head Start programs have produced virtually no scholastic improvements of the children they have trained in pre-school programs. There are some effects on parents and children so that such children are somewhat more likely to seek higher education. But, by measures of academic performance, there has been no significant gain. Yet, parents, teachers, education specialists, most newspapers, politicians, etc have continued to acclaim the programs which supposedly have solved most of the early school problems of disadvantaged youth.

There is no intention of slighting the importance of socio-cultural factors. We can only wonder what might happen with such children were a cognitively effective program (e.g. a cognitive level matching program) also installed. The enticing possibility is that we might reach a situation in which children perform at cognitive levels much above what is expected, let alone reached, today.

As admitted by groups working in the early Head Start projects, there was a general impression that cognitive gains were minimal. After many years of studying Head Start because it ran mainly during a slow brain growth period, I became convinced that the Head Start organization would never try to improve its cognitive aspects, so in 1988 I published that there were still no indications of significant scholastic/intellectual gains from Head Start (Human Intelligence Newsletter, p 11). During the winter of 1991-1992 articles appeared in the popular press also pointing out that evaluations of Head Start show no really significant intellectual gains (see NEWSWEEK Jan 27, 1992 or The N. Y. Times, March 4, 1992). A detailing of many weaknesses was contained in the Dec 18, 1992 monograph of the CATO Institute entitled "CAVEAT EMPTOR: THE HEAD START SCAM". (It is interesting that the recent review of Head Start by Washington and Bailey (1995) includes the CATO monograph in the list of references, but doesn't discuss its negative conclusions). Similar critical articles appeared again in 1993 on page 43 of the March 8 TIME, on page 1 of the N.Y. Times of

March 19, and on page 57 of the April 12 issue of NEWSWEEK.

To illustrate the outlook of those involved in Head Start reviews, the book by Washington and Bailey (1995) maintains that Head Start programs have yielded many positive results in terms of effects on parents, communities, and governments. Though what they cite seems believable, these are still only stories, without data. And, it is not unlikely that the kinds of positive results they cited are derivative of the socialization already shown for participants in Head Start. As such, Head Start deserves applause, but there are no data in the book that relate to cognitive gains by the children other than intimations that some such gains must have occurred. The authors cite (p 131) a Head Start Synthesis Project as containing 8 conclusions including (1) "that cognitive test score gains do not appear to persist over the long term; (2) Based on 'very few studies' Head Start appears to affect...being retained in grade or assigned to special education classes; (5) no significant differences in the health practices at home between Head Start and non-Head Start parents." The other five conclusions are equally without compelling information. The flimsy evidence for the initial approbation of Head Start will be detailed below. The criticism of the Synthesis Project by Gamble and Zigler (1989) focuses entirely on the methodology and does not offer evidence that the conclusions are wrong.

To show why I initially concluded that there was little cause for inferring any significant successes for Head Start, I give below the data for the program I chanced to choose to study: the work of the group headed by David Weikart of Ypsilanti, Michigan. This turned out to be an exceptionally fortunate choice because this is by far the best organized program; it has carefully followed up the experimental children out to age 27 years (in 1994) so that their abundant data are available for study. The initial claims were stated directly in their report (Weikart et al, 1970) entitled "Longitudinal Results of the Ypsilanti Perry Preschool Project." Its name was later changed to the High/Scope Project. I quote from the report's page 2.

"1. Children who participated in preschool obtained significantly higher scores on measures of cognitive ability than control group children. As both groups progressed through school this superior functioning disappeared by third grade.

2. Children who participated in preschool obtained significantly higher scores on achievement tests in elementary school than control group children. This significant difference continued throughout the years of follow-up, including third grade.

3. Children who participated in preschool received better ratings by elementary school teachers in academic, emotional, and social development than control group children. This difference continued throughout the follow-up years, including third grade.

The conclusion of the study is that preschool programming, at least as represented in this project, is an effective device for improving the general functioning level of disadvantaged black children who were initially diagnosed as functionally mentally retarded."

This conclusion is obviously based entirely on he third point. Note, first, that point 1 concedes that on measures of cognitive ability the initial differences vanished by 3rd grade. The data are given in the report to which I have referred. I have collected the results from the large number of tables in the back of the report, and the following table gives the essential information on which I base this analysis of the claims.

RESULTS OF THE PERRY PRESCHOOL PROJECT: A TWO-YEAR INTERVENTION
F-RATIOS ON VARIOUS TESTS

GRADE WHEN TESTED				SKG	S1G	S2G	S3G
1. Stanford-Binet Intelligence Scale				4.6	(8.3)	<1	<1
2. Leiter International Performance Scale				1.3	<1	<1	3.9
3. Peabody Picture Vocabulary				4.3	4.3	<1	<1
4. ITPA Total Language Age				<1	2.3	<1	<1
5. ITPA Auditory-Vocal Assn.				(8.3)	(9.6)	3.4	<1
6. California Achievement Test				---	4.3	2.9	(11.6)
7. PBI Classroom Conduct				<1	5.6	<1	2.5
8. PBI Academic Motivation				1.5	4.2	3.1	1.7
9. PBI Socio-Emotional State				<1	6.9	3.2	1.4
10.PBI Teacher Dependence				2.3	1.9	<1	<1
11.PBI Personal Behavior				<1	(8.9)	<1	<1
12.YRS Academic Potential				2.0	2.6	4.2	<1
13.YRS Mother Participation				<1	1.2	<1	<1
14.YRS Social Development				<1	4.0	6.8	<1
15.YRS Verbal Skill				<1	1.1	(9.8)	<1
16.YRS Emotional Adjustment				<1	(9.9)	(8.5)	4.1
	...AGE...						
4 WAVES TOTALED	3	4	k				
No of girls	25	17	19	20	16	8	5
No of boys	33	20	25	25	17	13	8

Underlined F-ratio numbers are significant at the 0.05 level. Numbers in parentheses are significant at the 0.01 level.

The numbers are given for the spring of the kindergarten, first, second, and third grades, and are expressed as F-values

which tell us how much variation there is between the average value for experimental children and that for the controls. F-values lower than about 4 are not significant by statistical criteria; an F-value of 4 or more could happen by chance alone perhaps once in 20 times, and a value of about 9 or more could happen by chance alone perhaps once in 100 times. A 1 in 20 value is said to be significant; 1 in 100 is said to be very significant. Thus, values below 4 indicate that no statistically significant improvement was found. Values lower than 1 could be, although it is necessary to examine the data more closely to verify the case, instances of control children doing better than experimental children. Notice that, of the 16 measurements in grade 3, there are 10 such instances of values lower than 1!

A look at all the numbers reveals 3 significant differences in kindergarten, 10 in first grade, 3 in the second grade, and 2 in the third grade. In only two instances, (social development and emotional adjustment) are there significant differences for any two consecutive years.

The 3 significant differences in kindergarten (after the end of the intervention) are in scholastic kinds of measures, while of the few differences in grades 2 and 3 only 1 is scholastic.

Because the program is aimed at <u>lasting</u> improvement in scholastic performance, it is extremely important to look carefully at the results in the spring of 3rd grade (S3G). It is readily seen that there is a significant difference in only two of the numbers: that for the California Achievement Test and emotional adjustment. Since there are 16 measures, this means that on 14 there are no significant differences. Something positive can happen so frequently by chance alone twice in sixteen measures that I looked for randomness in performance on that California Achievement Test. If it is really random, then in the second grade there will be a very different story and, indeed, there is not a significant difference in the second grade. Moreover, the report conceded in its first conclusion that cognitive increases disappear by third grade.

The next point to be considered uses the data from the so-called Consortium report which studied a number of the first Head Start programs. It pointed out that on two measures there were potentially interesting differences: children kept back a grade and children moved to special education classes. The significance of the differences was computed directly as a probability value: the probability that the observed difference could have happened by chance alone. (I remind the reader that only probabilities lower than 1/20 or 0.05 are considered significant). The data given in the Consortium report are gathered in the following table, in which there are two separate groups (A and B) for reasons to be explained later.

GROUP A				GROUP B			
PROGRAM	SPEC ED	GRADE REPEAT	BOTH	PROGRAM	SPEC ED	GRADE REPEAT	BOTH
B	-0.6	0.5	0.7	GO	0.02	0.9	0.13
GR	0.004	0.3	0.19	L	0.005	0.5	0.04
M	-0.5	-0.25	-0.35	P	-	0.006	0.006
Z	-	0.45	0.45				
W	0.06	0.065	0.01				

Of the 8 programs evaluated on these counts, in group A there is only one program (GR) in which a significant value (<0.05) was found for either criterion separately. In group B, all three programs were significant on at least one of the two criteria. Thus, totally, there are only 4 significant values, and in no instance was a program of significant help on both counts.

There is no way from the published data I have seen to learn how these two factors are managed in the individual school systems. Perhaps, to be a devil's advocate, children who don't "make trouble" in class are not kept back nor sent to special education classes. Even if we accept the data at face value, however, finding

4 successful probabilities out of 16 possibilities doesn't seem to be worthy of much interest overall, other than to give guidance to the next versions of the programs. Note, moreover, that there are 3 cases in which negative values are given. This means that in those programs the control children did better than the experimental children! So, overall, there are essentially equal numbers of failures and successes.

But, look at the column for BOTH. In this column are the calculations of Lazar and associates for the combination of factors (special education classes or repeat grade). Here it can be seen that two programs (W and L) produce statistically significant results, while a third (P) cannot be evaluated because data were obtained only for one of the factors. Consider program W. In neither individual factor is a significant result obtained, but for the combined factors a positive result is obtained. What is the meaning of evaluation of the combined factors? What it gives is the probability that a significant effect would be obtained in one or the other, and the fact that program W almost reaches significance lets the statistician say that it is likely that such a program would produce a significant result because it essentially adds two proportions while not changing the standard deviations very much, so it increases the probabilities of not having a result this large by chance alone.

If any program were to be designated as the one to be sponsored by the government, it would seem to be L's. But, that is not what happened.

The data actually presented by Lazar et al include those from Z (Zigler) and P (Palmer) who had no special education data; those from Palmer are highly significant because his single datum was highly significant, while those from Zigler remain non-significant because there is no way to know how many children would have been put in special education classes if available.

My interpretation is implicit in the separation of the Consortium Report's data into two groups. Group B programs started with children under 3 years of age and group A with children older

than about 3 1/2 years. Note that almost all of the successes listed in that report were in group B. Even the one success ascribed to W refers to children who were in the 3-year category. Others, such as J.M. Hunt -one of the gurus of early intervention programs- have already pointed out that programs starting by age 2 years have an enormously greater likelihood of succeeding to produce something pragmatically useful towards the goal we all desire: the significant improvement of the educational performance of children from poverty backgrounds.

There are substantial reasons of a biological nature for suspecting that this change of starting age would produce much greater success. Our obvious guess, then, about when to try to increase the effectiveness of helping the pre-school children would be during the 2-4 year rapid brain growth period using what is otherwise called day-care. There are some data from the so-called Milwaukee Project run by Heber for some 9 years starting with children not long after birth. His data show clear and seemingly permanent rises in average IQ of the experimental children (Heber, 1975). Similar positive effects of home-based intervention starting at age 20 months were found at age 3 years by Karnes et al, (1970) though the permanence of the effects was not evaluated.

You have probably guessed that the W program is Weikart's High/Scope program. Note again that <u>it produced no significant effect on either criterion separately</u>; only taking them together yielded significance. We know, however, that this later effect is measured in cognitive terms by the sleeper effect which revealed to us that the High/Scope program reduced the backwardness of children from being 4 grades behind norms to being 3 grades behind.

It is on the basis of these <u>flimsy</u> initial results that High/Scope has become the longest-running Head Start cognitive program, the one for which most data are available, and apparently the main model for subsequent national Head Start programs.

Finally, the meaning of significant differences needs to be examined more closely as in the report "Found: Long-Term Gains From Early Intervention" (Brown, 1978). It says in its preface "The

data are impressive. But early interventions do not ensure that the children will perform as well as the average child in the population. The benefits of early intervention over non-intervention are striking, but comparison of the children in early intervention programs with grade norms for 4th, 5th, and 6th Grades is not impressive. The children are behind the average child in the population." Thus, the title, while accurate, is misleading about the eventual extent of success of the programs. Such findings confirm the inference that Head Start and similar programs are not effective enough scholastically to be left unchanged as the major focus of the remedial pre-school programs.

It is true that there are some lasting effects, mainly of a non-significant variety, some positive, and some negative. What this should indicate is that the significance-producing programs should be looked at again with some care, and the negative programs should also be looked at with care in an attempt to learn what is counter-productive.

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